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1. *Rules and Bye-Laws of the Manchester Medico-Ethical Association.*—1848. 8vo, pp. 12.
2. *On the Nature of the Scholar and its Manifestations.* By JOHANN GOTTLIEB FICHTE. Translated from the German by WM. SMITH.—1847. 8vo, pp. 220.
3. *Claims of the Missionary Enterprise on the Medical Profession: an Address, &c.* By D. J. MACGOWAN, M.D., Medical Missionary at Ningpo, in connexion with the American Baptist Board of Foreign Missions.—1847. 12mo, pp. 52.
4. *The Pre-Adamite Earth: Contributions to Theological Science.* By JOHN HARRIS, D.D.—*London,* 1848. 8vo, pp. 367.

THE long interval of almost uninterrupted peace and tranquillity which Europe has enjoyed, and which is now disturbed not so much by international quarrels as by intestine commotion, has left the minds of men at liberty from the engrossing thoughts and efforts which war demands; and the result has shown itself,—and, we trust, in spite of a temporary check, will continue to show itself,—in the advance of all the arts and sciences. The sciences of morals and of mental philosophy have received their share of consideration; and consequently we find metaphysical theology, religious doctrines, formulæ and ceremonies, and ecclesiastical government and discipline, occupying the thoughts of a large and influential portion of the community. The medical profession is not exempt from the general movement; and, while we have to note with pleasure amounting to delight the wondrous progress that the medical sciences have made during the last thirty years, we cannot shut our eyes to the fact that the moral or ethical relations of the profession have not been neglected. Of recent years each European language has afforded at least one contribution to medical deon-

tology and medical ethics; and if, in our own country, the formal theory has attracted little attention, the practice of the great body of the profession has been in full accordance with its soundest teachings. No class of men—not even the clerical order—has exerted itself more disinterestedly and benevolently for the welfare of mankind; at home and abroad, we find the severest toil, the most dangerous duties, undergone with a cheerfulness and alacrity which can only result from a deep inherent sense of the claims of suffering humanity upon medical skill; and besides those whose official duties carry them to the remotest parts of that empire on which the sun never sets, numerous individual members of our body have been enlisted into the service of Christian missions, and Asia, from the coasts of Syria to the rivers of China, has felt the presence of practitioners, who carry succour for the souls of men in their right hand, and in their left help for their ailing bodies.

The little tract entitled 'Claims of the Missionary Enterprise on the Medical Profession,' by Dr. Macgowan, an American medical missionary at Ningpo, contains facts and arguments of interest to every practitioner who looks beyond this earth, and the pains and sorrows he has to alleviate; nay, of deep and surpassing interest, because if truly Christian, he must see that the medical missionary is therein more closely assimilated to the founder of his holy religion than any other. "Of the physician it is the high and honorable boast that with him science is merely the necessary means to an important end—that all his knowledge is eminently practical, and its great purpose benevolent. It is his province to assuage human suffering in all its varieties and aggravations, and, in imitation of the Saviour, 'to heal all manner of diseases.'" When, however, he passes the boundaries of European civilization (and we, of course, include the civilized states of the New World within them), his labours become far more valuable and far more effective. In highly-civilized nations medical science has interpenetrated the social condition of the people; and much of the prophylactic and practical knowledge of the profession is in daily application without the assistance of individual practitioners. The abounding numbers, too, of skillful hands and well-stored heads, lead to a lower estimate of the value of a skilled practitioner. He is no longer considered as "more than armies to the country's weal." But if we turn our eyes to half-civilized nations or to barbarous tribes, and mark the treatment of the sick and the ravages of disease amongst them, the glory of the medical profession—its power to save from misery and death—stands forth in brilliant clearness. "Behold Dr. Grant," says Dr. Macgowan, "armed only with his needle for the removal of cataract, forcing mountain passes, and, amidst ferocious warriors, winning his way to their homes and their hearts. On account of his professional skill he was enabled to traverse in safety regions heretofore untrodden by civilized man, and in whose defiles an army would perish in effecting an entrance." The destruction from epidemical diseases is frightfully appalling, especially from smallpox; and the sad traces of syphilis apparent amongst distant tribes, since they have become familiarized with Europeans, show that medical aid to the sufferers is called for alike as an act of moral duty and of Christian love. The treatment of the sick in many countries is truly cruel, either from ignorance, superstition, or apathy. The Brahmin priest chokes the sick Hindoo with mud from the Ganges, and the weak, the aged, and the dying are left ex-

posed on its banks to the glare of a burning sun, or are held up in the river and its sacred water poured down their throats until they expire. Immense numbers of blind, we are told by Dr. Macgowan, are seen in the streets of Chinese cities, and their blindness is frequently the result of a simple ophthalmia, easily cured by suitable remedies. Dr. Bradley, who is stationed at Bankok, in Siam, states that the relatives and friends of many who were literally all corruption, "helpless and hopeless," brought them to his door and then forsook them. His abode was almost constantly the scene of the groaning, the dying, and the dead.

Medical missionaries have not gone forth from Europe and the United States as missionaries of medical art and science exclusively, or even principally, but rather with the intent of rendering medical science and art subservient to the propagation of Christianity. Dr. Macgowan enumerates five American missionary societies, five British, and one French, as sending out thirty-five medical missionaries; and this list we happen to know is imperfect, as both the Church of England and the Wesleyan body have professional men thus engaged. They are to be found in Africa, India, China, Syria, and the Islands of the Pacific. The object of the persons by whom Dr. Macgowan's address has been reprinted and circulated—the Edinburgh Medical Missionary Society—is to stir up the members of the medical profession in Great Britain to consider their own duties and responsibilities in connexion with the great object of Christian missions to the Heathen; and, in fact, to raise up a new order of missionaries, who shall combine, as of old, in their own persons the sacred functions of teaching religious truth and curing bodily infirmities. The Scotch editor of Dr. Macgowan's address, in a note attached thereto, does not hesitate to call upon "practitioners of some experience," and "not very young men," to go forth and give their personal services; and adduces circumstances and considerations fitted and intended to warn us, that some or even many amongst us may one day be constrained personally to consider and answer the question, "Am I fitted, and if fitted, am I willing and ready to obey the call of my Divine Master to become a fellow-helper to the truth, in devoting my professional skill and personal exertions to the promotion of the spread of the Gospel?"

The facts and views we have just detailed are sufficient of themselves to show that medical deontology and medical ethics have risen from their cradle, and present to the physician a much wider field of thought and moral action than has hitherto been assigned to him. We think, however, that a due consideration of the true position and duties of the physician demands a more comprehensive tone and method than these exhibit. The physician's proper study is MAN in every possible relation. He has to study man as a spiritual being, and as a mere animal; as a moral creature, and as a piece of vital machinery; as in the "image of God," and as an unreasoning brute. In considering him as a spiritual and moral being, the physician, in common with the philosopher, trenches upon theology and moral philosophy, and is bound to study all questions thence arising in connexion with the structure, functions, and disorders of the brain and nervous system. It cannot be matter of surprise that with such a wide scope of inquiry, and a scope which it is imperative that the physician should occupy, the physician has come to conclusions not always in accordance with the principles of dogmatic theology, or of the popular

code of morals; that he has been lenient in his judgments, slow to punish, ready to plead human infirmity in excuse for crime, thrown the shield of professional opinion over the thief and murderer; looked not for uniformity of faith and practice, pleaded for toleration, has been latitudinarian in his principles, and, in short, has been pronounced a simpleton, a fool, a protector of felons, a heretic, a materialist, an irreligious person, an atheist. An irreproachable life, and gratitude for services rendered in the hour of need, may have often shielded the practitioner from persecution by the religious zealot, but oftener his discretion and caution have stood him in better stead. Feeling the impossibility of convincing, he has kept a watchful silence, or given a verbal assent to dogmas and doctrines which he could not comprehend, or which he suspected to be groundless.

Much of the infidelity of medical practitioners is due to the ill-directed zeal, the ignorance, and the presumption of certain teachers of theology. Very few of those who have addressed themselves to the task of combating their scepticism have placed themselves in the position of the heretic, and fairly met the arguments which determined his belief. Entirely unacquainted with physiology, and, in some instances, even with natural philosophy, they have trod in the beaten track of controversialists, from a period when modern science scarcely existed, and Christian truth was inculcated rather by the terrors of a brutal superstition than by the irresistible allurements of moral suasion. And although the sceptical practitioner might not be ruthlessly doomed to the flames of hell or of purgatory by his theological foe, or made to swear, on penalty of exclusion from practice, that he will defend the doctrine of the immaculate conception of the Virgin Mary; yet the ban of society has often been fixed upon him as a "materialist," and that man has been shunned by good and pious, but weak-minded persons, who, to all the graces of the Christian character, and all the morality of the Christian life, added a firm and unalterable adherence to what, after long thought and mature deliberation, he had determined to be TRUE.

There are many reasons, however, why the investigator, and expounder, and applier of the science of human nature should no longer shrink from his duty as a seeker and teacher of moral truth. The sciences of which the medical profession has been the foster-father, and even originator, are becoming more and more popularised; entering the popular mind, they will produce all the evil effects of truths imperfectly comprehended; and a large and influential class will soon be formed, who will secretly disbelieve in the dogmas and even doctrines of Christianity. And the denunciations of the ministers of religion will never touch them, if, as with the medical profession, the beaten path of controversy be not departed from, or if the great truths of the Christian religion be not harmonized with the great truths of natural religion and moral philosophy. Some few clerical and lay writers on theology and morals have, it is true, addressed themselves to this important task, either directly or indirectly; we shall shortly allude specially to Fichte's philosophy, and to Dr. Harris's 'Pre-Adamite Earth;' for although the latter is addressed mainly to meet the superficial scepticism which a shallow consideration of the facts of geology might engender, it is a book eminently suitable to the scientific practitioner, and will enlighten his understanding as well as confirm his faith.

Writers on medical ethics and deontology too hastily and crudely assume that all medical practitioners are Christians. It is certain that by far the greater proportion are nominally such; but of this, how many are Christians from conviction, and how many, under a profession of zeal, conceal a rooted scepticism and even dislike of Christian doctrines and discipline? We allude, of course, to the medical profession in every part of Christendom, whether Greek, Roman, or Protestant. There is a small portion of the profession which is not even nominally Christian, and this portion will increase both amongst the Pagans of India and China, and the Mahometans of Turkey, Africa, and Syria. If we leave out of consideration altogether the conflicting doctrines of Christian sects, it is evident from this circumstance only—the formation of a medical profession in the East—that a system of medical deontology or ethics must have a more extended basis than that which Christian writers usually supply. If that basis be fixed upon the acknowledged principles of philosophical inquiry and research, and be proved to be not only consistent but connate with the great Christian verities, we apprehend that medical philosophy may thus in Pagan countries be made the herald of Christian civilization, in a way the promoters of medical missions at home have not yet dreamed of, and may at the same time serve to check the scepticism of its own half-educated members, and of the popular mind, more effectually, because more convincingly, than the whole phalanx of exclusively theological arguments and mediæval sophisms.

But it may be advanced with perfect propriety, that the science of human nature, of which the medical profession are the recognised cultivators and appliers, has a mighty future mission to fulfil towards humanity. From the earliest periods of Christianity a second advent of Christ has been expected, and therewith a more perfect condition of morals. For such a great and glorious event, the daily and hourly prayers of Christendom are offered up in the words taught by Christ himself—"Thy kingdom come, Thy will be done in earth as it is in Heaven." How such events will be brought about, we are not taught; but if we seek for information in experience, and judge the future by the past, they will arise with fundamental changes in existing forms and ceremonies, and with a clearer and more perfect manifestation of God's infinite wisdom and goodness. Science, and especially the science of human nature, may be expected to have a large share in the agencies that will effect this. As the Mosaic ritual excelled the patriarchal, and as Christian doctrines surpassed and abrogated the Mosaic, so may the second advent be expected to eclipse the first. It cannot be denied that the principles and ceremonies of the Christian religion are even now being closely sifted and weighed in the balance of a severe philosophy; and while all that is essential and fundamental is rendered more sure and credible, that which is non-essential and accessory is being discarded. Such changes cannot and will not take place without much antagonism and much strife; but the stigma of scepticism can never be truly and fairly affixed to the study of human nature, as practised and taught by the enlightened members of the medical profession. If an undevout astronomer be mad, how much more mad the educated and instructed but undevout practitioner, to whom God's handiwork is revealed, and the operations of infinite wisdom laid open in the living creation, and especially in man, the image of God? This imputation

of infidelity was strongly repelled by Dr. Gregory, who observed in his Lectures:

"Medicine, of all professions, should be the least suspected of leading to impiety. An intimate acquaintance with the works of Nature elevates the mind to the most sublime conceptions of the Supreme Being; and at the same time dilates the heart with the most pleasing prospects of providence. The difficulties that must necessarily attend all deep inquiries into a subject so disproportionate to the human faculties, should not be expected to surprise a physician who, in his daily practice, is involved in perplexity and darkness, even in subjects exposed to the examination of his senses."

It is, we think, true in some small degree, nevertheless, that the members of the medical profession generally are sceptical as to the truths of Christianity, especially in Roman Catholic countries, where freedom of thought and of discussion is restrained. But this restraint is the true cause of the scepticism, and not professional studies. Let any mind honestly seek truth, asking for the guidance of God's Holy Spirit, and we have every reason to believe that it will find both; but if that mind be already illuminated by natural religion, and by a knowledge of the wisdom and goodness of God in creation, are we to conclude that it will, just in proportion as it is illuminated, go astray from the truth? Is it not a wavering and unworthy faith which holds as nothing the daily revelations of physical truths by God to mankind, and sets up a dull, sapless ignorance as the best qualification to an attainment of the highest, most perfect, most glorious knowledge?

But we would not dare to advise the profession thus boldly to assert their prerogative, and, casting aside the trammels of sectarian theology, seek to link philosophy with Christianity, if experience had not proved the value and practicability of such a step. We would not thus bid our brethren to dare to think, did we not feel assured that such bright and burning thoughts would result, as would render Christian truths more brilliant and more priceless; lead on the human race to social progress and a holier civilization; and quench the fetid smoulderings of real scepticism, the disbelief in a moral governor of the universe, and a moral and spiritual world. After the practitioner has thus dared to think, he will come to the conclusion of Johann Gotlieb Fichte, and say:

"The whole material world, with all its adaptations and ends, and in particular the life of man in this world, are by no means in themselves, and in deed and truth, that which they seem to be to the uncultivated and natural sense of man; but there is something higher, which lies concealed behind all natural appearance. This concealed foundation of all appearances may, in its greatest universality, be aptly named the *Divine Idea.*" (p. 124.)

The work of Fichte, on 'The Nature of the Scholar,' from a translation of which, by J. Smith, we have taken the preceding quotation, is one which must inevitably arrest the attention of the scientific physician, by the grand spirituality of its doctrines and the pure morality it teaches.

It consists of a course of public lectures, which was announced on the roll by the Professor of Erlangen, under the title of "De Moribus Eruditorum." Generally speaking, morality means a direction of character and conduct according to rule and precept. But Fichte demurs to this idea; and considers it true in only a limited sense. Whatever is to be manifested in the thoughts or acts of man, must first be in his nature, and indeed

must constitute it. That which lies in the essential nature of man, must necessarily reveal itself in his outward life, shine forth in all his thoughts, desires, and acts, and become his unvarying and unalterable character. This nature, as manifest in the scholar, it is Fichte's task to describe; and after expanding the definition of the "Divine Idea," which we have just quoted, he propounds the doctrine that a certain part of the meaning of this Divine Idea is accessible to every cultivated mind, and conceivable by it; that those who attain to the attainable portion are possessed by a higher and more spiritual life; and that "in every age, that kind of education and spiritual culture, by means of which the age hopes to lead mankind to the knowledge of the ascertained part of the Divine Idea, is the learned culture of the age; and every man who partakes of this culture is the scholar of the age." There may be the apparent scholar and the true scholar; the former has gone through a course of learned education; the other, through the learned culture of the age, has arrived at a knowledge of the "Idea."

"In individual human beings the eternal Divine Idea takes up its abode, as their spiritual nature; this existence of the Divine Idea in them encircles itself with unspeakable love; and then we say, adapting our language to common appearance, this man loves the Idea, and lives in the Idea,—when in truth it is the idea itself which, in his stead and in his person, lives and loves itself; and his person is only the sensible manifestation of this existence of the Idea. . . . . In the true Scholar the Idea has acquired a personal existence, which has entirely superseded his own, and absorbed it in itself. He loves the Idea, not before all else,—for he loves nothing beside it—he loves it alone;—it alone is the source of all his joys, of all his pleasures; it alone is the spring of all his thoughts, efforts, and deeds; for it alone does he live, and without it life would be to him tasteless and odious." (p. 129.)

We might multiply quotations of this kind, were it necessary, as well from other works of Fichte, as from this before us, but one or two more must suffice:

"The true-minded Scholar will not admit of any life and activity within him, except the immediate life and activity of the Divine Idea. This unchangeable principle pervades and determines all his inward thoughts and outward actions. With respect to the first—as he suffers no emotion within him that is not the direct emotion and life of the Divine Idea which has taken possession of him, so is his whole life accompanied by the indestructible consciousness that it is one with the Divine Life—that in him, and by him, God's work shall be achieved, and his will accomplished; he therefore reposes on that will with unspeakable love, and with the immovable conviction that it is right and good. Thus does his vision become holy, enlightened, and religious; blessedness arises within him,—and in it, changeless joy, and peace, and power,—in the same way as these may be acquired and enjoyed by the unlearned, and even the lowliest among men, through true devotion to God, and honest performance of duty viewed as the will of God." (p. 191.)

And again:

"Whatever man may do, so long as he does it for himself, as a finite being, by himself, and through his own counsel—it is vain and will sink to nothing. Only when a foreign power takes possession of him, and urges him forward, and lives within him, in room of his own energy, does true and real existence first take up its abode in his life. This foreign power is ever the power of God. To look up to it for counsel—implicitly to follow its guidance,—is the only true wisdom in every employment of human life, and therefore most of all in the highest occupation of which man can partake—the vocation of the true Scholar." (p. 192.)

Shall we be presumptuous if we recommend these views to our professional brethren? or if we say to the enlightened, the thoughtful, the serious, this—if you be true scholars—is *your* vocation? We know not a higher morality than this, or more noble principles than these: they are full of TRUTH.

In his third Lecture, Fichte treats of the progressive scholar, and in particular of genius and industry. He points out the nature and bearing of the two qualities, and the characteristics of those in whom they are deficient. Whenever a man, after having availed himself of existing means of mental improvement, remains inactive, satisfied with his acquirements, and proud of his powers, then he has neither the "Idea" nor "Genius," but only a vain ostentatious disposition, which assumes a singular and fantastic costume in order to attract attention. Fichte thus compares this man of Dutch metal with one of sterling ingot.

"Such a disposition shows itself at once, in self-gratulatory contemplation of its own parts and endowments, dwelling on these in complacent indolence, commonly accompanied by contemptuous disparagement of the personal qualities and gifts of others; while, on the contrary, he who is restlessly urged on by the Idea, has no time to think of his own person;—lost with all his powers in the object he has in view, he never weighs his own capacities of grasping it against those of others. Genius, where it is present, sees its object only,—never sees itself;—as the sound eye fixes itself upon something beyond it, but never looks round upon its own brightness. In such a one (one who contemplates his own brightness) the Idea does certainly not abide. What is it, then, that animates him—that moves him to those eager and active exertions which we behold? Is it intense pride and self-conceit, and the desperate purpose, in spite of Nature, to assume a character which does not belong to him?—these animate, impel, and spur him on, and stand to him in the room of genius." (p. 150.)

Such is Fichte's description of that man in whom study and science have worked but imperfectly; parallels we fear are to be found too numerously amongst the members of the medical profession, although most assuredly not exclusively there. Self-contemplation, self-admiration, and self-flattery, though the last may remain unexpressed, and even carefully concealed from every observer, these, with indolence, and disdain of the treasures already gathered into the garners of science, are signs of the absence of the genius common to every profession. But as the medical profession has a wider sphere of scientific culture, and a deeper insight into the greatest of knowledges,—the knowledge of human nature,—by so much should he be free from these marks of imperfect cultivation of the moral powers. It may happen that there is a natural tendency in the mental constitution of individuals to think more highly or more lowly of themselves than they ought to think; but even with these, true scientific or learned culture will have much weight. To those we would say with our philosopher: No one need pride himself upon genius, for it is the free gift of God; but of honest industry, and true devotion to his destiny, any man may well be proud; indeed this thorough integrity of purpose is itself the Divine Idea in its most common form, and no really honest mind is without communion with God.

In the fourth Lecture, on Integrity in Study, the same high tone of morals is maintained.

"The honest scholar," observed Fichte, "is to us the only true scholar."..."If knowledge appears to those who want both genius and integrity, only as a means

to the attainment of certain worldly ends, she reveals herself to him who, with honest heart, consecrates himself to her service, not only in her highest branches, which touch closely upon things divine, but down even to her meanest elements, as something originating in, and determined by, the eternal thought of God himself." (p. 161.)

Fichte thus inculcates on the student integrity and trust in God:

"Whatever thou doest, do it with integrity; if thou studiest, let it guide thy studies; and then, as to whether thou shalt prosper in what thou doest, leave that to God; thou hast most surely left it to him when thou goest to work with true and honest purpose;—with the attainment of that integrity thou wilt also attain unbroken peace, inward cheerfulness, and an unstained conscience; and in so far thou wilt assuredly prosper." (p. 156.)

Nor is this a mere scintillation amidst the gloom of a sterile and comfortless philosophy. "Man shall BE and DO something," he adds; "his temporal life shall leave behind it in the spiritual world an imperishable result." And again, discoursing on integrity, he says, in reference to this being and doing:

"He in whom this integrity has become a living idea cannot conceive of human life in other way than this; from this principle he sets forth, to it he constantly returns, and by it he regulates all his other modes of thought. Only in so far as he obeys this law, and fulfils this purpose, which he recognises as his being's end and aim, is he satisfied with himself; everything in him which is not directed to this high end—which is not evidently a means to its attainment, he despises, hates, desires to have swept away. He looks upon his individual person as a thought of the Deity; and thus his vocation—the design of his being—is to him as a purpose of God himself." (p. 156.)

It is to the physician that the Divine image in man becomes a grand reality; and his professional studies fully carried out to their results, lead him to say with Fichte—

"Man is not placed in the world of sense alone, but the essential root of his being is in God. Hurried along by sense and its impulses, the consciousness of this life in God may be readily hidden from him, and then, however noble may be his nature, he lives in strife and disunion with himself, in discord and unhappiness, without true dignity and enjoyment of life. But when the consciousness of the true source of his existence first rises upon him, and he joyfully resigns himself to it till his being is steeped in the thought, then peace and joy and blessedness flow in upon his soul. And it lies in the Divine Idea that all men must come to this gladdening consciousness—that the outward and tasteless Finite Life may be tasted by the Infinite and so enjoyed." (p. 142.)

As to the mode in which this integrity shows itself, we have it discussed in a distinct lecture. In the first place, the holiness and grandeur of his vocation will be impressed on his mind and determine the acts of the true scholar. He enters upon his vocation in consequence of the conviction that it is the purpose of God in him and for him; it is his providential course, fixed by God, and therefore holy and great. And thus his person as well as his vocation, become to him before all things, honorable and holy.

"This thought, with its indestructible certainty, enters and fills the soul of every honest student;—this, namely,—'I, this real, this expressly commissioned individual, as I may now call myself, am actually here—have entered into existence for this cause and no other—that the eternal counsel of God in this universe may, through me, be seen of men in another, hitherto unknown, light,—be made clearly manifest, and shine forth with inextinguishable lustre over the world; and this phase of the

Divine Thought, thus bound up with my person, is the only true living being within me; all else, though looked upon even by myself as belonging to my being, is dream, shadow, nothing; this alone is imperishable and eternal within me.'" (p. 159.)

Thus impressed, he shuns the contact of the vulgar and the ignoble; he is no boasting depreciator of his brethren; no vulgar quack; no base raker-up of riches; no pilferer of the merits or reputation of his compeers. He shuns everything which weakens spiritual power;—idleness, drunkenness, sensuality, pride, self-contemplation.

"Lastly, everything is vulgar and ignoble which robs man of respect for himself, of faith in himself, and of the power of reckoning with confidence upon himself and his purposes. Nothing is more destructive of character than for a man to lose all faith in his own resolutions, because he has so often determined, and again determined, to do that which nevertheless he has never done...........Not so the upright student; he keeps his purpose, and whatever he has resolved to do, that he does, were it only because he has resolved to do it. For the same reason,—that he must be guided by his own purpose and his own insight,—he will not become a slave to the opinion of others, or even to the general opinion. It is doubtless of all things most ignoble, when man, out of too great complacency, which at bottom is cowardice and want of spirit, or out of indolence—which prevents him thinking for himself, and drawing the principles of his conduct from his own mind—gives himself up to others, and relies upon them rather than upon himself." (p. 171.)

There are other lectures "On an Academical Freedom," "On the Finished Scholar," "On the Scholar as Ruler," "On the Scholar as Teacher." We may have an opportunity of noticing more of the views therein contained, before we part from our readers.

The truly spiritual Christian cannot fail to recognise in the doctrines and precepts we have quoted, some fundamental Christian verities; and not the less Christian because translated from modern German instead of ancient Greek. The first of Christian philosophers, St. Paul, anticipated Fichte, when he wrote, "Man is the image and glory of God;" and throughout his active life and immortal correspondence, the hero and the martyr blend. The man who can only recognise divine truths in the conventional phrases derived from the ancient words of revelations, believes without having faith; we would not condemn such an one, but we would question his fitness to be a judge of what is orthodox. We would add here that, while the absence of all reference to Christian truths, as such, would render such moral philosophy as that of Fichte more acceptable to philosophical Pagans and infidels, by not wounding their prejudices or their pride, it cannot prevent the Christian man rejoicing that philosophy is thus made the handmaid and promoter of Christian truth, nor hinder the Christian philosopher from recognising with deeply solemn and grateful feelings a continued revelation of truth to man.

But if metaphysics and moral philosophy thus lead the earnest inquirer to a knowledge of his God, of himself, and his duties, how much more attractive and convincing must be that philosophical theology which grapples with science, and firmly binds it to revelation? Hitherto scientific truths have had little consideration in theological discussions; a dogmatic theology has fostered a dogmatic system of metaphysics, and the fierce controversialists of all sects have only suspended their bickerings to growl an anathema maranatha against natural philosophy. Such conduct could only injure the cause of true religion; the conscientious judgment and

enlightened understanding instinctively avoided the fellowship of such defenders of the faith, and therewith, it is to be feared, religion itself. To all men of this class, Dr. Harris's book must be welcome, whether as reconciling natural philosophy with Christian truth, or as an elegantly written and profoundly philosophical work. Yet Dr. Harris himself cannot enter upon his subject without an apology for his undertaking.

"Of the connexion," he observes, "between theology and natural science generally, it may be assumed that every one who admits that there is a true theology and a true science of nature, will admit also that there is a sense, whatever it may be, in which the two are related. The mind which elicits and embraces both is one; so that, however distinct the processes by which it arrives at a knowledge of each, and however different the sources and kinds of evidence on which that knowledge rests, both branches evince their inherent unison, in the unity of the knowing mind itself. On this conviction it is that men no sooner begin to think, than they next proceed to examine the laws of thought; if they collect facts, they next inquire for the causes of those facts; and when they have succeeded in developing any of the sciences, they then look for the internal bond of union which makes them all one. And for such a *nexus* they seek under the unquestioned conviction that it exists; for the conviction simply implies that, as reasoning concerning each separate science is possible, so reasoning concerning collective science is possible. Well it had been for theology and philosophy if the bond which unites them had been clearly ascertained, and never dissevered." (Preface, p. vii.)

It is refreshing to the medical mind, honestly inquiring after this connexion, to find a kindred spirit illumining his path from that profession which has so almost invariably demanded the surrender of mental freedom, and peremptorily said, yield or be excommunicate. It is encouraging to meet with a divine who proposes to show a theology in nature which is ultimately one with the theology of the Bible; who attempts to deduce principles and apply them to the successive stages of creation, on the assumption that the whole process of divine manifestation, including nature, is to be viewed in the light of a sublime argument in which God is deductively reasoning from principles to facts, from generals to particulars. It is pleasing to find a theological writer who hesitates not to adorn his page with names celebrated in the history of science. We would particularly refer the reader to the fifth part of Dr. Harris's work, which treats of sentient existence as "the third stage of the divine manifestation," and treats of the power, wisdom, and goodness of God, as manifested in the animal creation, with a comprehensiveness of thought and a clear perception of many obscure facts in animal physiology, that cannot fail to charm the scientific practitioner. A few more such writers will render religious scepticism almost an impossibility with the practitioner.

Having established his faith on broad and comprehensive principles, the medical practitioner can hardly become sectarian; and the question necessarily arises, how shall he comport himself with reference to the various forms and shades of religious belief current among men, and especially among Christians? We apprehend this must be decided by the individual, or rather will be decided for him, according to his education and domesticities. If emancipated from the bias of these, the advice of Fichte is, we think, judicious, and in accordance with the practice of the wisest men, even among Pagan nations:

"The true-minded student will not make himself a slave to common opinion; nevertheless, he will accommodate himself to established customs where they are in themselves indifferent, precisely because he honours himself. The educated youth grows up amid these customs; were he to cast them off, he must of necessity deliberately resolve to do so, and attract notice and attention to himself by his singularities and offences against decorum. How should he, whose time is occupied with weightier matters, find leisure to ponder such a subject?—and is the matter so important, and is there no other way in which he can distinguish himself, that he must take refuge in a petty peculiarity?" (p. 171.)

In idolatrous or infidel countries, many established customs are not indifferent; they are, on the contrary, deep offences against truth and humanity, and as such must be met. With regard to Christian sects, the peculiarities are not so important; but even some of these must be considered in their political results, and the practitioner will then perhaps feel himself called upon to assert the dignity of human nature, and the priceless worth of civil and religious freedom; to fight for the truth in its purity; and feeling that through him God works to reveal and do His will, assume the sacred mantle, and go forth undismayed to his duty.

Nevertheless, the medical philosopher must look with a pitying eye upon the religious strife and persecution so rife among men. Seeing so much diversity of temperament, of prejudices, and of education, arising from causes which must themselves be counteracted or changed before unity of faith or practice can be attained; and knowing that nothing short of a miracle can counteract or modify those causes, he looks upon the persecution of religious sects as totally useless for the object it has in view. The moral agitation may develope truth; but it also developes evil. Dr. Lettsom, in his 'Observations on Religious Persecution,' has put the necessity of a difference of feeling and belief amongst men in a strong light.

But, while the practitioner is tolerant even to latitudinarianism, and allows to every man that freedom of opinion that he claims for himself, as the sacred gift of God, and as the emanation of the godlikeness of human nature, he pities those aberrations in religious principles and practices which throw ridicule on sacred things, and give an air of folly or knavery, or both, to religious professors. To him there is no essential difference between the asceticism of the fanatical Hindoo and the self-inflicted penances of the fanatical Christian. The sainted hermit, dwelling on the top of a pillar, differs in nothing from the sainted faquir, swinging from a pole, with a hook in his back. The tinsel tawdry of imagery is to him equally removed from the true spiritualism of human nature, whether visible in the islands and shores of the South or North Pacific, or in the peninsula of Italy or India. In the vagabond life of the faquir or friar, he sees only a modification of that appetite for restless movement and objectless action, which impels the professional mendicant to endure cold and hunger and the restraint of the lock-up, rather than the toils of honest industry. The delirious ecstasies of pious women he often knows to be none other than anomalous forms of hysteria, founded partly on fraud, partly on vanity, partly on insanity. The convulsive seizures and cries of multitudes in popular assemblies, induced in excitable persons by impassioned apostrophes or terrible denunciations, are not to his experienced eye the solemn manifestations of the Holy Spirit of God, such as he loves to recognise in the pages of inspiration, but only the unmeaning and

neurotic phenomena of an excited brain and nervous system. He hesitates to accuse the sufferers of hypocrisy or blasphemy; he denies that they are inspired by God. He pities their weakness; he maintains the truth.

It may be asked, to what extent should the medical practitioner interfere to propagate the religion he professes. We already find Christian medical missionaries abroad in the earth, and we are led to think that more will be influenced to go. Upon this point we think the free-will and conscience of the individual must decide. What we concede to the Christian, we cannot deny justly to the Hindoo. The liberty that we demand for the Protestant cannot be refused to the Jesuit. The only condition we have to demand is, that the mission be done honorably. But if the medical practitioner seeks, through the weakness and folly of his patients, to insinuate his creed and propagate his religious tenets, he is criminal: such conduct is only tolerable in the bigoted and crafty ecclesiastic, if in him. Many good and well-meaning people would have the practitioner to hold up the terrors of death before the eyes of his confiding patient, and extort from his fears and enfeebled mind, what his sound and perfect judgment refused to concede. They would have the physician to preach a sermon at the bedside, and the apothecary to pray. It is reasonable and a duty to warn a patient of his approaching end, or of his danger, and to hint that an attention to the duties of religion is incumbent upon him, and a good thing. This may be so done that the chance of recovery, slight although it may be, shall not be entirely destroyed, and that the life which the practitioner has intrusted to him to save be not extinguished. Where the practitioner is in attendance on a co-religionist, a closer bond exists, and he may with propriety engage with his patient in those spiritual exercises, to which, during health, they have been both alike accustomed; but still he must remember that the care of the body is his chief concern,—the care of the soul is the duty of another; with him he may co-operate, but his place should not be usurped. Frequently the sectarian practitioner is the least learned and skillful; for the time that he devotes to his religious exercises and public services is necessarily taken from that which ought to be devoted to his studies. A high spirituality is by no means inconsistent with professional eminence; but a minute and slavish adherence to forms, or a usurpation of clerical duties, always is. It is, we think, certain that the path of duty lies to the professional man, in the exercise of his profession with Godlikeness, and with integrity and love. He is the revealed hand of Providence to suffering man; the earthly means whereby God softens the weight of the primal curse. To do this with singleness of purpose, should be his first and greatest duty; for it comprises love towards both God and man, and is the vocation to which God has called him.

The punishment and reformation of criminals will ultimately come within the pale of medical science. Many a wretched man has been hung and quartered, burnt at the stake, broken on the wheel, or racked with merciless cruelty, simply because he had had chronic cerebral inflammation; and many a poor, uneducated creature, abandoned by society to his own way, trained up, from no fault of his own, but from the neglect of his fellow-men, in the practice of every vice, is forced, when comparatively innocent, to consort with hardened mockers at everything good and virtuous; and then, when the necessary results have followed, and he has

injured society, society turns upon him in the sacred name of justice, and with its dread formalities, inflicts, not punishment, but revenge; seeks not to amend and reclaim, but to injure and annoy. Yet sound philosophy and genuine Christianity must and ought to, and we believe will, plead against such blind vengeance and unmeaning, useless cruelty; and will appeal to medical science for the means and the mode whereby mercy and justice may be linked together. If a criminal cannot be reclaimed, he may be restrained. At present, the antagonism between the ermine on the bench and the rags at the bar, is too unnatural to be right; the dignity of human nature is outraged not more in the culprit than in the judge.

The relations of medical ethics to political economy, or, in other words, to political parties and strife, constitute a delicate subject for discussion, and perhaps admit here only of the consideration of general principles. The practitioner may set out from one of two opposing principles, or may adopt the *juste milieu*. If he be of opinion that man is like the beast that perisheth; if he adopt a naked materialism, either in theory or practice, and regard the spiritual nature of man as of little importance or non-existent; if he allow him no freedom of thought and action, as a morally responsible being; if he look upon the mass of mankind as a herd of creatures, for whose merely animal appetites and wants it is sufficient to provide; who have to be subservient to the minds of a chosen few above them; to submit without murmur to their caprices, to receive their benefits as favours, with deep thankfulness, to accept their creeds without inquiry, and believe in their God without a thought; if a medical practitioner sincerely thinks this, let him carry out his views and repress exertion. But if, on the contrary, man appears to him a dignified spiritual being, with God-like powers, moral responsibilities, and freedom of thought and action—a being, whose duty, as well as privilege, it is *to think*; if he believe that, in these respects, all men are equal; that a future state and a greater knowledge of the Divine Mind and its divine creations is the final cause of man's existence, and that his moral position in this world will determine his place of dignity in the next; then is the practitioner bound by the sacred principles of natural and revealed religion to exert himself for the improvement of his species. All those legislative measures and philanthropic efforts which have this object in view, will receive his cordial support. On the question of education, he will decide that it be carried to its utmost limits: for, believing that, in a future world, the spiritual dignity will be in proportion to the spirituality attained in this; that the ways of Providence are in perfect harmony, and that order is heaven's first law; that just in proportion as the mind is evolved here, it will be evolved there; that, as the soul is nearer to the Deity in wisdom and knowledge here, as well as in grace and goodness, in that proportion will it be nearer to the Deity in that future state of existence, the nature of which he can theorize upon but dimly, but of which he knows this much,—that it will be a state of happiness, and that the happiness will be in proportion as the soul knows and loves God, or the Divine Idea,—believing all this, the communication of knowledge will be to him a duty of transcendent importance. This better state of knowledge can only be attained by a training and development of the faculties; these are best to be attained by a knowledge of physiological laws, and therefore the medical

practitioner will not only be anxious that education be pushed to its utmost limits, but that it be conducted on sound scientific principles; for it is not sufficient that the mere organs of mental action be developed, but it is also required that they be developed with a reference to the motives from which they will act, and to the final cause of their action; otherwise, the training, so far from being a good to man, may be an evil, by affording him greater power for the commission of evil and of departure from God.

The movement in favour of a systematic application of the principles of public hygiene to society, acquires greater importance and a grander moral aspect, when it is remembered that its object is to allay moral as well as physical evils. To the intelligent practitioner it is melancholy to witness the ravages of death amongst the poorer classes of the population; to see domestic ties dissolved, poverty induced, widows and orphans struggling with adversity, and a large amount of suffering inflicted, all which might in some degree be prevented or diminished. It is he who sees the arrows of fever, invisible to all else; who helplessly watches their havoc, and who knows whence they come and how they may be stayed. But the physical evils are slight in comparison with the moral: sickness, poverty, and crime, so often linked together by the tongues of men, walk really hand in hand; where there is much poverty and sickness there is much crime; where there is much crime and poverty there is sickness, and therewith a helplessness of degradation, a clouding of the faculties, a dullness of the moral sense, a prostration of the God-like powers of humanity, that leave little to hope for the position of beings in the next world, so degraded, so debased in this.

The relief of the sick poor is a duty which has ever been diligently performed by the conscientious practitioner; and, so long as society leaves the poor partially or wholly uncared for in this respect, that duty ought still to be performed. But it should be performed to fulfil a duty, rather than to display an active benevolence; from an active principle of humanity, rather than to gain applause. Indiscriminately gratuitous relief of the sick poor is like indiscriminate almsgiving; sickness, like hunger, is not accidental, but the common lot of man; and they differ only in this, that the one occurs regularly and at short and certain intervals,—the other is of uncertain recurrence. It is a necessity for which the individual, or society for him, is bound to provide, and not a class of educated men. Relief of poverty from the public purse, as a social right, is one of the characteristics of modern civilization; and this principle should be further extended, to include relief of sickness. Kindness, tenderness, and gentleness should, however, ever accompany the administration of this public relief. The poor man, bowed down by disease, has a large claim upon the sympathy of the practitioner; and perhaps the greater, now that a number of sects prevail. Formerly, when the priest of the parish was the special guardian of the poor, the poor man had in him an influential advocate, and one intimately acquainted with his necessities. Now it is the union surgeon or dispensary physician who is brought exclusively into this intimate relationship, and it is he who has to fight the battle of poverty against the proud man's contumely and the greedy man's avarice.

Although the relief of the sick poor is seldom withheld, it is to be lamented that the officers of public charities often diminish the value of their

services by a want of punctuality in their attendance. Few consider how wearisome it is to the sick man to wait; how valuable is the poor man's time, how much loss and suffering is inflicted, when the medical officer either comes late to his duties, or neglects them altogether.

But, while it is maintained that the burden of relieving effectually the sick poor ought to be borne by the community at large, and not by the medical profession, there are individuals whom the practitioner will be glad to relieve from both poverty and sickness. Multitudes of persons may be found, who, with honest pride, endure hunger and pain rather than suffer what they think to be a degradation, by applying to the dispenser of the public dole. These have seen "better days;" they have been accustomed to give and not to receive, and to them the change is grievous. Such examples of penury are peculiarly the charge of the practitioner; and their relief should be secured to them with the utmost delicacy, and the tenderest regard for their feelings. We have known an instance in which the practitioner was måde the channel of relief from the board of guardians, and a poverty-stricken lady received her weekly stipend until her death, without ever knowing the bitter degradation of feeling herself a pauper. To fallen, educated affluence, the practitioner should be gentle as a ministering angel.

In advocating the application of medical science to political economy, we think it needless to notice those minor methods which have been brought before the public by imaginative physiologists, especially on the Continent, with no other effect than to bring themselves and their subject into contempt, and to expose their brethren to ridicule. One seeks to render all men equal by an equal development of their faculties, and thus prevent all those evils which inequality in power and position produces. Another sees in large cities the greatest scourge of mankind, and, by annihilating them, would restore man to pristine simplicity and Arcadian happiness. An enthusiastic phrenologist would have the whole machinery of society regulated by his favorite principles; he would choose a servant or a judge phrenologically, conduct his amours by cranioscopy, and educate his child with exact reference to the development of the "organs." A favorite means for the improvement of mankind is with one to be found in a perfect system of gymnastics; with another in the scientific crossing of races; with another in the admission of women to political rights. Some would fix the age of marriage; some advocate polygamy and divorce physiologically; some would forbid persons having hereditary diseases to marry at all. The suppression of prostitution, and the eradication of the diseases thence arising, have occupied others; while a host of customs and habits, as the use of stays, garters, and breeches, of tobacco and snuff, of flesh as an article of diet, of tea, coffee, and chicory, &c., have each had their crotchety reformers.

There can be no doubt but that medical science is to be a mighty moral agent for centuries to come, and that its application to social and political economy promises the most brilliant results. The arrest and extinction of epidemics is one of these; another is the highest salubrity attainable by architectural arrangements and domestic sewerage. These and the like must await the evolution and development of medical science itself; but the minor points referred to above are within the power and judgment of the individual practitioner. He is a social reformer in the

highest sense of the word. Everywhere he comes in contact with misery and vice, with degraded habits and injurious customs, with the numerous families of the poor, and the sterile pampered homes of the rich. To all he can give advice with benefit, and in every sphere of labour diffuse a knowledge of hygiene. If there be a "disgrace to the family," it may be within his power to show that it is a species of eccentricity, bordering on insanity, which guides the culprit's actions; and it is education and moral culture that will reclaim him, not punishment. The matrimonial alliances of those families that give him their confidence, may be rendered safer and happier by his skill and knowledge as to the detection and demonstration of hereditary diseases. If a family become inconveniently numerous, from the indolence of the mother, he can show that, in many instances at least, the natural check is prolonged lactation and a diligent attention to maternal duties. And thus the enlightened and conscientious practitioner can act with the multifarious relations of hygiene.

The question may arise, how far the communication of this knowledge should be oral and private, and how far oral and public, or even published? The widely-extended circle of readers, the multiplication of institutes of popular science and literature, and the greatly increased number of dabblers in philosophy and physic, render this question of some importance. But it appears to us that it is already decided as to the main point, that such knowledge shall be communicated. Lecturers at temperance meetings demonstrate the anatomy of the abdominal viscera and the physiology of digestion; institutes of popular science include anatomy and physiology in their curricula; and, more decisively still, some of the men that have been most distinguished in the profession have laboured by precept and example to diffuse a popular knowledge of hygiene and of the medical sciences related to it. In short, such a diffusion is really one of the necessities of the age,—the age of great cities and large masses of men. All attempts to attain a higher degree of salubrity amongst such will be comparatively failures, so long as the people do not appreciate them with the earnestness of enlightened judgments. The requisite degree of information can never, we think, be communicated by the desultory efforts just mentioned; nothing short of the method by which other fundamental and necessary departments of knowledge are imparted will serve the purpose; and we therefore think that the science of public and individual hygiene, in a popular form, should constitute a part of primary education. In the mean time, all existing opportunities may be seized of imparting the necessary knowledge, provided always that it be done with integrity; not with the sordid intent of surpassing a rival and gaining a meretricious popularity, but with the nobler object of making men wiser, happier, holier.

We might discourse on the duties of the physician in relation to society at much greater length; for, as his proper study is man, and his peculiar sphere of action is man's welfare, his release from disease and suffering, his advancement to a higher and better state of being, and, finally, the enhancement of his moral dignity, even so wide and comprehensive might be our theme. Let, however, the practitioner feel the grandeur of his mission and the power of his knowledge, and he will rightly regulate his conduct. He will say with Fichte:

"What is more noble than the impulse to action, to sway the minds of men,

and to compel their thoughts to the holy and divine?—and yet this impulse may become a temptation to represent the holy in a common and familiar garb for the sake of popularity—and so to desecrate it! What is more noble than the deepest reverence for the holy, and disdain and annihilation of everything vulgar and opposed to it? And yet this very reverence might tempt some one to reject his age altogether—to cast it from him, and cease to hold any intercourse with it.... It is evident from these considerations, that, for his peculiar vocation, the scholar needs shrewd practical wisdom, a profound morality, strict watchfulness over himself, and a fine delicacy of feeling." (p. 180.)

The duties of the practitioner to the sick and to his brethren have been so well discussed by various writers, that little need be said. A broad distinction should, however, be drawn between offences against morals, and offences against etiquette or propriety. Unquestionably the latter are often more offensive than the former, and their less importance is forgotten amidst the angry feelings excited; and then the punishment awarded is widely disproportionate to the fault, and a gross act of injustice is perpetrated. The principal offences of this kind originate from vanity and cupidity. Inasmuch as the worldly success of the practitioner depends so much upon the opinion of his skill and professional learning entertained by the public, and, in fact, upon a preferential opinion, and therefore implying a comparison with others in his favour, he is apt to bring his claims before the tribunal in a way which displays selfishness, meanness, and duplicity; he puffs his own merits—he depreciates his neighbour's. He is well aware that the tribunal before which he brings his cause is incompetent to decide between himself and his brother practitioner—that ignorant prejudices have to be pandered to, and an overweening conceit of knowledge flattered; and hence arise all those paltry attempts to depreciate the merits of a rival and exalt his own. We find one class who assume superior skill and talent on the ground of having been educated at a celebrated school, or at a school which they sedulously praise for the talent of its teachers, the length of its curricula, or the difficulty of its examinations; and will diligently inculcate the idea that a rival, educated at some other school, or holding the diploma of some other examining board, has completed his education and passed his examination at an inferior institution, and is therefore less able to treat disease successfully, and less to be trusted professionally than himself. The public knows little of the relative merits of the schools and examining boards, and is perhaps quite unaware of the fact that a perfect blockhead may make his way through any, or a paragon in industry and genius be ignominiously repelled; lay people, therefore, can only award the palm to the most plausible and the most loquacious, and hence arises a rivalry in petty arts of disparagement and self-laudation. The licentiate of Apothecaries' Hall will pass in disdain the licentiate of the Faculty of Physicians and Surgeons of Glasgow; the Oxford or Cambridge "man," looks down with a certain condescension of manner upon the "Scotch doctor;" and the "fellow" of a college will chuckle in his imagined superiority over the simple "licentiate." There is truly no substantial difference; but the result of this is mutual recrimination and the disparagement of all.

A very shallow and common method is the publication of a work in a popular form; but this method is not always adopted. Some empirical practitioners will write a superficial monograph on some interesting subject of nosology, and so use technical terms as to give it the air of a true

professional work. On analysis, it is found, however, to contain nothing worthy the notice of the profession; but the writer, remembering the proverb, *omne ignotum pro magnifico,* distributes it gratuitously amongst his patients and neighbours, and hopes thereby to secure an opinion in favour of his professional powers from the lay public, which he knows must be based on false grounds, and which is not awarded by his brethren. Another method is, under a pretence of love of humanity, of science, and of scientific truth, to directly depreciate the skill and ability of a rival, or to seize upon a real error, and upon that erect their own character for superior ability and integrity. Like the runaway thief, the offenders cry "stop thief." This ungenerous form of criticism is far too common amongst even the higher ranks of the profession; and is so prevalent in smaller towns and villages, where rival interests more directly clash, and where more is directly gained by depreciating an opponent, as to embitter all the relations of life. A striking illustration of this form of ethical aberration appeared lately in one of the weekly medical journals.

Whether a mere profession of religion, and the restraint of the ecclesiastical police which such profession involves, can check aberrations from the true principles of medical ethics, would appear from the preceding and similar instances to be somewhat doubtful. The heart of man "is deceitful above all things;" and, while the religious professor flatters himself that he is doing his full Christian duty, by a strict attention to the forms and ordinances of the church, the spirit and essence of Christianity is wanting, and he forgets its fundamental principle of conduct, the law of Divine Wisdom and Love, "Do unto others as ye would that men should do unto you." This only can be the foundation alike of medical ethics and medical etiquette. Much, no doubt, is to be gained by a natural suavity of demeanour and humility of feeling, by gentlemanly training and associations, and by a fear of the results consequent upon a breach of etiquette or ethical manners; but the only true and universally safe principle is that principle thus announced. And we would commend the comment of St. Paul on this new law of love, revealed by Christ, to be found in his letter to the Christians of Corinth, as the best code of medical etiquette, and as comprising all that is necessary for soothing or preventing those bickerings, jealousies, rivalries, and deadly enmities, felt and too much indulged by some professional men. "Love," St. Paul says, "is forbearing, obliging; love is not envious; love is not arrogant, is not proud, is not rude or selfish, or irritable or slanderous. It has pleasure in truth, and not in falsehood. It is content with all, confides in all, trusts to all, bears with all. The highest rank, the greatest skill, the profoundest learning are, without this, nothing; the greatest performances and accomplishments in literature and science without it are vain as the jingling cymbal." Such are the sentiments, freely translated as to form, but correctly translated as to the spirit, of the inspired philosopher and martyr of early Christianity. How much, from time to time, has there been occasion to regret that these sentiments have not influenced the feelings of many even distinguished philosophers and practitioners in the course of their intercourse, whether personal or literary!

We have brought to the test of these principles the rules of the Manchester Medico-Ethical Association. This institution was founded to frame a code of etiquette for the guidance of its members, to decide

upon all questions of usage or courtesy in conducting medical practice; to support the respectability and maintain the interests of the profession; to promote fair and honorable practice; to correspond with bodies or individuals in other parts of the kingdom on any matter touching professional interests; and, by its moral influence and the exercise of a judicious supervision, to prevent abuses in the profession.

The following are the bye-laws which regulate the disqualification for membership:

"Any practitioner who may act in opposition to the principles involved in the eight succeeding laws, shall not be eligible to the membership of this association; and if already a member, he shall, on infringing the same, be liable to expulsion.

"1. No member shall practise, professedly and exclusively, homœopathy, hydropathy, or mesmerism.

"2. No member shall, by advertisement, circular, or placard, solicit private practice.

"3. No member shall be the proprietor of, or in any way derive advantage from the sale of, any patent or proprietary medicine.

"4. No member shall give testimonials in favour of any patent or proprietary medicine, or in any way recommend their public use.

"5. No member, who may keep an open shop, shall sell patent medicines, perfumery, or other articles than pharmaceutical drugs and preparations.

"6. No member shall enter into compact with a druggist to prescribe gratuitously, and at the same time share in the profits arising from the sale of the medicines."

Looking at these bye-laws, with reference to medical ethics, the first consideration which strikes us is the total absence of any fundamental principles of medical morals or etiquette, the exclusively trade character of the laws, and their inadequacy to the purpose aimed at. If a member may not be permitted to practise any *one* of the three leading varieties of the day exclusively, there is nothing on the face of the laws to prevent him practising them conjointly. If a member may not solicit private practice by advertisement, circular, or placard, he may fee a reporter to publish a speech or a puff, or he may advertise a professional publication in the newspapers, get a paragraph inserted in the same as to the successful use of chloroform, or permit a friend to admire in print a singularly skillful operation. The three laws as to the ownership, recommendation, and sale of patent medicines are of questionable wisdom. The demand for remedies having specific objects, and according to approved formulæ, is one that is universal; it is felt *in* the profession as well as *out* of it. The authorized Pharmacopæia is a proof of the one; the "antibilious pills," "antiscorbutic drops," "family pills," and the thousand nostra of the quacks, are the proofs of the other. The demand then being certain, why leave the sale of these specific compound remedies exclusively to the ignorant pretender? Why not let the bye-law run that the practitioner shall not patent, recommend, or sell any remedy, the composition of which is not known to the Society and approved of by them?

We observe with pleasure that the sale of drugs and pharmaceutical preparations is permitted to the practitioner. It is certain such an acknowledgment of professional status to the shop-surgeon will be received by that class with gratitude, and will add dignity to each person so practising. It is the man of the *res angusta domi* ministering to the man of the *res angusta domi;* and the poor are glad so to get from the surgeon-

chemist what otherwise they *must* get from the drug-chemist, or not have at all—that is, aid in sickness, unless they seek a medical charity. The true method would be for the profession to have a fixed number of such authorized shops in each town, managed by qualified practitioners, but placed under the surveillance of the associated profession residing in the locality. At such establishments the poor could be chiefly supplied with drugs and advice across the counter, and depôts established for the sale of the popular medicines of the faculty.

We are well aware that there may be objections raised against such a plan, on the ground that it would impugn the respectability of the profession. The word respectable is perhaps the most ill-used word in the vocabulary; it is the phrase of a bilious mediocrity; generally speaking, the attempt to be respectable is nothing more dignified than a mere pandering to dullness. Anything that is poor, or ministers to the wants of the poor, is not respectable. The union surgeon is, according to those who pride themselves upon this quality, anything but respectable; the practitioner who prefers to walk rather than to drive a pair, is not respectable; the man who has his surgery to the street, and red and green bottles glaring in his window, is not respectable. This twaddle is by no means peculiar to the medical profession; the clerical is quite as bad. An Irish clergyman, it would appear from the papers, is somewhat of a pariah in the diocese of London; a "St. Bees'-man" is hardly respectable; the Cambridge graduate thinks his degree decidedly more respectable than that of Durham; Oxford prides itself as being more respectable than either. It will well become the medical profession, however, to throw off such petty prejudices and consider the ends of their being, their training, their glorious knowledge of human nature. Everything is respectable which helps on man to fulfil more pleasantly his weary pilgrimage; everything respectable by which the profession, as the interpreter of Divine Providence, holds out the hand of succour to suffering man, relieves his woes, developes his powers, elevates and refines his nature, and fulfils all those duties which we have already ventured briefly to shadow forth. A Medico-ethical Association should therefore make these objects the aim of its institution; its bye-laws and rules should centre round these, and with its members thoroughly imbued with a sense of their high calling, we should have less need for those minute regulations comprised in the Manchester code of etiquette, and which seem so obviously proper, that it appears almost superfluous to express them in writing. An ethical association should take the passage we have quoted from St. Paul for the motto of its code of etiquette: "*Love is obliging; love is not envious; is not arrogant, is not proud; is not rude, or selfish, or irritable, or slanderous.*"

The ethical relations of practitioners in their corporate capacity, and as having public duties to perform as members of medical corporations towards the public and the profession, are of sufficient importance to merit special notice. Many, if not all, the remarks made with reference to medico-ethical associations apply specially to these. An inquiry into the ethical history of our corporations is not one which affords a pleasing retrospect. If we take the bye-laws of the London College of Physicians, they will not bear a comparison with those of the Medico-ethical Association of Manchester. We find no principles of ethics laid down; no rules to

guide the physician in his ethical relations towards the public or his patients, but simply a code of etiquette based on collegiate exclusiveness. Every physician, whether fellow or licentiate, shall attach to each prescription which he writes, the day of the month, the name of the sick man, and his own initial, on pain of being fined five pounds. No member of the College shall hold a consultation in London, or within seven miles, with a physician not being a member, on pain of the same penalty. Such are examples of the College bye-laws.

This exclusive dealing and narrow adhesion to the mere civic privileges of the College, although degrading to what aspires to be a National institution, would have some merit if it created a feeling of brotherhood amongst the London physicians; and if the College, like the other civic guilds of the city of London, were remarkable for its hospitality and good-fellowship. But the history of the College is one tissue of squabbles, lawsuits, and recriminations. We find few of the results of that law of love which we have proposed as the fundamental principle of medical etiquette and ethics. Love is not arrogant, but the "fellows" of the College forgot this when they treated the licentiates as an inferior class. Love is not rude or selfish; but the fellows appear to have been both to their humbler rivals for fame and practice. Love is forbearing and obliging; we fear the licentiates will not say that the fellows have been so to them.

The recent parliamentary inquiry has caused the opinions held by leading members of the College to be made public; and to persons at a distance from the battle-field of professional rivalry, it seems incredible that men presumed to have high general and professional attainments, should show so little nobleness of mind or greatness of purpose. Those attainments seem rather to have narrowed their views and contracted their feelings, than to have expanded or enlarged them. Cockneyism and club-exclusiveness seem to be their guides, and consequently offences against sound ethics and etiquette abound. We will supply a few illustrations; and we do this with an unaffected desire to correct what we think grave errors, prejudicial to the profession at large, but especially so to the class of physicians. We should be sorry to be thought censorious; all we propose is, to lay down certain principles of ethics, and then try how far the conduct of medical corporators is in accordance with those principles.

Dr. Paris thinks that "a higher order of physicians should be secured for the metropolis." By a higher order Dr. Paris means physicians educated to a greater extent. Thus the inhabitants of London are, in the opinion of the President of the College, to have more efficient physicians (if a better education and greater efficiency go together) than the inhabitants of Bristol or Liverpool, Leeds or Manchester. But how can this be reconciled with the great principle of moral duty? Supposing it to be granted, as Dr. Paris assumes, that the metropolitan physicians *are* more highly educated than the provincial, that such a system "has always worked very well, and has preserved very much the dignity of the profession," as Dr. Paris gravely asserts, would it not have added still more to the dignity of the profession to have had the physicians of the teeming millions without the seven-mile circle, as highly educated as the physicians to the two millions within it? Would they not have had a deeper feeling of their moral responsibility, a higher estimate of their powers for good, a

greater earnestness of resolve to exercise their powers, and obtain that good for poor, suffering, debased humanity? Would not, we ask Dr. Paris, such results as these be greater and holier than that increase of dignity—another word for respectability, which seems only in his case to have had the effect of an overweening estimate of the qualifications possessed by a London physician? But after all, it is but an assumption of the aged man of Dover street, that the physicians of Bristol, and Liverpool, and Leeds, and Manchester, and Birmingham are less educated than their metropolitan brethren. For him, it appears, the press has worked and the literary and scientific labours of the provinces have been poured forth in vain; but love boasteth not itself, and Dr. Paris, as the President of the metropolitan body, should not thus have arrogated for himself and his brethren a superiority of education and qualification; it is nothing less than an imputation (as compared with themselves) of ignorance in the provincial physicians.

Let us take another example from the same quarter. Dr. Burrows is one of the Censors of the College of Physicians, and acknowledges that a part of his duty is to exercise a considerable moral control over the members of the College. This acknowledgment implies a study of the principles of medical ethics on the part of Dr. Burrows; let us then see with what effect he has prosecuted those ethical studies. Dr. Burrows was asked whether, in his opinion, the number of students in London had diminished in consequence of the reputation of the provincial schools; in reply, he disparages "the goodness of the article," to use his own phrase, sold at the provincial schools; it is cheap, but not good; for eminent men in their localities will not give their time to tuition. Before Dr. Burrows hazarded this opinion, had he made himself acquainted with the facts? Had he actually inquired as to the staff of these provincial schools, and ascertained whether the eminent physicians and surgeons of the towns in which the schools are established were connected with them or not? We venture to assume that Dr. Burrows had not made inquiry, otherwise he would not have uttered the opinion. But when it is remembered that Dr. Burrows is himself a teacher in a London school, and therefore a competitor with his provincial brethren for pupils, are we not justified in saying that Dr. Burrows has not acted according to those principles of medical etiquette we have laid down? His offence is, we think, to be placed in the same category as that of Dr. Paris. If it be wrong to enhance our own merits by depreciating the merits of our rivals, then both the President and Censor of the Royal College of Physicians have done that wrong; done it before a lay tribunal, and by so doing, have lowered, so far as their opinions may be esteemed valid, the reputation and dignity of the physicians and teachers of the medical sciences out of London. They have injured the profession at large without the city boundary, by diminishing, so far as their opinion can diminish, the confidence and esteem which members of the legislature may have hitherto afforded the higher class of the profession, and by so much they lessened their weight in society, and their influence for good. We do not say how much, and we honestly think it is not much; but the offence against a sound code of medical ethics is not the less because the offender is impotent for evil.

Let us take another example of metropolitan arrogance and club-exclusiveness, and compare it with the conduct which a healthful line of gentle-

manly feeling and medical morals would induce. Dr. Seymour, who it appears was formerly a censor, and ought consequently to know some little of medical etiquette, is asked, "You say that you consider the extra license an abomination?" He boldly answers, "I do; it is very injurious to the profession in every way, and ought to be done away with, unless we wish to have *officiers de Santé* in this country." Upon further questioning, Dr. Seymour states apparently the grounds of his opinion; and these appear to be, that the examinations are not upon paper for three days, nor are examinations in Greek and Latin instituted; and very recently the examinations have been greatly increased in severity. We cannot comprehend how all this could raise up a class of such inferior practitioners as the officiers de santé in France; an examination greatly increased in severity is the best-acknowledged means of securing diligence in study, and an extended education. But this discrepancy in his evidence is not the greatest offence against medical etiquette, which Dr. Seymour has committed in his corporate capacities. Two hundred and fifty-five practitioners hold the license thus stigmatized as "an abomination;" they have in virtue of that license an equal legal right to the honours and privileges of physicians, with the graduates of Oxford and Cambridge; the curriculum of study required from them previously to examination, is of the same extent as that required from the candidates for the London license; and they have all been examined by the proper authorities and declared competent to practise medicine. The greater number are physicians in actual practice, and some of them have contributed largely to medical literature. No individual is on a *lower* footing than individuals amongst the general practitioners, the intra-licentiates, or even the fellows. Is this conduct compatible, we ask, with that true gentlemanly feeling which is not "rude, proud, selfish, slanderous; which has pleasure in truth and not in falsehood?"—we quote the words of St. Paul; or can Dr. Seymour be fairly acquitted of a breach of those principles of medical ethics which we have laid down?

We have not made these remarks, we feel conscious, in a captious or censorious spirit. We have propounded certain principles of morals for the guidance of professional conduct, whether the individual acts in his individual or in his corporate capacity. We have taken certain public deeds and doings by public men solely to illustrate our views, and without the slightest personal feeling to any. We have, we venture to state, a higher object than the exposure of small delinquencies, committed, we believe, from an imperfect appreciation of what is morally right, and therefore to that extent excusable. The conduct of practitioners in their individual and corporate capacity, becomes of a higher importance when we remember that, as the conduct is dignified and regulated by a high standard of ethics, just in that proportion will the dignity of the profession generally be enhanced, and its usefulness increased. It is undoubtedly to the interest of society, that the medical profession be accounted dignified and honorable; for then honorable and dignified minds will seek to enter it. But how can members of Parliament and the educated classes esteem a profession, the members of which mutually disparage each other? It can never follow in the judgment of the educated and enlightened portion of the public, that the discoverer of the motes in his brother's eye has his own free from them; while they believe as to the motes in the

one case, they will suspect the existence of a beam in the other; and thus a low estimate is entertained of all. This is no imaginary evil; for we happen to know an English nobleman, who, struck with the depreciating tone in which his London physician spoke of the provincial practitioner, mentioned it to the latter; and the result was a reciprocity of crimination, injurious to the character of both. We believe that that grace, which yields with candour the palm of superiority to a competitor, and which praises a rival rather than depreciates him, is to be attained by minds of the highest order only; but we think that if physicians and surgeons were to remember, that every disparagement of their rivals and their brethren was virtually an injury to themselves, the feeling of self-interest would come into operation, and place that check upon their actions which a higher principle would not supply.

Leaving, now, the principles of ethics already stated to be applied by our readers to individual conduct, and to be used as the test and touchstone of such acts as we have alluded to in the preceding pages, we take up the question of professional education in relation to ethics, with the view of offering a few suggestions in regard to it. Considering the wide scope of medical studies and their intimate relation to mental philosophy, we do not, we think, claim more for them than we ought to claim, in designating them as containing the elements at least of that learned culture which leads men "to the attainable portion of the Divine Idea." Looking at these studies in the abstract, we cannot but think that there is some radical defect in the plans of the directors of those studies, and of the agents of that learned culture, since so little of that grandeur of principle, that deeply pious feeling, that elevation of sentiment found in the doctrines of Fichte, as well as in the Gospel of Christ, are found in the sentiments and opinions expressed by leading members of the profession, and especially by those who assume to themselves the duty of directing the learned culture of the medical student. On looking through the evidence given by distinguished individuals in the metropolis before the Parliamentary Committee on Medical Registration, that is to say, by the presidents and examiners of the Royal College of Physicians and Surgeons of London, we find no reference whatever made to ethics as a part of the learned culture of the students; we do not find the sentiment once expressed that the pursuits of the student should be directed towards the attainment of higher objects than the successful and lucrative performance of mere professional duties; we find, not merely no reference to the "nature of the scholar," but no reference whatever to those moral duties which are taught in the fundamental doctrines of our common Christianity. We fear that a higher tribunal than ours would say of such leaders of professional culture, it is the blind leading the blind; or if Fichte could rise from his grave, he would not quote the first part of the inscription on the tall obelisk that surmounts it, as being applicable to them: "THE TEACHERS SHALL SHINE AS THE BRIGHTNESS OF THE FIRMAMENT,"—so runs the funereal legend;—is it, or can it be made, applicable to the teachers of the medical schools?

Surely, it is time that medical ethics took a higher position than this! Surely, there should be in each school, if not a chair of ethics, the means, at least, of a moral culture afforded. We think men competent to the task of teaching them would be rare; so few estimate rightly the im-

portance of the nexus between religion and science; so few have the force of intellect that could seize the vast extent of the two branches of human knowledge, and weld them into a compact and efficient form. But who knows? Some medical Fichte may arise and glad the ears and hearts of listening thousands; some bravely and eloquently wise man may appear in the field of medical culture, and head the crowd of young and enthusiastic aspirants to moral glory; and so with them, both the first and second part of the inscription on Fichte's funereal obelisk will be fulfilled:

"THE TEACHERS SHALL SHINE
AS THE BRIGHTNESS OF THE FIRMAMENT;
AND THEY THAT TURN MANY TO RIGHTEOUSNESS
AS THE STARS FOR EVER AND EVER."

Seed may be sown by the scholar as a teacher on an unfruitful soil; he may feel that he has failed in guiding the studies of his hearers to the most essential study of all. Yet, after all, he may not have laboured in vain; and this, indeed, is *our* hope.

"He can never know that he has not thrown into the soil some spark which, though now unapparent, will blaze forth at the proper time. Even in the worst possible event,—that he has not accomplished so much as this,—his activity has still another object; and if he has done something for *it*, his labour has not been utterly lost. If he has, at least, upheld, and in some breasts quickened or renewed the faith, that there is *something* worthy of the reverence of men; that by industry and honour men may elevate themselves to the contemplation of this object of reverence, and in this contemplation become strong and happy; if some have only had their occupation made holier in their eyes, so that they may approach it with somewhat less levity than before; if he can venture to hope that some have left his hall, if not precisely with more light, yet with more modesty, than they entered it;—then he has not entirely lost the fruit of his exertions." (p. 206.)

It is not, truly, as a lecturer alone, that the scholar is a teacher; as an author, he may have a more widely numerous audience. There is no class of professional men, to whom some well-defined principles of morals are more necessary than this. The direction of the mind exclusively to one object of pursuit is apt to narrow the intellect, and render the man bigoted and intolerant of all those who interfere antagonistically with that sole object. A mind so warped is deformed. It sees no beauty in others; it indulges in self-contemplation, self-admiration, and self-flattery, and entertains ideas of its own powers far beyond those of bystanders or antagonists. And the object thus pursued is very often pursued by such a mind, if not at first, certainly after awhile, not for its own sake, or for the greatness or moral grandeur inherent in it, but for the sake of the pursuer, and to lead *him* to greatness and grandeur. He makes the object of his pursuit a stepping-stone to wealth and fame; and thus selfishness joins with prejudice to render the man more intolerant of doubt or contradiction, more dogmatic, presumptuous, and dictatorial.

The claims to the honour of certain discoveries set forth by authors are ordinarily rendered doubtful, or are announced so as to embitter all the relations of professional and literary life, by feelings of this kind. On the one hand, it cannot be denied that men of mediocre intellect and abounding vanity are apt to jump up behind the genius, and attempt to filch from the latter the fame and honour belonging to it. Such literary

footpads and pirates abound; but we apprehend that attempts of this kind always fail. It may be sooner or later—there may be a difference as to the skill with which the merits of another may be appropriated or depreciated—but sooner or later the fraud will be detected. The false plumes will be stripped from the pretender; the spurious coin will be nailed to the counter of the literary world.

Very often those who have done good service to the profession, and have added to the garnered treasures of science, estimate their labours too highly, or resent with too great warmth and too intemperately the piracy or depreciation of others. They forget their vocation; its integrity, its objects, its duties. They look too much to self; the fine gold of their "Genius" becomes dim; their "Integrity" is lost or diminished. Proper self-appreciation is undoubtedly compatible with the highest integrity and the noblest genius; but neither are compatible with an intemperate, ignoble, vulgar mode of asserting claims to notice and fame. No man should lie down to be kicked; but he may, both as a matter of policy and morals, while firmly asserting his claims, turn the other cheek to the scoffer.

Professional intolerance is often shown in an undue estimate of the *artistic* skill of rivals; the practitioner not only esteems himself more skillful than he really is, but thinks his competitors less skillful than they really are. Self-contemplation and selfishness have blinded his eyes to his own demerits, and quickened his perceptions of the demerits of his rivals. We may all say, indeed, in this matter, so universal is the failing in some form or other—

"Oh, wad some power the giftie gie us
To see oursels as others see us!"

There are a large class of professional prejudices which belong to the whole profession. All bodies of men are intolerant of any departure from principles and practices that have become conventional. Although such departure may have nothing whatever in it morally wrong, yet it is visited "with the utmost rigour of the law"—that may have been conventionally established. Thus physicians fully engaged in practice will bitterly regard the young physician who, feeling the pressure of the *res angusta domi*, may exercise any surgical talent he may possess, or who, suspecting that his medicamina are not well compounded, or of a spurious quality, may look to the manufacture of his powder, or point his own guns. A submission to conventionalism is demanded in preference to submission to a moral or professional duty, and the patient must die, rather than the physician be guilty of bloodletting or pharmacy. But in the face of the great duties of the professional man, what sad trifling is this!

The obloquy and abuse which has been showered by the regular and conventionally practising physician or surgeon upon the various modern sects which modern times have produced in such abundance, arises from the same source as indicated above. The homœopathic, hydropathic, or mesmeric practiser, addressing himself to that large portion of society in whom superstition and credulity are influential, succeed for a time in eclipsing the conventional practiser, or "allopath," as his antagonists have termed him, and hence no little ill-will and envy arise. But, independently of this inroad upon the sources of pecuniary gain, perpetrated

by the homœopath and his congener, there is the outrage on current doctrines and conventional methods of treatment, which, to that numerous portion of the profession not given to change, is an unpardonable sin. In accordance with this feeling have been the denunciation of the medical heretic and schismatic by his orthodox brother; who, regardless of what there might be of truth in the new ideas and ways, has poured forth upon them the full vials of his unmitigated wrath. To any hints at moderation he has turned a deaf ear; all inquiries into the utility and veritableness of the new doctrines he has met with a scoff.

Such conduct is, we think, perfectly useless for any beneficial object. Nay, it is worse than useless, because the estimation in which homœopath or hydropath is held by the imperfectly informed public is not decreased by such unreasonable denunciations, but rather increased. They look upon him as the victim of unjust persecution, as a martyr to the cause of truth and honesty. Unreasonableness begets unreasonableness; and the defence therefore is as the attack. It is worse than useless, because the truth there may be in the system being thus rejected by the allopath, he is looked upon by the public as bigoted to his own ways, inapt to learn from others, unsafe to trust in sickness. Intelligent laymen, seeing that good has resulted from these new methods of treatment, cannot sympathise with that blind conventionalism which wages war to the knife with the doers of that good, and this the more decidedly, because in the ranks of the schismatics, men of science, or, at least, of a regular professional education and some position in the profession, are to be found.

We would then advocate a different course of conduct, not only as ethically right, but as expedient for the interests of the profession. Let the pretensions of the new sectarians be received in silence; let the results of their treatment be carefully investigated; let their methods and views be fully inquired into, in a spirit of enlightened and philosophical courtesy; let all that is good be selected and appropriated, and all that is bad rejected. This may be done with nothing more bitter than a laugh; may be done, too, with immense benefit to the practitioner and the public; and may be *so* done, that the empiric will at last find his level. When we hear of quacks carrying the public with them, we may be sure it can only be that they are better tacticians than the regular troops; but why should this be? The profession ought to know that human nature is a thing full of frailties and infirmities, and should be treated accordingly.

We perhaps should here refer to those bickerings and disputes between the members of different sections of the profession at large on points of etiquette, and the material duties and relations of practice. It too often happens that a petulant general practitioner will depreciate the skill and talents of his brother the physician, while the latter (perhaps too often in self-defence) asserts his superiority over the former. It is very clear that some rules for the definition of the relations between these grades or sections are wanting, or there never will be an end of bickering. If the physician be required to keep his place as the consultee, it should not be occupied by the general practitioner. If the physician be not allowed to act as surgeon, the surgeon ought not to be allowed to act as physician. Dr. Gregory discussed this question of grades and distinctions with his usual good sense. "As a doctor's degree," he observes, "can never confer sense, the title alone can never command regard; neither should the want

of it deprive any man of the esteem and deference due to real merit. If a surgeon or apothecary has had the education and acquired the knowledge of a physician, he is a physician to all interests and purposes, whether he has a degree or not, and ought to be respected accordingly." We should scarcely have made reference to this point of medical ethics, had we not seen an essay published in a popular periodical, entitled a "Plea for Physicians," so utterly wanting in sound principles of morals, as to call for the unqualified reprobation of the whole class.

And this brings us to our final division of our subject, namely, the organization and unity of the whole profession. Whatever the *individual* is required to do in accordance with sound principles of ethics, so much is incumbent on the *whole* profession, as a part of the body politic. Hence all those duties which have a relation to political economy devolve upon it; and all that discipline and legislation which is required for the individual practitioner, should emanate from it. How far this is practicable amidst the jar of contending interests and the clash of prejudices, is more than doubtful. The profession seems little better than a chaos; the whole mass is upheaving; decomposition and recomposition are going on; but we can discern no great principles by which coherence and strength may be given to the discordant elements. It is quite impossible that the intelligent lay public will notice the professional desire for organization and legislation, so long as the impelling motives are nothing more dignified than sectional interests, grade prejudices, or interested clamours in a pecuniary sense. The first principle should be unity to carry out the moral objects of the profession. Let "good-will towards men" be the rallying cry of those who seek an organization; let the list of their demands include every means whereby the profession may be made more extensively useful to mankind; in short, let religion and morals, in some such sense as we have so imperfectly set forth, be the sole object of their union. With these principles to guide, the humblest apothecary will find his place, and be made useful; every man of good principles and high feelings will be made welcome, however high or however low he may be; and so union and strength will arise. Then from every grade of the profession a higher tone of feeling may be looked for with reference to each other, and while all honorably compete for the public favour, the wisest and best will be at the head, and govern. The conventionalisms of differences of education and title, and the hundred pettinesses which set professional brother against brother, will appear in their true character as utterly insignificant, when compared with the gloriously beneficent and grand objects which a united profession might compass.

It is, indeed, remarkable, that while in those societies devoted to the cultivation of medical science and art, as the Royal Medico-Chirurgical Society, and as the numerous medical societies in the provinces of the United Kingdom, all grades combine and act in harmony, without complaining, and without selfishness or pride, the educational institutions raise their Shibboleth before the public, before Parliament, and in the profession, and establish their differences where there is scarcely any distinction. Of the leading men in the College of Surgeons in London, how few are there who have not only the qualifications of physicians, but the practice also. Take away the cases requiring medical treatment from the practice of the surgeon, and the best portion is gone. To all purposes,

and in every way, the surgeon is a physician, with the ability to operate chirurgically superadded to his medical acquirements, and is conventionally permitted to operate, prescribe, and receive his fee, so long as he calls himself "surgeon." But let him but add M.D. to his name, and conventionalism forthwith binds up his right hand, severs him from his college, and circumscribes the sphere of his usefulness; he is at once disabled and disfranchised chirurgically: he *may* possibly attain to office in the College of Physicians, but the College of Surgeons is to him hermetically sealed. Now, if it could be proved that this line of demarcation, already obliterated in the voluntary associations, is of any use whatever to either the profession or the public when drawn between two classes of practitioners, in which the difference of education and attainments is *now* at least really but nominal, we would acquiesce at once in the arrangement. But it has yet to be shown that a union of these two educational institutions, and a reorganization on a broad base of ethical principles, would either render the surgeon less skillful, or the physician less educated or intellectual. The whole matter is indeed hardly capable of serious argument. It is manifest that the private interests and feelings alone of influential metropolitan practitioners maintain the *status quo;* we cannot doubt, however, that the enlightened practitioners in the provinces will ultimately address themselves to the question, place the organization of the profession on its proper basis, and, looking to the kindred professions of the Church and the Bar, take that which is worthy of imitation from both. It is true that the Church, at least in some degree, has lost that perfection of organization and unity of action which it once possessed; but the plan and the principles remain, and are worth the study of him who would apply medical science and art, and a numerous body of its practisers (numbering tens of thousands), to the improvement of society, and the amelioration of man's moral and physical nature.

We have finally to consider what are the ethical relations which society ought to bear towards the medical profession, or, in other words, what are the duties of society and the rights of practitioners. If the profession were an organized body, like the clergy of various sects, and if, by a comprehensive system of corporate government, the minds of its members were so trained and disciplined as to take that line of conduct which the true principles of ethics point out as just and therefore expedient, we could not doubt for a moment that it would attain to a higher importance in the estimate of mankind, and would share in those external marks of esteem and gratitude which society has conferred upon the clerical and legal professions. We should not doubt to see its dignitaries sitting amongst the peers of the realm, or its representatives amongst the representatives of the people. Poverty there would also be amongst us, as there is amongst the clergy; and men amongst us, who would disgrace their calling; but we think all history teaches the important lesson, that wise and mighty schemes of benevolence, carried out by the combined powers of educated men, never failed to aggrandize the agents. Still the riveting bolt of high and holy principles must unite them; personal aggrandizement must only be sought by the advancement of the general weal; the selfish instincts must be disciplined into docility; otherwise the profession will remain as it is—a chaos of conflicting elements.

ART. II.

Contributions to the Pathology and Practice of Surgery. By JAMES SYME, F.R.S.E., Surgeon in Ordinary to the Queen in Scotland, &c. &c.—*London*, 1848. 8vo, pp. 336.

THIS work consists of a collection of papers on different subjects in surgical pathology and practice, which have appeared during the last twenty years in various periodical publications. As many of them are sufficiently well known, or on subjects fully understood, and as others have in a great measure lost the interest that originally attached to them, we shall be content to notice those contributions which are more particularly calculated to inform and engage the attention of our readers.

Article I is on the solution of caoutchouc, its purport being to show that in the application of this useful substance the author had anticipated Mr. Macintosh, but without gaining any credit or profit from the improvement. Mr. Syme's pretensions, having no particular interest for the profession, need not detain us.

In Article II Mr. Syme enforces the advantage of a far more rigid abstinence in respect to diet, than is commonly considered necessary in the treatment of senile gangrene. He restricts his patients to a strictly vegetable diet, and allows no sort of stimulant. Since the publication of his paper on this intractable disease, in 1841, he states that the opportunities of further observation have tended to confirm the views it communicated to the profession. Surgeons have long been convinced of the impropriety of the highly-stimulating treatment formerly recommended in the management of *gangrena senilis*, and Mr. Syme is mistaken in supposing that the practice generally employed still continues the same as Mr. Pott has described it. In Sir B. Brodie's observations, quoted by our author, ale, wine, or brandy is allowed to patients affected with this disease, not, indeed, to obtain a stimulating effect, but chiefly from fear of a failure in the powers of life from depriving patients of an accustomed stimulus. "It is true," Sir Benjamin adds, "those whose mode of life has been different will require the exhibition of stimulants under these new circumstances." But he further remarks, in regard to the quantity of wine, "In the majority of cases, from the third of a pint to a pint daily will be sufficient," which our readers will regard as tolerably moderate. He recommends, too, stimulants to be given cautiously, observing that "any quantity of wine that does not occasion heat of skin, nor raise the pulse, nor make the mouth clammy, nor render the patient nervous or irritable, may be given with advantage; but whatever does more than this does mischief." We have made these quotations from Sir B. Brodie's recently-published Lectures, because they express pretty clearly the practice which is commonly employed in London, and the effects of which are by no means what Mr. Syme represents in his allusion to "the admitted hopelessness of the case under ordinary treatment." The results of cases of senile gangrene are, we fully admit, sufficiently unfavorable to justify a fair trial of new principles of treatment, especially coming from a surgeon of Mr. Syme's experience; but whether his system of rigid abstinence, or limiting the patient to a strictly farinaceous and milk diet, be preferable to the plan of nourishing and supporting the system with animal food and

stimulants in great moderation, as recommended by Sir B. Brodie and others, further experience can alone determine. We have said nothing of local treatment, as all are agreed as to the principles upon which it is to be conducted.

Articles III and IV contain nothing but what is now well understood by the profession. In the first, Mr. Syme gives an account of some bold operations for excision of the lower jaw, the details of which are interesting; but he makes no attempt to define the diseases of the bone suitable for the operation—a matter upon which we should have been glad to have had the benefit of his experience.

In Article V, on Ulcers of the Leg, Mr. Syme describes a plan of treating callous ulcers, first recommended by him in 1829. This is—

"To apply a large blister over the sore and neighbouring swelled part of the limb, which has the effect of speedily dispersing the subcutaneous induration and thickening, so as to relax the integuments, and thus remove the obstacle opposed to healing action. In the course of a short time, seldom exceeding a few days after the blister has been applied, the surface of the ulcer, however deep it may have been, is found to be on a level with that of the surrounding skin, not, of course, through any process of reproduction or filling-up, but merely from the removal of interstitial effusion, allowing the integuments to descend from the position to which they had been elevated, as may be readily ascertained by measuring the circumference of the limb before and after it has undergone the effect of blistering. But along with this change of form, the ulcer, in other respects, no less speedily acquires the characters of a healing sore, assuming a florid colour, affording a moderate discharge of purulent matter, and presenting a granulating surface with surrounding margin of cicatrizing pellicle." (pp. 43-4.)

Mr. Syme considers this plan to possess decided advantages over the other methods in use, and to be preferable to the treatment by strapping and bandaging,on the score of economy, the expense of materials, and the attention requisite for their proper employment, placing the latter process out of reach of that rank which chiefly suffers from the disease. On this point we entertain a rather different opinion from our author. We admit that the application of a blister to a callous ulcer may be very serviceable in the first instance, in promoting the removal of interstitial effusion, and exciting a new and healthy action. But considerable opportunities for observation have satisfied us, that, in the management of these sores, nothing can be more efficacious, successful, and suitable for the circumstances of the poorer classes, than strapping and bandaging as modified and improved by Mr. Scott. The cost of material may be estimated at a shilling, and renewal of the plaster, &c., is required, on an average, twice a week. But this weekly expense of two shillings secures for the patient the means of following his daily labour, the mechanical support afforded to the weakened vessels enabling the healing process to advance steadily, whilst he continues at his usual work. Whereas, in pursuing the blister treatment, rest of the leg in the horizontal posture can seldom be dispensed with, the patient losing, therefore, what is far more costly than a few yards of plaster and calico—the ability to labour.

In some observations on ulcers of the legs, proceeding from the injurious influence of mercury on the system, Mr. Syme, after condemning the practice of giving mercury, remarks: "The enlightened views of the late Dr. Thomson gave Edinburgh a distinguished place in reforming this department of medicine; and though the comparative slow progress

of improvement in the capitals of England and Ireland may still, perhaps, tolerate the administration of mercury for the ulcer in question, it was long since abandoned in this school, and succeeded by treatment of a local kind." We cannot pass this passage without censuring the bad taste and want of courtesy which characterise it. Mr. Syme, having migrated southwards, has probably learnt by this time that in the practice alluded to, the surgeons of London are in no respect *behind* those of the capital of Scotland, and ascertained the absurdity of the claims to superiority which he prefers for the latter.

The object of Article VI is to show that obstinate sinuses in the region of the pelvis sometimes depend on death of some part of the pelvis far from the surface, and are to be cured by removal of the loose exfoliation. The occasional occurrence of such exfoliations, and the treatment obviously required when they are detected, are matters with which practical surgeons are well acquainted.

In Article IX, Mr. Syme describes a case of disease of the shoulder-joint, in which he excised the carious head of the humerus with a successful result. This operation is a revival of one originally performed by Mr. White of Manchester; and the case having attracted notice at the time it was published (1826), is probably familiar to most of our readers. The chief point of interest at present, is the account here given of the benefit derived from the operation. Mr. Syme states—

"This patient survived the operation ten years, during which she enjoyed perfectly good health, and the full use of her arm, with which she was able to sew, knit, and do all ordinary domestic work, without any feeling of uneasiness or perceptible defect. After her death I had an opportunity of examining the parts concerned, and found a firm ligamentous texture, uniting the bones, so as to prevent undue mobility, but permit motion in all directions to the ordinary extent. It may be remarked, that while she had the perfect use of her hand and fore-arm, and could move her arm at the shoulder backwards and forwards, she did not regain the power of raising the limb directly from her side, by the muscles of the shoulder, but experienced no inconvenience from this source, as, by a sort of climbing action with the hand upon her dress, she elevated her arm so quickly and easily, as to elude the most careful observation." (pp. 97-8.)

Mr. Syme attempts to show that the case "in which Mr. White operated was necrosis, not caries; and that, instead of cutting out the head of the humerus, he had merely accelerated the separation of an exfoliation of its shaft." Whether the disease was necrosis or caries, does not seem to us at all to affect the credit which is undoubtedly due to Mr. White, of having first performed the operation of excision of the head of the humerus, not as Mr. Syme puts it, of excision of the shoulder-joint. Neither Mr. White nor Mr. Syme interfered with the articulating portion of the scapula, and consequently neither of them excised the shoulder-joint. It is far from clear that the disease was, as Mr. Syme supposes, acute necrosis. The case may have been one of acute suppuration, with caries of the head of the humerus, as the figure in Mr. White's book seems to show; and the subsequent exfoliation of the shaft may have resulted from the injury inflicted on the bone by the saw, which, we need not inform Mr. Syme, is common enough after amputation.

We pass by the next article, on "Amputation at the shoulder-joint for the removal of an osteo-sarcomatous tumour," which is interesting only on account of the great size of the tumour (it weighed 12 lbs.), and pro-

ceed to notice Mr. Syme's observations on amputation at the ankle-joint, an operation, the value and applicability of which are not yet fully determined by the profession. As Mr. Syme's claims to the merit of reviving and suggesting a new mode of practising this operation are not disputed, he might have avoided prolixity and much tedious repetition, and awkward corrections, by giving an account of the operation founded upon his later experience, instead of reprinting the five papers communicated to the 'London and Edinburgh Monthly Journal of Medical Science,' with additional observations. This method of amputating was proposed for those cases of incurable disease of the foot, in which the astragalus and os calcis being affected, the operation of Chopart is inapplicable,—cases previously treated by amputation below the knee. The advantages reported to be gained by Mr. Syme's operation are less risk to life, a more convenient stump, and a more seemly and useful limb. Our author gives the following directions:

"The best instrument for performing the operation is a large bistoury, or small amputating knife with a blade about four inches long. There is no occasion for a tourniquet, as the assistant has complete command of the vessels by grasping the ankle. In my first operations the flap was made unnecessarily long; and I feel confident that the following directions may be trusted for exactly determining its proper extent. The incisions across the instep and sole of the foot should be curved, with the convexity forwards, and exactly opposite each other. A line drawn round the foot, midway between the head of the fifth metatarsal bone and the malleolus externus, will show their extent anteriorly, and they should meet a little way further back, opposite the malleolar projections of the tibia and fibula. Care should be taken to avoid cutting the posterior tibial artery before it divides into the plantar branches, as, in two cases where I did so, there was partial sloughing of the flap. If the ankle-joint is sound, the malleolar processes should be removed by cutting pliers; but if the articulating surfaces of the tibia and fibula be diseased, a thin slice of these bones should be sawn off. The edges of the wound should be stitched together, and lightly dressed. When the cure is completed, the stump has a bulbous shape, conical in form on the interior surface, and having for its apex or central point of pressure the thick integument which covered the heel." (pp. 123-4.)

Notwithstanding the directions so confidently expressed in the preceding extract, Mr. Syme subsequently remarks:

"In my earlier operations I made the flap unnecessarily long, and in consequence suffered two inconveniences,—in the first place, from difficulty in executing the dissection, and, secondly, from the occurrence of sloughing in the event of the posterior tibial artery being divided above its division into the plantar branches. Succeeding experience taught me that a much smaller extent of flap than had originally been considered necessary was sufficient for the purpose, and that hence the operation could not only be simplified in performance, but increased in safety from bad effects.

"I am now able to state precisely the limits of incision which will be found to render the practice no less easy than secure.

"The foot being placed at a right angle to the leg, a line drawn from the centre of one malleolus to that of the other, directly across the sole of the foot, will show the proper extent of the posterior flap. The knife should be entered close up to the fibular malleolus, and carried to a point on the same level of the opposite side, which will be a little below the tibial malleolus. The anterior incision should join the two points just mentioned at an angle of 45° to the sole of the foot and long axis of the leg. In dissecting the posterior flap, the operator should place the fingers of his left hand upon the heel, while the thumb rests upon the edge of the integuments,

and then cut between the nail of the thumb and tuberosity of the os calcis, so as to avoid lacerating the soft parts, which he, at the same time, gently but steadily presses back, until he exposes and divides the tendo achillis. The foot should be disarticulated before the malleolar projections are removed, which it is always proper to do, and which may be most easily effected by passing a knife round the exposed extremities of the bones, and then sawing off a thin slice of the tibia connecting the two processes." (pp. 146-7.)

The inconvenience of reprinting these separate memoirs instead of giving a concise account of the operation, founded upon further experience, is fully shown by the foregoing extracts. In one of Mr. Syme's operations, bagging of matter took place in the posterior flap, and required a counter-opening for its discharge. In another case, a counter-opening was rendered unnecessary, an opening through the integuments of the heel having been made in the operation, owing to their thinness and firm adhesion to the bone. This is an accident at all times very liable to happen. Mr. Syme observes, however, that the drain thus afforded has proved so useful, by permitting a free escape to the discharge, and allowing the edges of the flaps to unite throughout their whole extent, that he thinks its intentional establishment would be advantageous. Mr. Syme relates ten cases in which this amputation was performed with a successful result. At the end of the article, a satisfactory account is given of the state of the stump in two of the cases, a few years after the operation. In one, in which only the malleolar portions of the articular surface were removed, when the stump was examined, nearly four years afterwards, it was quite sound. The patient suffered no inconvenience nor tenderness. He had often ten and fifteen miles to walk to go to his work, yet felt no discomfort. In the other case, the whole of the articular extremities of the tibia and fibula being carious were removed. At the end of three years after the healing of the stump, the patient experienced no pain or uneasy sensation in it. He could bear the weight of his body on the naked stump without inconvenience. The patient says he uses a stick in walking, but except on rough causeways, or very uneven ground, it is unnecessary, neither is it required in ascending or descending stairs.

We wish Mr. Syme had described what kind of artificial foot he had found to answer best in these cases. The comfort of the patient materially depends upon a clever contrivance; and although we must look to an instrument-maker for its construction, the hints of a practical surgeon may be useful to those who cannot avail themselves of the service of a mechanic experienced in these matters. The letter of Mr. Syme's patient, at page 142, giving an account of his artificial foot, may be read with advantage.

Having stated the results of Mr. Syme's latest experience concerning the amputation at the ankle-joint, we have pleasure in expressing our obligations to him for the revival of this operation. It has been successfully performed in London, in the mode recommended by our author, by the late Mr. Liston, and by Messrs. Fergusson and Avery. In comparison with most of the other amputations, it is certainly one difficult of performance, and the surgeon will do well to practise it several times upon the dead body, before venturing to operate on the living. The liability of the flaps to sloughing, and the delay in healing, from the bagging of matter and the formation of sinuses, are certainly drawbacks. These may, however, admit of being counteracted in a great measure by care in the

operation and skilful management afterwards, and we are disposed to view the amputation at the ankle-joint as a valuable improvement in surgery. We venture particularly to recommend to our metropolitan brethren a trial of it in cases of compound dislocation of the ankle. This is not an uncommon accident in London, and it is one which usually ends badly, when the attempt is made to save the leg. The soft parts being in these cases sound, we believe an useful limb may be preserved by the performance of Mr. Syme's operation; the stump being, as he rightly remarks, "in all respects preferable to a shattered, stiff, irritable foot." Our author states, that in the Royal Infirmary of Edinburgh, of thirteen patients who had suffered compound dislocation of the ankle, and were not subjected to amputation, only two recovered. We are much mistaken if the records of the hospitals in London show a more favorable result.

In Article XII, Mr. Syme advocates the performance of amputation of the thigh, at its lower third, by circular incision in preference to the flap operation, though he considers the latter most proper for amputations at or above the middle of the bone. The question of preference to be given to circular or flap amputations in the removal of limbs, is one of much importance, and well deserving of consideration at the present time; but the observations in this paper, referring only to amputations of the thigh, are insufficient to induce us to enter fully on this subject now.

Article XIII contains three interesting cases of axillary aneurism. In one, the subclavian artery was tied, but unsuccessfully, and amputation was afterwards performed at the shoulder-joint, from which operation the patient recovered. In the second case, the limb was also removed at the shoulder. In the third case, the disease was remedied by ligature of the subclavian artery without removal of the limb. Both these cases did well.

In Article XIV, on the treatment of popliteal aneurism, Mr. Syme takes occasion to express his unfavorable opinion of the treatment by compression, which has been revived within the last few years by Dr. Bellingham of Dublin. That the operation of tying the femoral artery, when properly performed, is attended with a certain amount of danger, we are fully convinced of; notwithstanding the success of the cases operated on by Mr. Syme and Mr. Busk, which are referred to in this paper. Dr. Bellingham is certainly entitled to some credit for the skill and perseverance with which he has practised compression, and for his efforts to cure a formidable disease without recourse to an important operation. But even with the improved apparatus now in use, and with every care and perseverance, compression usually proves painful, tedious, and uncertain; and we agree with our author that its advantages are not such as to entitle it to supersede the operation of tying the artery. In a popliteal aneurism of small size, affecting a patient possessing considerable powers of endurance, we should be disposed to give this plan a trial. But when the disease is more advanced, we think it better to operate at once; for we have witnessed cases of failure from compression, the tumour continuing to enlarge during the treatment. In one case, the delay consequent upon the trial of compression was attended with a serious result, the sac having giving way, and the aneurism become diffused, so that it was necessary to amputate the limb. Mr. Syme quotes a case communicated to him by Mr. Busk, in which gangrene of the limb followed the application of compression to the femoral artery. Amputation was performed, and the patient died.

This case shows that compression is not free from one at least of the same risks as attend the operation of tying the artery, viz. gangrene of the limb, from the stoppage of the circulation.

Article XV is on Fungus of the Testicle. Mr. Syme viewing the projecting fungus as being partly composed of tubuli seminiferi, and partly of organizable lymph, which on the surface assumed the character of granulations, so that the excrescence might be regarded as merely an extreme degree of exuberant granulation, or what in vulgar language is called "proud flesh," proceeds to remark:

"This observation suggested to me the idea that, by the use of proper means, the fungus might be made to retrace its steps, through absorption of the white substance, and gradual approximation of the brown, and that the granulating materials of the surface might thus be enabled to complete the healing process. Pressure was obviously the agent on which reliance should be chiefly placed for producing the effect desired with this view, and the most convenient mode of compressing the growth, seemed to be inclosing it within its proper covering of the scrotum." (p. 208.)

Two cases of fungous excrescence of the testicle are related. In the first, Mr. Syme "cut round the fungus, and extended the incision upwards as well as downwards, so as to give it an elliptical form. The integuments were then separated on each side, and brought over the growth, where they were retained by three stitches. The scrotum was supported by plasters and a bandage." The wound cicatrized in 25 days. In the second case a similar operation was performed, and the part healed in three weeks. We are surprised that Mr. Syme should assume to himself the credit of a method of treating this disease which preserves the testicle entire, and completes recovery in a shorter time than if castration were performed. He is quite in error in supposing that it is the recognised practice either to remove the projecting fungus by excision or by escharotics, or to excise the gland altogether. Sir B. Brodie, in his published Lectures so far back as 1834, recommended applications, such as lint dipped in a solution of sulphate of copper, in order to improve the character of the granulations and check their growth; under which treatment he states, the skin is "gradually drawn over the surface of the fungus, while the latter gradually recedes within the scrotum, until at last the fungus, or rather testicle (for testicle it is), becomes entirely covered by the scrotum, and the external sore is cicatrized."* Mr. Curling, too, in his recent work on the 'Diseases of the Testis,' published before the appearance of Mr. Syme's paper, describes the protuberance as composed of the tubuli testis mixed up with lymph, and of ordinary granulations; and condemns the excision of the fungus as an unscientific mode of proceeding, and one but little short of actual castration. He recommends the application of a strong solution of the nitrate of silver, and compression made by compresses of lint on the projecting fungus; and as the protrusion recedes, the scrotum is to be drawn over it, and the edges of the wound gradually approximated by narrow strips of plaster. Under this treatment, he says, cicatrization takes place, and the testis gradually resumes its place in the scrotum. Now we do not see that Mr. Syme's operation is any improvement on the plans recommended by Sir B. Brodie and Mr. Curling. We have had some experience in these cases, and can assert that the granula-

* London Medical Gazette, vol. xiii.

tions are not likely to adhere to the detached skin; indeed it appears that no such union took place in the two cases related by our author. In fact, the part closed in the ordinary way by granulations at the usual period.

The only suggestion to which we can attach any particular value, is one made at the conclusion of this article, viz. the removal of "the hard ring of skin through which the fungus protrudes, so that the whole integument concerned may be competent for adhesive action, not only on the surface, but also between its edges;" a proceeding which we think likely to expedite the healing process, by substituting a raw edge for an indolent and perhaps callous one; but Mr. Syme quotes no case in which this plan has been practised. We have been thus particular in showing what is the treatment of fungus of the testicle recommended in recent works, because both in this article and in the journal in which it originally appeared, pretensions have been set up for an unimportant modification of the practice commonly adopted, as a valuable improvement in surgery, to which we have no hesitation in saying that the plan of treatment suggested has no just claim.

In Article XVI, Mr. Syme describes a case of bursal swelling of the wrist and palm of the hand, in which he divided the annular ligament, and gave vent to a quantity of glairy fluid, with many small, flat, cartilaginous-looking bodies. The cavity was filled with dry lint, supported by a bandage moderately compressing the hand and wrist. Not the slightest disagreeable symptom followed the operation. Since the occurrence of this case, similar operations have been performed three times by the author, and once by Dr. Duncan, with perfect success. Mr. Syme states:

> "It may therefore be considered as affording safe, speedy, and effectual relief, from a disease rendering the hand affected nearly, if not quite, useless, and, under the influence of remedial means previously in use, nearly, if not quite, incurable." (p. 214.)

We must confess that we should have hesitated to perform such an operation from apprehension of serious consequences, but none appear to have followed the incision in the cases here related.

Our readers are no doubt aware that the subcutaneous operation has been applied to the removal of cartilaginous bodies from the knee-joint, by puncturing the skin with a narrow-bladed instrument, freely dividing the synovial membrane, and then forcing the cartilage into the surrounding areolar tissue, from which it may be subsequently dislodged by a simple incision through the skin. This operation has been practised with success by Mr. Syme and Mr. Liston. The former claims the credit of having been the first to perform this operation, but admits that a similar proposal emanated about the same time from a French provincial surgeon, M. Goyraud of Aix. It appears that in a case treated by Mr. Syme last winter, finding it impossible to effect dislodgement of the cartilage, on account of its peculiar form, he applied a bandage, and enjoined perfect rest, with the effect of obtaining an adhesion of the cartilage at the wound, and complete relief from the annoyance produced by the moveable body. This is a hint worth remembering in a similar case of difficulty.

Mr. Syme gives an account, in Article XXIV, of a case in which he disarticulated the clavicle from the sternum. This case does not appear to have been published elsewhere. He states that "the only part of the

operation in the slightest degree difficult or embarrassing, was in separating the large articular surface of the bone from its connexions, where the vicinity of the pleura and venous trunk rendered extreme caution requisite." The bone was sawn through at about an inch from its acromial extremity. It seems that very little deformity resulted from the operation, and that the patient had a strong and useful arm. Mr. Syme gives us no description of the particular disease for which this operation was performed. All he states is, "at the advanced stage when it came under my observation, the case seemed free from any obscurity, either as to its nature, or the course requisite for its remedy. There was obviously a morbid growth affecting the bone throughout its whole thickness, and admitting of removal only by excision of the clavicle." We should have liked to know the nature of this morbid growth, and to have been fully satisfied of the necessity for this operation. Mr. Syme leaves his readers to conjecture even the size of the osseous tumour.

We find little in the remaining papers to interest or detain us. In bringing our review to a conclusion, we must express our sincere regret that we have been unable to bestow unqualified praise on this book. Scarcely had it issued from the press, when Mr. Syme was appointed to fill the important office in this metropolis held by the late Mr. Liston, and we gladly give him a stranger's welcome. We believe him to be eminently qualified for the duties he has undertaken, and fully to deserve the high reputation he has gained in the North, as a practical surgeon, enterprising operator, and clinical teacher. The work before us contains evidences of these qualifications—details of cases ably treated, and of operations skilfully performed. But it also contains a good deal which, though possessing interest when originally published, has now become common-place; pathological observations without novelty, or on subjects well understood, and cases too unimportant to deserve republication. But what we feel chiefly bound to notice, is the egotistical tone of the writer, and his almost contemptuous reference to the opinions and practice of his cotemporaries. Mr. Syme never loses an opportunity of telling us that he was the first to suggest some new plan of treatment, or to carry into execution some new operation, and he sometimes gives to trifling modifications of treatment, or novelties in practice, all the consequence of grand original discoveries, or valuable improvements in surgery. Mr. Syme must excuse our reminding him that, in this metropolis at least, there is no backwardness in acknowledging real worth, and in according ample credit for any additions made to professional knowledge; and that the recognition of such claims may be safely left in the hands of his medical brethren. In proof of this, we need only refer to the generous appreciation by the profession, of the talents and services of the late Mr. Liston.

ART. III.

Researches on the Motion of the Juices in the Animal Body. By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen. Translated by WILLIAM GREGORY, M.D., Professor of Chemistry in the University of Edinburgh.—*London*, 1848. 8vo, pp. 109.

HAD this work proceeded from an unknown author, we should have passed it by with the simple remark, that the writer, before putting himself forward as an instructor of the scientific public, should have made himself acquainted with the existing state of knowledge on the subject he undertook to discuss, in order to avoid the unpleasantness of being told that nearly everything which he had done had been done better by his predecessors; and that he should have taken the opinion of some abler judge than himself, whether the really novel portion of his work would bear a searching examination, and merited the estimate attached to it by its propounder. To dismiss in this summary manner, however, a treatise of Professor Liebig's,—the latest product of the Giessen Laboratory,—would probably stamp us, in the minds of most of our readers, as guilty of a piece of insufferable presumption; and we must therefore occupy their time and our own, less profitably than we could wish, in the demonstration that, however strange it may seem, the work before us is really unworthy of their attention. We feel the more called upon to do this, because the publication of this treatise, whilst the article on the Present State of Chemical Philosophy in our last Number was passing out of the hands of the printer into those of our readers, furnishes an earlier and fuller justification than we could have anticipated, of the strong statements which we ventured to make as a needful caution against the too ready reception of Liebigian dicta as constituting the Evangel of Chemistry.

We shall undertake, then, to prove in regard to this treatise, that what is true is by no means new; and that what is new is not true. And if we should not succeed to the satisfaction of our readers in effecting this, we recommend them to go to the book itself, to master its contents, and then to turn to certain other treatises which we shall name, and candidly judge for themselves whether these do not contain—not vague anticipations—but clear and explicit statements, of every principle of real importance enunciated by Professor Liebig.

In order to understand the state of the author's mind when he entered upon the inquiry, it is necessary to revert to a passage in the Third Edition of his 'Animal Chemistry;' the promised early completion of which, be it observed, has been delayed for the prosecution of the investigations upon the juice of flesh, and upon the motion of the juices, which have given us two new books instead of the conclusion of the one already commenced. At p. 165 of that treatise, we have, in the midst of a series of sneering remarks, headed "Erroneous Views of Modern Physiologists," the following passage:

"The commonest phenomena even yet are personified in the minds of many physiologists as peculiar powers, as properties, which they are tempted to explain by peculiar causes, differing from other known causes. Thus the restoration of equilibrium between two fluids of different nature, or two solutions of dissimilar

substances, separated by an animal membrane, has obtained the names of endosmose and exosmose; and men regard these names as if they were independent things, while the phenomenon is nothing else than a filtration, different from the ordinary one only in so far as that the passage is effected by an attraction (a drawing, an affinity) instead of by pressure."

Now we shall not stop to comment upon the singular confusion of terms displayed in this accusation,—physiologists being said to regard a *phenomenon* as itself a *power* or a *property;* just as if Professor Liebig were to say that the fall of a stone to the earth, is itself the power of gravity, or the property of gravitation, instead of being the operation of the power, the manifestation of the property;—but shall simply, on the part of physiologists, in this country at least, deny that they have ever regarded the phenomenon in question in any other light than as a peculiar manifestation of physical forces, the combined result of which was conveniently expressed by terms simply indicative of the fact, *endosmose* meaning inward flow, and *exosmose* outward flow. From the time when it was proved that an *inorganic* septum would serve as the condition of the action, as well as an animal or vegetable membrane, it became evident that its explanation must be sought in *physical* principles alone; and the physiologist has accordingly left it to the physical philosopher to unravel this somewhat complex case of capillary attraction, contenting himself with laying hold of the general facts of the case, and with showing that the process has an intimate relation with numerous operations taking place in the living body. Thus, to say that the absorption of liquids from the alimentary canal into the blood, is an operation of endosmose,—or that the blood-corpuscles treated with water are distended, or if treated with syrup, collapse, by endosmotic agency,—has of late, at any rate, been understood simply to mean, that these phenomena take place in virtue of that peculiar play of physical forces, of which the term in question is a convenient and appropriate designation. What else, we would ask, is "an attraction, a drawing, an affinity," than a hypothetical mode of expressing a fact?—and why is the physiologist exclusively to be charged with personifying properties, when the chemist is constantly doing the same? In the present stage of inquiry in every department of science, it is found almost indispensable to employ fictions of this description; and when we abandon one, it is almost invariably to embrace another. The Newtonian laws of motion and gravitation become fictitious personifications,—in no respect superior to the cycles and epicycles of the Ptolemaic astronomy, save in their superior simplicity,—the moment that they are treated as *entities*, or are viewed in any other light than as general expressions of phenomena, themselves perhaps to be included in some higher generalization.

There is an old proverb, that "those who live in glass houses should not throw stones." We recommend this wise saw, which doubtless has its equivalent in the German language, to Professor Liebig's attentive consideration; and proceed, without further delay, to the examination of the work before us, from the Preface of which we shall quote so much as will give the author's own account of the objects of his investigations, and of the degree in which he believes himself to have attained them.

"The present little work contains a series of experiments, the object of which is to ascertain the law according to which the mixture of two liquids, separated by a membrane, takes place. The reader will, I trust, perceive in these researches

an effort to attain, experimentally, to a more exact expression of the conditions under which the apparatus of the circulation acquires all the properties of an apparatus of absorption.

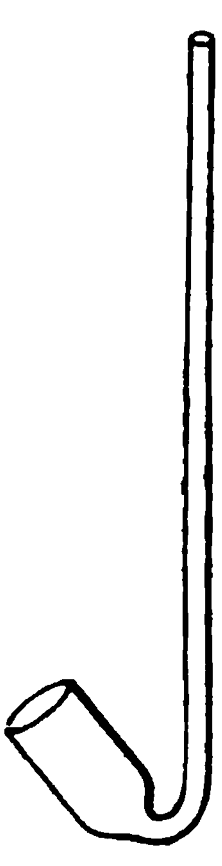
"In the course of this investigation, the more intimate study of the phenomena of endosmosis impressed on me the conviction that, in the organism of many classes of animals, causes of the motion of the juices were in operation, far more powerful than that to which the name of endosmosis has been given.

"The passage of the digested food through the membranes of the intestinal canal, and its entrance into the blood; the passage of the nutrient fluid outwards from the blood-vessels, and its motion towards the parts where its constituents acquire vital properties,—these two fundamental phenomena of organic life cannot be explained by a simple law of mixture.

"The experiments described in the following pages, will, perhaps, be found to justify the conviction that these organic movements depend on the transpiration and on the atmospheric pressure.

"The importance of the transpiration for the normal vital process, has indeed been acknowledged by physicians ever since medicine has had an existence; but the law of the dependance of the state of health on the quality of the atmosphere, on its barometric pressure, and its hygrometric condition, has been hitherto but little investigated." (pp. ix-x.)

The first sixty pages of the treatise are occupied by the details of experiments upon the phenomena of the intermixture of two dissimilar liquids separated by a membrane. It would be doing injustice to Professor Liebig, were we not to state that many of these experiments are very ingeniously devised, and serve as excellent elucidations of the nature of the process in question. But, as we shall presently show, they lead us to no new result whatever. In our progress through this part of the treatise, we find ourselves constantly at a loss, from the almost total absence of reference to the previous labours of others in the same field, to determine what is regarded by the author himself as the special novelty of his views; but when, on coming to its termination, we find him enunciating, as the results of his own investigations, conclusions which he seems to regard as peculiar to himself, we can adopt no other course than to consider the *whole* train of inquiry as in the author's opinion, an advance upon the previous state of knowledge of the subject; and in our examination of it, therefore, we shall commence *ab initio.*



The most familiar phenomena of capillary attraction are first cited; with the view of showing the existence of an attraction between the particles of liquids, and the solid walls of capillary tubes, and for the sake of proving that this attraction varies in degree in the case of different substances—facts with which the merest smatterer in physics is well acquainted. A new method of demonstrating the fact, however, is contrived by Professor Liebig; which we cite as being in itself ingenious, and as capable of being turned to useful account. He employs a tube shaped as in the accompanying figure, with a piece of bladder tied over the wide end of it; and having filled the dilated portion with water, brine, oil, alcohol, or other liquids, he measures, by means of the height of a column of mercury in the long narrow portion of the tube, the pressure requisite to force these several liquids through the membrane. The following are some of the results:

"Through ox-bladder, 1-120th of an inch thick, water flows under

a pressure of 12 inches of mercury; a saturated solution of sea-salt requires from 18 to 20 inches; and oil (marrow oil) only flows out under a pressure of 34 inches of mercury.

"When the membrane used is the peritoneum of the ox, 1-240th of an inch in thickness, water is forced through it by 8 to 10 inches, brine by 12 to 16 inches, oil by 22 to 24 inches, and alcohol by 36 to 40 inches of mercury." (p. 7.)

The explanation given of these differences is nothing novel; being simply to the effect, that the membranes have different absorbent powers for different liquids; a fact which everybody knew before. The corrugation of organized tissues when treated with alcohol, and the shrinking of meat when salted, are familiar examples of the fact that these substances would less readily take up alcohol or brine than they would water; and the method long in use for concentrating alcohol, the inclosure of it in a bladder, has always been regarded as a "pregnant instance" of the same general fact. That we do not speak "without book" on this point, will be evident from a passage which we shall extract from the 'Treatise on the Forces which produce the Organization of Plants,' published by Professor Draper (of New York) in 1844.

"The rise or depression of a liquid in a capillary tube is determined by its quality of wetting or not wetting the surface of that tube. Of two liquids in a given tube, that will rise highest which will wet the tube most perfectly. And, therefore, we can see in these movements, that if two liquids be placed on opposite sides of a porous septum, or at the opposite ends of a capillary tube, which is wetted by one more perfectly than by the other, that one which exerts the most energetic action will flow fastest. If a piece of bladder be soaked in water and in alcohol, it will readily be seen that the former acts more powerfully on it, giving it greater flexibility and translucency, and having a stronger affinity for its tissues. For these reasons, if a mixture of water and alcohol be tied up in a bladder, as is well known, the water will soak out and evaporate away, but the alcohol will be retained. And for the very same reason, when these two liquids are placed on opposite sides of such a porous body, the water moves fastest through it, as in the experiment of Dutrochet." (p. 28.)

We have quoted from Professor Draper, simply because (as we shall presently show) he has applied these physical principles to the explanation of *physiological* phenomena far more fully and satisfactorily than Professor Liebig has done. We might have gone backwards, however, by several years; and have quoted from Professor Daniell's most valuable 'Introduction to the Study of Chemical Philosophy,'—a treatise with which we should have supposed Professor Liebig to be well acquainted,—a lucid demonstration of the same fundamental facts, which are treated of by him under the head of "Heterogeneous Attraction." In regard to the explanation of the *physical* conditions under which these phenomena take place, we cannot find anything in Professor Liebig's pages which adds to the knowledge of the subject that we had derived from Professor Daniell.

Having rung the changes at some length upon the variety of absorbing power possessed by animal membranes, &c. for different liquids, Professor Liebig then goes on to examine the causes of that change of bulk, which usually occurs when the two liquids intermix through a porous membrane, and which is generally one of the most obvious indications that the processes of endosmose and exosmose are taking place. We do not think, however, that, in the ordinary acceptation of these terms, a change of bulk is regarded as an essential portion of the phenomenon; the

mutual penetration and intermixture of the liquids through the porous septum being that in which it fundamentally consists. It is scarcely necessary for us to conduct our readers through the succeeding forty pages of the treatise, in which the physical conditions of this process are examined; since we think that they will be satisfied with the general result; and, if they are not, they can easily gratify their curiosity by turning to the book itself. The general conclusion from the author's experiments (many of which are so ingenious and illustrative as to afford full confirmation to the truth of the view, if any had been wanting) is thus expressed.

"From what has been stated, it appears that the change of volume of two miscible liquids, separated by a membrane, is determined by the unequal capacity of being moistened, or the unequal attraction of the membrane for these liquids. The unequal absorbent power of the membrane for these liquids depends on the dissimilar nature of the liquids or of the substances dissolved in them." (p. 56.)

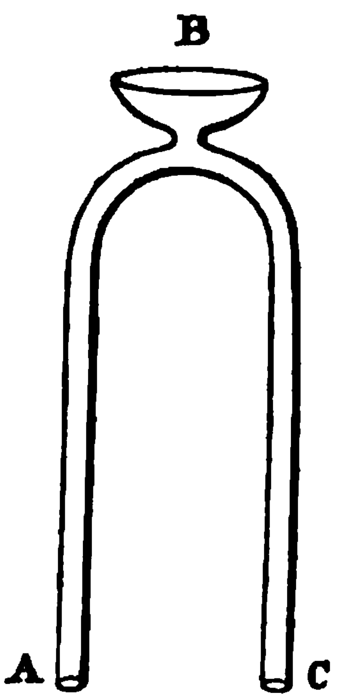
Now in what, we would ask, does this conclusion differ from that already stated by Professor Draper, and before him by Professor Daniell? We subjoin the enunciation of it by the latter, as contained in the treatise already cited.

"We have already noticed the *statical* adjustment of the forces of homogeneous and heterogeneous attraction between a solid and a liquid, in the case of capillary action; a curious and highly important *dynamical* effect results from the operation of the same forces in what has been denominated the process of *endosmose* and *exosmose;* in which the mutual attraction of two liquids is called into action, one of which is more capable than the other of freely wetting a porous solid which forms part of the combination.

"If we dip a piece of bladder, or other animal membrane, into water, it will be wetted, and completely soaked with the liquid; but if we dip it into spirits of wine, it will superficially adhere to it, but will not be imbibed into its pores, and may be wiped off without difficulty. Take a funnel, and tie over its broad end, of three or four inches diameter, a piece of bladder; invert it, and fill it with spirits of wine, and fit to its small end a glass tube three or four feet in length, and then place it upon a perforated tin plate in a vessel of water; in a short time the liquid will be observed to rise in the tube; and, notwithstanding the accumulating pressure, which will be the same on the bladder as on that of a column of the liquid of equal base, it will ultimately reach the top and flow over. The first moving power is here the force of adhesion between the water and the bladder; the former penetrates the pores of the latter, and comes in contact, upon its upper surface, with the spirit, by the heterogeneous attraction of which again it is removed, and mixes with its mass. The height of the column is in some degree the measure of the force thus called into action, which, in the case under consideration, is also opposed by the difference of the specific gravities of the water and spirit. Other liquids may be substituted for the spirit, provided only there be a strong disposition in them to mix with the water; and even strong solutions of solids, as sugar, gum, &c., will determine the action of *endosmose* by their still predominant attraction for more water. The organic texture of the membrane is not necessary to the process, which will equally take place through thin plates of clay, slate, and other porous substances. The *endosmose* (or flowing in) of the exterior liquid is generally accompanied by the *exosmose* (or flowing out) of the interior liquid, but to a much less extent; the difference depending upon their greater or less attraction for the interposed solid, or their capability of wetting it. Modifications of this process are of the highest importance in many of the operations of the organic portion of the creation, and the study of them promises to throw light upon many obscure points of animal and vegetable physiology." (*Introduction to the Study of Chemical Philosophy,* pp. 72-3.)

It is not a little curious that, in the course of his inquiries, Professor Liebig should have attained a glimpse of an application of these general facts to the explanation of physiological phenomena, which has been fully and (to our minds) satisfactorily made by Professor Draper in the treatise from which we have already quoted; and that, instead of following out this far more promising line of investigation, he should have been seduced into one that has conducted him into a quagmire of error and absurdity. In page 33 of Liebig's treatise, we find it stated that the mixture of two different liquids is the result of a chemical attraction; and in page 48, the moistening of solid bodies by liquids is ascribed to the same cause. In these views he is anticipated by Professor Draper; who, throughout his exposition of the subject, speaks of the wetting of a solid by a liquid as the result of chemical affinity; whilst Professor Daniell took what we believe to be the more philosophical view of the case, in referring these phenomena to that attraction between the particles of dissimilar substances, which is *one of* the agencies concerned in the production of the phenomena usually regarded as results of Chemical affinity. Such being the case, continues Professor Liebig, "It follows that when a porous body is saturated with a liquid, and brought in contact with a second liquid, which has a stronger attraction for its substance than the first has, then the first liquid must be displaced from the pores by the second, even in the absence of hydrostatic pressure, and this, whether the two liquids be miscible or not." (p. 48.) Now let our readers compare this passage with the following, which we shall cite from Professor Draper. "When two different liquids are brought in contact with a porous solid, which is wetted by both, but by them unequally, that one which has the greatest affinity for the solid, or which wets it most perfectly, will pass most rapidly through it, and may even drive the other entirely before it." (Op. cit. p. 28.) If Professor Liebig or any of his partisans will show us any essential difference between these two statements, we shall be most glad to be informed wherein it lies; for ourselves we can see none, except in the superior applicability of Professor Draper's *formula* (for such it may be designated) to the analysis of physiological phenomena. The application was made by Professor Draper himself, in no hesitating or uncertain manner, but with a clearness and precision which shows that he has really understood what Professor Liebig has only thought he understood. The portion of his treatise immediately succeeding the extracts we have made, is occupied with an examination of the phenomena of the circulation in plants and animals, and with a masterly demonstration of the existence of a physical force in their capillaries, adequate to assist and modify the flow of nutritious fluid through them; thus supplying the explanation of those residual phenomena, which had been perceived by most physiologists of sagacity and comprehensive knowledge of their science, to be inexplicable by any *vis a tergo* communicated to the current from the heart and arteries. We shall give a brief condensation of these views, notwithstanding that they have been already presented to one portion of our readers in the pages of the 'British and Foreign Medical Review' (Vol. XX, p. 157), and have been incorporated in some of the physiological treatises in most general use in this country; because we think it right to bring a valid and philosophical application of physics to physiology into striking contrast

with one which is alike crude and inappropriate. We shall confine our observations, for the sake of brevity and simplicity, to the question of the circulation in animals. From the principle already laid down by Professor Draper, it follows, that if the two ends of a capillary tube be in connexion, the one with arterial and the other with venous blood, whichever of these liquids has the greater attraction for the walls of the capillary tube, will enter it and displace the other, supposing the latter to be already there. But the tube being thus saturated with the liquid for which it has the preference, no further motion would ensue, if it were not that, in its course through the tube, the liquid undergoes a change which reverses its affinity for the surrounding walls. Thus, to take the simpler



case first, the venous blood which enters the pulmonary capillaries is conveyed by them over the walls of the air-cells, and in them undergoes a change by which it is converted into arterial. We have a right to assume, therefore, that each particle of venous blood has an affinity for that portion of the pulmonary capillary, through the medium of which its conversion into arterial blood takes place; and that, as soon as the change has been effected, this affinity ceases. Upon the foregoing principle, then, it will be evident that, if the tube ABC represent a pulmonary capillary, A being its connexion with the pulmonary artery, B being the part of it traversing the wall of an air-cell, and C its connexion with the pulmonary vein, there will be a tendency to a constant flow from A to C; since the venous blood entering at A is attracted towards B; but having there undergone a change in its character, that attraction no longer exists, and it is consequently pushed onwards towards C by the new current which will be continually entering at A.* In the systemic circulation, the reverse conditions obtain, and the reverse result is consequently produced. The most important changes which the blood undergoes in the tissues, are of the opposite kind to those which take place in the lungs, oxygen being given off and carbonic acid being taken in; and although there are others more particularly concerned in nutrition and secretion, yet those we have cited may be considered as types of the rest, more especially as the rate of the circulation has special reference to the rapidity with which it is requisite that these should take place. Here it is the *arterial* blood which has the stronger affinity for the walls of the capillaries, or at least for certain points of them; and this fluid is consequently drawn into the tube at the extremity A, with a force sufficient to push on the column which it contained. On passing the point B, however, its character undergoes a change; it becomes venous, thereby losing its affinity for the wall of the tube; and is in its turn pushed on towards C by the constantly incoming current of arterial blood, which is itself as constantly altered in its properties on passing the point B, and is thus progressively dis-

* The phenomena of asphyxia demonstrate the necessity of this change for the passage of the blood through the pulmonary capillaries; since, if it be impeded or prevented by the want of oxygen or the presence of carbonic acid in the air-cells of the lungs, the action of the heart is insufficient to maintain the onward flow.

placed by the stream behind. In this manner, a satisfactory *vera causa* is assigned for the phenomenon in question; for this explanation applies as well to the pulmonary circulation, in which the venous blood goes first, as to the systemic in which it is arterial blood that enters the capillary tubes; and it fully harmonizes with the general principle which had been deduced by physiologists from their observation of the phenomena of the circulation,—that the heart's action remaining the same, the rate of flow of blood through different parts of the body depends upon the relative activity of the changes taking place between the blood and the tissues in those parts respectively.

The whole of this beautiful application of the laws regulating the motion of fluids in capillary tubes, is left unnoticed by Professor Liebig; let us now examine whether he presents us with anything as good in its stead. At pages 58 and 59, we find the first indication of the physiological bearing of his investigations, in the heading "Causes of absorption of other animal fluids into the blood." After a repeated and careful examination of the paragraphs to which this heading refers, we can find no indication of any other force as concerned in the phenomena of absorption, than that which has been customarily designated under the name of endosmose. It is pointed out that the motion of the blood will much facilitate the passage of fluids towards it; but this is no new observation, as the readers of Professor Matteucci's Lectures will doubtless remember. In Lecture IV (at page 82 of Dr. Pereira's translation) an experimental demonstration of the fact is given, which leaves nothing more to be desired. The extent of Professor Liebig's knowledge of the physiology of Absorption is shown in the following extract:

"The whole intestinal canal is surrounded with this system of blood-vessels, and all the animal fluids, in so far as they are capable of being taken up by the parietes of the intestinal canal, and of the blood-vessels situated around it, are rapidly mixed with the blood. The volume of the blood increases, if no compensation is effected by the kidneys: and the intestine is emptied of the liquids contained in it. The intestinal glands, through which this transference is effected, and each of which represents a similar apparatus of suction, contain, within them, two systems of canals,—blood-vessels and lacteals; the blood-vessels are placed next to the external absorbent surface, the lacteals chiefly occupy the central part of the gland. The liquids circulating in these two systems have very unequal velocities; and as the blood moves much faster in the blood-vessels, we perceive how it happens that the fluids of the intestine are chiefly (in quantity and velocity) taken up into the circulation." (p. 60.)

And then we have a repetition, *totidem verbis*, of the contrast between the effects of repeated glasses of spring water, of a saline solution of the same density with the blood, and of a strong saline draught; which was first put forward by Liebig many years ago, with the view of showing—what everybody knew before—that the absorption of liquids into the sanguiferous circulation takes place according to the ordinary laws of Endosmose; and which is here again introduced, as if the readers of Liebig's works had not already experienced from its frequent repetition "a sense of repletion" analogous to that which is felt after the third dose of water containing one per cent. of sea salt.

But the physiologist will not be by any means satisfied with the explanation given by Liebig of the different character of the absorbent process in

the blood-vessels and the lacteals of what he terms the "intestinal glands," by which he means, we perceive, the *villi*. If the difference were merely in *rate*, what would be the use of the lacteal system at all? Its place might be far more profitably occupied by blood-vessels. But our readers know full well that this is not the case. In ascending the animal series, as soon as we meet with proper *red blood*, so soon we have a special absorbent system; the blood up to that point having partaken of the properties of the chyle and lymph. Repeated experiments have shown that any substance that can be *dissolved* in water will pass into the mesenteric blood-vessels, but that these substances rarely pass into the lacteals; whilst, on the other hand, the nature of the fluid in the lacteals is found to be remarkably uniform, and the solid matters which enter into its composition (chiefly of a fatty nature) are such as had not all been in a state of solution in the intestinal liquids. This distinction, then, long since pointed to a peculiar power of *selection* as exercised by the lacteals; and this power is now attributed by physiologists, on grounds furnished by that microscopic observation, which Professor Liebig has *ignored* ever since he was worsted in the matter of yeast, to the *vital* process of cell-growth. If a *physical* explanation is to be found for it, assuredly this is not furnished in the work before us.

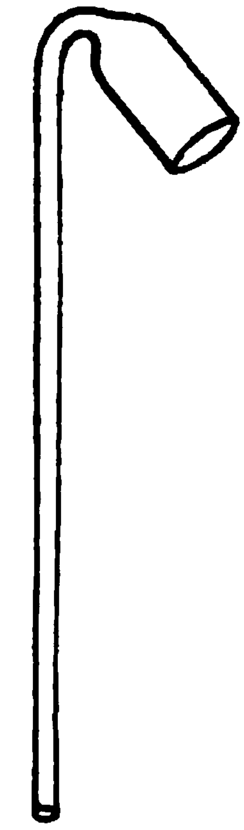
We can scarcely suppose that our readers will find anything new in the way of information or suggestion in the following passage, which is all that we can find relating to other functions in this section of the work.

"Since the chemical nature and the mechanical character of membranes and skins exert the greatest influence on the distribution of the fluids in the animal body, the relations of each membrane presenting any peculiarity of structure, or of the different glands and systems of vessels, deserve to be investigated by careful experiment; and it might very likely be found that in the secretion of the milk, the bile, the urine, the sweat, &c., the membranes and cell-walls play a far more important part than we are inclined to ascribe to them; that besides their physical properties, they possess certain chemical properties, by which they are enabled to produce decompositions and combinations, true analyses; and if this were ascertained, the influence of chemical agents, of remedies, and of poisons on those properties, would be at once explained." (p. 63.)

We should like to hear of any modern physiologist, who ever thought of looking elsewhere than in "the membranes and cell-walls," except perhaps in the cell-nuclei, for the forces concerned in the phenomena in question. And not only has the importance of the investigation here suggested been fully perceived by Professor Matteucci, but the experimental inquiry has already been prosecuted by him to no inconsiderable extent (See Lecture III). We find it difficult to account for Professor Liebig's extraordinary display of ignorance in regard to a book of which almost every chemist and physicist in Europe must have heard; and we cannot help suspecting that it is rather the result of design than of accident. In either case, however, it is unworthy of his position as an expositor of science.

Perhaps we have too long kept our readers from the *bonne bouche* furnished by what Professor Liebig seems to regard as the notable discovery, worthy of a whole book to be occupied in setting it forth; namely, the "Influence of the cutaneous evaporation on the motion of the fluids of the animal body." In order to demonstrate this influence, he uses a tube resembling that which we have already figured, but placed in an

inverse position. The tube being filled with water, its lower end is immersed in mercury; and as the bladder allows a gradual transudation of the water, which is constantly evaporating from its surface, the quantity of water in the tube undergoes a progressive diminution, and, as a necessary consequence, the mercury rises to replace it, being forced up by atmospheric pressure. This elevation, however, has a limit; since when the column stands at a certain height, air tends to enter the tube through the bladder, and thus checks its further rise. Several other experiments are detailed, but they are only variations of this one; and we may take as the basis of our remarks Professor Liebig's own statement of their general results; namely, "that all liquids which are in connexion with a membrane, from the surface of which evaporation can take place, must acquire motion towards that membrane." (p. 73.) Surely the scientific world has not been in a state of such puerile ignorance, as to require that a great philosopher should write from Giessen, to instruct it in a fact so simple and familiar. In regard to the particular case on which so much stress is laid by Professor Liebig, we venture to affirm (having, indeed, tried this *mental* experiment for ourselves) that any child, acquainted—like Miss Edgeworth's "Harry,"—with the principle of the barometer, and with the mode in which fluid is carried off by evaporation, would be able to predict the result of the arrangement just described, in so far as the rise of the column of mercury is concerned. We can find nothing new, then, as respects the physical principle, in Professor Liebig's magniloquent announcement.



But it will, perhaps, be affirmed that although the principle is not new, it is now for the first time applied to physiology; and that, as in the case of Columbus's egg, everybody will at once recognise the application as true, and will wonder that it had not been made before. We must, then, pursue the inquiry further. At p. 77 we find the following passage:

"It is hardly necessary to remind the reader that the experiments described in the foregoing pages, in so far as they permit us to draw conclusions as to the cause of the motion of the juices of the animal body, agree in all respects with the observations made on plants by Stephen Hales more than 120 years since."

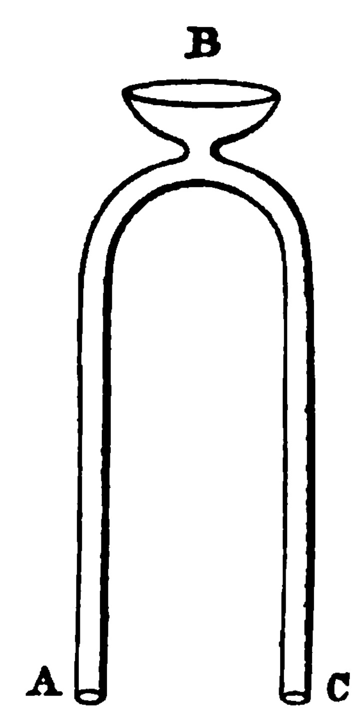
And then, as if the results of these experiments were not sufficiently well known, several pages are occupied in the quotation of them. Now we believe that every writer on vegetable physiology, from that time to this, has most fully recognised the exhalation from the leaves of plants as the chief condition of the rise of the sap in the stem and branches,—has pointed out that when the exhalation is checked, the rise of the sap ceases,—and has shown that if the exhalation be artificially stimulated, an unusual rise of sap occurs. Moreover, a circumstance is well known to vegetable physiologists, of which Professor Liebig takes no account whatever; namely, that the amount of exhalation from the leaves of a living plant is very different from that which takes place from the surface of a dead one, the loss of fluid being far greater than mere *evaporation* will account for. And whatever be the cause of this difference, it is one which accounts for the rise of the sap in the branches of the living tree, as compared with its stagnation in the dead,—a point on which Professor

Liebig is entirely silent. If the cause of the rise of the sap be to be looked for altogether in the *evaporation* from the leaves, why should not the process go on just as well in a plant killed by an electric shock, or by immersion in an atmosphere of poisonous gas, as in the living structure? Before Professor Liebig next writes upon these questions, we would recommend him to make himself acquainted with the phenomena which he purposes to elucidate, and thus to spare the physiologist the trouble of showing that he knows next to nothing about them. Even his translator and puffer, Dr. Gregory, thinks it necessary to enter a *caveat* on this head; for he remarks (Editor's Preface, p. vi) that "it is equally obvious that the above-mentioned mechanical and chemical causes are not alone sufficient to explain the phenomena of animal life, since they are present equally in a dead and in a living body; so that while every advance in physiology enables us to explain more facts on chemical and mechanical principles, something always remains, which, for the present, is beyond our reach, and which may for ever remain so." This "something" that "always remains," happens, in the case before us, to be by far the most important part of the process; for the simple *evaporation* from the surface of almost any plant, which is of course equal for the dead and the living if the moisture of the surface and other circumstances be the same, is as nothing to the *exhalation* which takes place from the leaves of a *living* plant under favorable circumstances.

Several other points are raised by Professor Liebig in regard to the ascent of the sap in plants, which tend rather to distract attention from his main argument than to elucidate it; and we shall therefore next examine his application of the foregoing principle to the animal circulation. This application simply consists in the assertion (p. 74) that "the fluids of the body, in consequence of the cutaneous and pulmonary transpiration, acquire a motion towards the skin and lungs, which must be accelerated by the circulation of the blood." Now, in this statement, the difference between the movement of fluids in plants and animals, as also between the relative amounts of transpiration from their external or internal surfaces, is altogether lost sight of; and these differences, as we shall now show, so materially affect the conditions of the phenomenon, as to render the explanation in question altogether inapplicable. The experiment with the curved tube already quoted, sufficiently well elucidates the causes of the movement of sap towards the leaves of plants; because, of the entire ascending fluid, by far the greater part is lost by exhalation from those organs. In the experiments of Dr. Woodward (Phil. Trans. 1699) on four plants of spearmint, grown with their roots in water, the total amount of liquid taken up during 56 days was 53,011 grains; and their total increase of weight during that time was only 719 grains. Of this increase, a certain part was doubtless due to the carbon and other solid matters introduced into their structure during the same time; but even making no allowance for this, and supposing that the 719 grains consisted entirely of water *fixed* in the plant, we see that the remaining 52,292 grains of water, constituting 72-73ds of the entire quantity absorbed, must have been exhaled from the leaves, since it could have passed off in in no other way. But how widely different is the condition of this function in animals. The total amount of exhalation from the skin and lungs of a man during the twenty-four hours, is considered to average a little

above 2lbs.; whilst the quantity of blood that passes through either the aorta or pulmonary artery during that period, may be calculated at about 12,600lbs.* The entire amount of blood sent through both these vessels in any given time would, of course, be double that just stated; but that we may not be accused of overlooking the fact, that only a small proportion of the blood propelled through the aorta is transmitted to the cutaneous surface, we will base our calculation upon the single amount transmitted through the pulmonary artery, and treat the case as if the whole of the loss of fluid by exhalation proceeded from that source only. We find that, even with such an advantage, the entire exhalation from the cutaneous and pulmonary surfaces, in any given time, is only as 2 to 12,600, or 1-6300th part of the fluid propelled through their vessels, instead of forming (as in Woodward's experiment on the spearmint) 72-73ds of the fluid brought to them. It is then to be conceived that a change so utterly insignificant can have the vastly important effect which Professor Liebig ascribes to it?

But there is another fundamental error in his explanation. The motion of fluids in the animal differs further from that in the vegetable in this important particular,—that the former is a real *circulation*, the current returning into itself; whilst the latter, as regards the ascending sap, is a mere flux from one point towards another, without any corresponding reflux,—the movement of the descending or elaborated sap being altogether independent of that of the ascending. An experiment which is apposite to the former, therefore, may be altogether inappropriate as regards the latter; and this, we think, will prove to be the case in the present instance. For let us again employ our own form of tube, and test by its means this supposed influence of the exhalation upon the pulmonary and cutaneous circulation. Suppose the tube represented in the accompanying figure to have a piece of bladder tied over the wide mouth B, and then, being filled with water, to be placed with its extremities A and C immersed in mercury. What will be the result? Clearly that, as the water evaporates from the surface of the bladder, the mercury will rise in *both* legs of the tube equally. Let A B C represent a capillary tube, of which A is the arterial extremity, C the venous, and B the part from which the exhalation takes place. Now allowing to the exhalation the influence upon the motion of the fluids which Professor Liebig claims for it, we ask,—how will that influence operate? The reply is evident. The blood on the venous, as well as on the arterial, side of the capillary would be drawn towards the evaporating surface; and *the venous circulation would be retarded as much as the arterial would be accelerated* (if such a thing were possible at all), so that a state of congestion would result. The condition of a portion of skin from which the atmospheric pressure has been removed by a cupping-glass, offers, in a somewhat exaggerated aspect

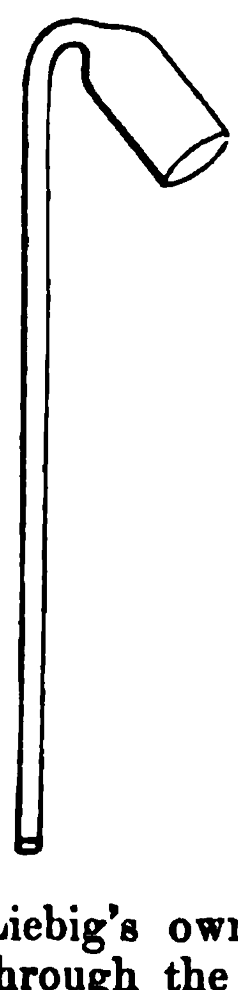


* Reckoning that 2 oz. of blood are propelled by either ventricle at each systole, and counting 70 pulsations to the minute, we have 2 × 70 × 60 × 24 = 201,600 oz., or 12,600 lbs., propelled alike through the aorta and the pulmonary artery every twenty-four hours.

perhaps, a picture of what the entire surface would be, if Professor Liebig's doctrines were true.

So much, then, for the wonderful discovery, to enunciate which this book has been given to the world.

But we have not yet done with Professor Liebig. He argues that, as the ascent of the mercury in his tube is immediately occasioned by atmospheric pressure, which forces it up to supply the place of the water that has been lost by evaporation, so atmospheric pressure is the cause, or, at least, one great cause, of all the absorption by which fluids are introduced into the circulating system of the plant or animal. This adds, we think, the climax to the absurdity of the whole speculation. For, in the first place, the substitution of *this* cause for that peculiar form of capillary attraction which we shall still venture to call by the name of endosmose, would seem to be equivalent to throwing the latter overboard altogether, and would consequently render unnecessary the whole discussion with which the first part of the volume is occupied. This, however, would be a matter of little moment, if the change were really for the better. But, in the second place, we think that no two opinions can exist in the mind of any two intelligent and candid persons, as to the utter inapplicability



of this explanation to the phenomena of absorption in animals. Let the lower part of the Liebig-tube be of India rubber instead of glass; how then will the case stand? As the water evaporates from the large extremity, the sides of the tube will collapse, and the mercury will not rise in it the fraction of an inch. Now, is the vascular system of the animal most like the rigid or the elastic tube? Surely, the latter. But the Liebigian hypothesis falls to the ground, if we do not allow that "the structure of the cutaneous surface does not permit a diminution of its volume, a compression, in consequence of the loss of liquid by evaporation;" an admission totally irreconcileable with every-day observation. Again,—if this hypothesis were correct, a limit to the absorption by the intestinal surface would be imposed by the amount of evaporation from the cutaneous and pulmonary; since, in the Liebigian experiment, no more mercury will rise in the tube than is sufficient to replace the water lost by evaporation. But is this the case? Do we not find, on Professor Liebig's own showing, that pint after pint of water may be taken up through the intestinal vessels, causing a distension of the sanguiferous system, which is only relieved by the safety-valve action of the kidney; and this, whilst the amount transpired through the skin and lungs in the same period is scarcely worth naming? This well-known fact, we submit, cannot be explained in any other way than on the principles of endosmose, which, as already shown, are quite adequate to account for it.

We are ready to grant that the explanation is more applicable to the case of the plant; for there we find that the amount absorbed is in closer relation with that exhaled; and the superior rigidity of the vegetable tubes, which may be sufficient to prevent their collapse, may allow atmospheric pressure to act in favoring absorption. But, as we have already shown, all the conditions, both of the exhaling process and of the circu-

lation, are so different in these two groups of living beings, that no argument can be legitimately drawn from the applicability of this explanation in the one case, with respect to its suitableness in the other. Still, even in the ascent of the sap, there are many phenomena for which it will not account; and we cannot admit that Professor Liebig has here done more than suggest an additional cause, which may possibly influence the operation. That simple capillary attraction, however, would cause continued ascent of fluid without the aid of atmospheric pressure, so long as its removal is being accomplished above, is clearly shown by the ascent of oil in the wick of a lamp, and numerous other phenomena; and we do not see, therefore, why an additional cause should be sought for. And the assumption required by the theory, that the tubes contained in the leaves and leaf-stalks, the sap-wood, and the root-fibres, are perfectly incompressible, would seem scarcely borne out by the *drooping* of a plant from deficiency of moisture, when the exhalation has been forced by excess of heat and light beyond its proper amount, or the roots have failed from any cause to yield the proper supply of fluid.

We shall not occupy the time, or try the patience of our readers, by a similar critical examination of the numerous minor questions which are introduced by the author; such as the cause of the death of a fish when partly kept out of water, as to which he incorrectly cites the experiments of Dr. W. F. Edwards, and attributes them to Milne-Edwards; and the notions as to the cause and treatment of the potato disease, regarding the superiority of which over the thousand-and-one previous speculations on the same subject, experience alone can decide. We have discharged our duty in examining closely, but we trust fairly and honestly, the merits of Professor Liebig's researches, and the correctness of his conclusions respecting the points for the elucidation of which these researches were undertaken. We trust that we have convinced our readers, that, however much Professor Liebig's own knowledge of the subject may have been advanced by his inquiries, they have not carried him beyond, or even up to, the stage at which others had arrived long before him. We would have him to know, that there are other philosophers in the world, besides those who own him as their Magnus Apollo; and that science does not stand still because he may not witness its progress. Whilst his chariot wheels are rolling rapidly and triumphantly in one direction,—ere long, perhaps, to return over nearly the same tracks to the starting-point,—humbler labourers are quietly pursuing their onward course in another, and, having thus vanished from his sight, are supposed by him to have departed from the pursuit, when they are in fact more advanced in it than himself. It would be absurd in us to blame Professor Liebig for his imperfect acquaintance with physiology;—just as absurd as it is in him to blame physiologists for their imperfect acquaintance with chemistry. We should all remember the old and excellent adage,—*non omnia possumus omnes;*— which, in these days of the multiplication of the sciences, is becoming more and more true. All that any one can hope to accomplish, even under the most favorable circumstances, is that, whilst he gives his chief attention to the advancement of one department, he may keep up with the general progress made by other philosophers in those which are in closest relation with it. But no good, we think, can arise from his continually quitting his own legitimate occupation, to interfere with their pursuits,

And it especially behoves him, if he should venture to assume the position of their instructor, to take good care that he really knows more about their subject than they do themselves; and more especially to guard himself from doing injustice to the labours of others in the same department. All this we consider that Professor Liebig has *not* done in the present case. He has quitted those chemical investigations which he is eminently qualified to carry on, and for the results of which the scientific world is waiting with all the readiness to receive and appreciate them that he could reasonably expect; to apply himself to the investigation of a problem which lies within the domains of the physicist and the physiologist. He has either allowed himself to remain ignorant of the elucidation which this problem had received from his predecessors in the physical part of the inquiry, especially from Professors Daniell, Draper, and Matteucci; or, being acquainted with their labours, he has wilfully ignored their results; and he has consequently published as new that which was in reality old, and has endeavoured to take to himself credit for doing that, which has been already better done by others. And in regard to the physiological applications, he has committed gross blunders, of which even a tyro might be ashamed.

Against such a system we feel called upon to raise our voice, as calculated to obstruct the progress of science instead of accelerating it, to introduce discord where there should be harmony, to sow the seeds of mischief, which will in due time bear pernicious fruit. A bad book is at any time an evil. But it is a much greater evil, when it proceeds from a man of great name. An obscure author puts forth a trashy production, and it falls still-born from the press. But everything proceeding from the pen of a Liebig, as from that of a Dickens or a Dumas, is sure of being read by numbers who have not the power of discriminating between its faults and its merits. And we therefore hold it to be the special duty of the critic to endeavour in such cases to set the public right, even if he be thereby forced into an unpleasant collision with the object of his comments. He, of all mankind, ought to be unawed by the authority of great names, when *truth* is in question; unseduced by popular admiration, when he honestly believes the object of it to be unworthy. This duty we have fulfilled to the best of our ability. We have no private pique to gratify; no party purpose to serve. We have never had any quarrel, personal or scientific, with Professor Liebig; and are not aware that he has ever thought us worthy of the epithets "dunghill cock," "old washerwoman," or others of equal elegance, which he has so courteously and liberally bestowed upon men, who, though now less worshipped than himself, may hereafter be regarded as belonging to the foremost rank of chemical philosophers. Our desire has solely been, to prevent our readers from being misled by the authority of a great name, and to suggest to Professor Liebig the propriety of a more rigid adherence to his own department of scientific inquiry, if he wishes to sustain his present reputation. With that reputation we have little disposition to quarrel. We believe that Professor Liebig has done, and we expect that he will do, more for the progress of organic chemistry, and its applications to physiology and pathology, than any other single individual of the present generation. But we consider that his success lies rather in opening up new lines of inquiry, than in himself following them out; in demolish-

ing old errors, rather than in himself establishing new truths. And we regard his numerous errors and failures as by no means the least instructive part of his course.

We shall be sorry if we should have appeared to our readers to express ourselves too decidedly and dogmatically, whilst setting ourselves up as the opponents of a man of such weight and authority as Professor Liebig. We can only say that we have endeavoured to give them the power of judging for themselves upon the chief points on which we have felt called upon to remark. We have adduced quotations sufficient, we apprehend, to establish our first position, that what is true in the book is not new; and we believe that we have also placed before our readers adequate materials for the formation of their own judgment on our second, that what is new is not true.

We shall only add, that we trust to be able speedily to welcome the completion of the Third Edition of Professor Liebig's 'Animal Chemistry;' and that we shall endeavour to enter upon its examination with minds as free from a depreciating spirit, as if the learned author had not disturbed our equanimity by forcing upon our attention the misbegotten production now before us.

ART. IV.

The Seven Books of Paulus Ægineta: translated from the Greek: with a Commentary, embracing a complete View of the Knowledge possessed by the Greeks, Romans, and Arabians, on all Subjects connected with Medicine and Surgery. By FRANCIS ADAMS.—*London, printed for the Sydenham Society.* 3 vols. 8vo. Vol. I. 1844. pp. xx. 683; Vol. II. 1846. pp. 511; Vol. III. 1847. pp. 653.

ONE of our predecessors intimated his intention of not reviewing the works published by the Sydenham Society, "for these among other reasons: the books, in the first place, are not published for sale, but for distribution among the members of the Society; and secondly, they will obtain so very wide a circulation in their destined channel, that it seems superfluous to give any detailed account of them in the journals." (British and Foreign Medical Review, vol. xvii. p. 522.) At the same time, "lest it should be supposed that total silence on his part might imply indifference to the interests of the Society, or disapproval of the works published by it, he thought it necessary to say a few words respecting" (vol. xviii. p. 526.) each of the first nine volumes published by the Society.—As we confess we were never convinced by our excellent predecessor's reasons in this matter, so we have no intention of imitating his example: for in the first place, though it is very seldom that a work printed for private circulation among a limited number of friends can be considered a legitimate object of criticism in the public journals, yet the case of a Society consisting of two thousand members appears to us to be widely different; and secondly, though it is undoubtedly one part of a reviewer's office to bring before the notice of the public, works of merit that have but a limited circulation, yet we conceive that there are other and more important duties which he has to perform. Accordingly, the works issued by the Sydenham Society will receive the same treatment at

our hands as those published by any common bookseller: those which are of sufficient importance we shall notice, and those of less interest we shall pass over in silence; those that are well executed we shall commend with pleasure, and those which exhibit marks of haste, carelessness, ignorance, or incompetency on the part of the editors, we shall consider ourselves bound to censure, with candour and fairness.

We will begin our notice of the Sydenham Society's publications with certainly one of the most remarkable, viz. Dr. Adams's translation of Paulus Ægineta. We use this epithet advisedly; first, because it is the most important work *of its kind* that has appeared in Great Britain for more than a century; and secondly, because of the notoriety it has attained,—for we all remember the ridicule heaped upon it, and the very general outcry raised against it at the appearance of the first volume. Part of this outcry (perhaps also part of the ridicule,) would probably have been excited among some of the members of the Sydenham Society by the publication of *any* work of *any* of the ancient physicians; for undoubtedly to most readers these authors would belong to the ornamental, rather than the useful portion of their library; and it must be confessed that they contain no lack of unintelligible blunders, any one of which would be sufficient to ensure the *plucking* of a medical student in the present day. Into an examination of the merits and utility of ancient literature in general, or even of ancient *medical* literature, it is not now our intention to enter, inasmuch as the whole question has been frequently discussed; and the gradually increasing interest felt in the subject of medical antiquities, both in this country and on the continent, is a sufficient indication of the result to which public opinion is inclining. But, with reference to this particular work, it seems advisable to say a few words on the conduct of the Council of the Sydenham Society in publishing it, and on the dissatisfaction with which it was received.

The circumstances attending the publication of this work were (we believe,) as follows. Dr. (then Mr.) Adams published the first volume of his translation of Paulus Ægineta in 1834. It did not excite much attention in this country, but immediately gained the translator a very high reputation among those persons who took an interest in medical antiquities, and was favorably noticed on the Continent, both in one or two periodical publications, and also in Choulant's 'Handbuch der Bücherkunde für die Aeltere Medicin,' which is known to be a work of the highest authority on the subject. He was deterred from proceeding with the publication by the pecuniary loss entailed upon him, arising in a great measure from the failure of his London bookseller, though the whole of the manuscript was finished and prepared for the press. When this circumstance was brought before the notice of the Council of the Sydenham Society, it seemed a pity that so learned and laborious a work should be lost; and accordingly the proposition was made to Dr. Adams "to prepare a new edition of the volume already published, and to complete the remaining two on the same general plan." (Advertisement.) We think the Council were quite right in acting as they did, and that they do not deserve the severe censures that have been cast upon them for their conduct; but we confess we should have recommended a little delay in the publication, and should not have chosen such an author as Paulus Ægineta to be the first ancient medical writer published by the Society. For,

let us consider for a moment the effect it was likely to produce on the great body of the subscribers, and whether it was calculated to render the study of the ancient physicians more popular among the medical practitioners of this country. The work would come before them without any of the prestige of name and reputation; for, while every one is familiar with at least the names of Hippocrates, Celsus, Galen, Dioscorides, and Aretæus, probably many persons had never before even heard of Paulus Ægineta;—nor in fact need any one be ashamed of confessing his ignorance. The work, therefore, would have to stand or fall entirely upon its own merits, and its power of interesting its readers. Dr. Adams selected it for the honour of translation, because it afforded him a convenient peg on which to hang a copious, learned, and interesting commentary, forming "a complete manual of the surgery and medicine of the ancients, with a brief but comprehensive outline of the sciences intimately connected with them, especially physiology, the materia medica, and pharmacy." (Preface.) But was a work which professes to be only a compilation from the writings of former authors, and to contain little original matter, "except a few things which the author had seen and tried in the practice of the art" (Author's Preface), likely to interest any but medical antiquarians, a class of men who are not very numerous in this country? We trow not; and therefore, as long as the works of really great and original thinkers and discoverers, like Hippocrates and Galen, remained untranslated, we should have been inclined to have left Paulus Ægineta quietly on the shelf.

From the character of the work of Paulus Ægineta, as a mere compilation, not enriched by any power of thought or original discoveries, nor even enlivened (like Celsus,) by elegance of style, we think it unworthy of the honour of being the first of the ancient medical writers, presented by the Council of the Sydenham Society to their subscribers; perhaps there is also another reason which may have added to its unpopularity, though not with equal justice. The general principles of therapeutics were almost as well understood by the ancients as by ourselves; and a treatise on that subject, written by a sensible man two thousand years ago, would bear reading very well even in the present day. But a work on pharmacy, or one containing many medical prescriptions, certainly labours under great disadvantages when exposed to the public in an English dress. Accordingly, when some of the members of the Sydenham Society, upon opening the pages of Paulus Ægineta, were inclined to shut the book with feelings of mingled ridicule and disgust, on account of the uncouth names they met with, and the strange substances recommended as medicines, they should have borne in mind, that it is only of late years that these and similar substances have been expunged from the materia medica of modern European nations; that several of even Sydenham's prescriptions read somewhat strangely in Swan's or Pechey's translation; and that, even in the case of our own pharmacopœia, the plain homely English names of *coltsfoot, bearberry, dandelion, pennyroyal, pellitory, buckbean*, &c., have not quite so imposing and dignified a sound as their Latin synonymes.

With respect to the work itself, the most valuable part will probably be considered to be Dr. Adams's Commentary; which quite agrees with the character given of it in the title-page, and "embraces a complete view of

the knowledge possessed by the Greeks, Romans, and Arabians on all subjects connected with medicine and surgery." The learned editor's analysis of the opinions and practice of the ancients is (as far as we have observed,) exceedingly correct; but we cannot help regretting that the references are in many cases so vague and imperfect as to be almost useless. Indeed, he himself, in his 'Advertisement,' "admits that there are grounds for this objection," and that, "if he had the work to do over again, he should most probably take pains to obviate it."

Another very important defect, is the absence of any biographical account of the numerous medical authors quoted by Paulus Ægineta, the only exception (as far as we have noticed,) being a few meagre sentences about Diocles of Carystus. (vol. i. p. 186.) We have not even an index of proper names, which would have been of great service to the reader, and indeed (we may add,) to no one more than to the translator himself, as he would thereby probably have been enabled to avoid several little mistakes, into which he has fallen. We will instance one of these. In Book iii. chap. 64. (vol. i. p. 621.) among other "malagmata," or plasters, he mentions "that from apples, invented by Serapion:" (where, by the way, we will first remark that Dr. Adams has said nothing to warn his readers against the very pardonable error of supposing the Serapion in question to be one of the Arabic pharmaceutical writers of that name, instead of the empiric of Alexandria, in the third century B. C.); and again, at iv. 25. (vol. ii. p. 76.) he speaks of "the one from apples ascribed to Serapion." The Greek name in both these places is *μηλίνη** (sc. *ἔμπλαστρος*), and certainly one of the meanings of the word is *made of apples*. However, in process of time Dr. Adams arrived at the actual formula for the composition of this plaster (vii. 17. vol. iii. p. 566.), which is still called by the same name, *μηλίνη*, and which is found to consist, not of apples, but of litharge, wax, ammoniac perfume, galbanum, verdigris, Colophonian rosin, myrrh, and oil. Accordingly the translator no longer calls it the plaster "from apples," but "emplastrum melinum," which is the name given to it in the Latin translations of Galen, Aëtius, Paulus Ægineta, and Nicolaus Myrepsus, (by whom the same preparation is mentioned,) and which, if it does not explain the term to the unlearned reader, does not at any rate mislead him. With respect to the meaning of the word, we are sure that Dr. Adams will excuse our referring him to a passage in one of Galen's works (*De Compos. Medicam. sec. Gen.* ii. 6 sq., vol. xiii. p. 503 sq.), with which he is probably much better acquainted than ourselves, though perhaps at the moment it escaped his memory: and we think this instance (we might have given two or three others,) will show how much he would have been aided in his task, if he had made an index of proper names for himself, or had used that which was ready at his hand in Fabricius, *Biblioth. Graeca*, vol. xii. p. 580.

Again, Dr. Adams has fallen into the error (very common among learned men,) of supposing that all his readers are as well informed as himself, and that therefore it is unnecessary for him to be very particular in helping them to discover the nature either of the compound medicines or of the simple medicinal substances mentioned in the work. It is true that,

* Or, perhaps, to speak more correctly, the word *ought to be* *μηλίνη*, as in fact it is spelled in one place *μελίνη*, in another *μηλλίνη*, and in the third *μηλήνη*.

in the Advertisement, he promises a general index, that will facilitate the reader's researches in these cases; but we are sorry to say that this promise has been very imperfectly fulfilled, and that there are numerous articles mentioned in the text, respecting which the reader is forced to remain in perfect ignorance.—Indeed, the want of a good index of drugs and of proper names appears to us to be so great a defect in the execution of the work, that, if it were one of more general interest, we should consider it to be the duty of the Council even now to cancel the present index, and to substitute a complete one in its place: at all events, we may express our hope that they will never allow any similar work to be issued under their auspices, so imperfectly supplied with this indispensable appendage.

Another point of which we are inclined to complain, is, that the translator has not stated when he has ventured to correct the Greek text, (which he says he has done in the case of one chapter, vol. iii. p. 608, and which he certainly ought to have done very frequently,) nor has he even told us which edition he has used in making his translation. We do not mean that Dr. Adams should have noticed every unimportant correction (for this the corrupt state of the Greek text, which has not been reprinted for more than two hundred years, would have rendered an endless task,) but it would have been a satisfaction to have been assured that the emendation of the text had employed some of his attention; and we fear this has not been the case so often as we could have wished. For instance, the name of the *Tamarix Orientalis*, or White Tamarisk, is, in the Aldine edition of Paulus Ægineta, printed by mistake ἀκαλλίς, instead of ἀκακαλίς (vii. 3); an error which is corrected in the Latin version, and which even the alphabetical order of the words ought to have pointed out, but which is transferred by Dr. Adams to his translation (vol. iii. p. 26) and also to his index; and he has thus done his best to introduce into the pharmaceutical nomenclature of the ancients a plain and palpable botanical monstrosity.*

Like many great collectors, whether of facts, or books, or curiosities, he has occasionally admitted into his treasury some rubbish that ought not to have found a place there; and in many instances (particularly in the last volume, which treats of the materia medica and pharmacy,) a more concise form of commentary might have been adopted, without lessening the value or the amount of the information conveyed. Occasionally, too, the want of a critical spirit displays itself, so as to put us in mind of the person said by Bentley to "have a monstrous appetite, but a very bad digestion." For instance, there is a little work, not very often to be met with, entitled "Averroëana, being a Transcript of several Letters from Averroës, an Arabian Philosopher at Corduba in Spain, to Metrodorus, a young Grecian Nobleman, Student at Athens, in the Years 1149 and 1150." (Lond. small 8vo. 1695.) The very title would be almost sufficient to stamp the book at once as a palpable modern forgery, if indeed it was ever seriously intended to deceive any one; (see the *Biograph.*

* In his Commentary on this chapter, Dr. Adams says, that "it does not appear that it (the Acacalis) is treated of by the Arabians;" but this is incorrect. Sprengel, in his Commentary on Dioscorides (vol. ii. p. 396) gives the Arabic name أثل *Athl*, and Dr. Adams would have found a tolerably full account of it in Ibn Baitár, vol. i. p. 13.

Dict. of the Usef. Knowl. Society, vol. iv. p. 294)—but Dr. Adams says, that "*whether genuine or not . . . . . .* it must at all events be of *considerable antiquity.*" (vol. i. pp. 92-3.)

Again, he says (vol. i. p. 651) that Alsaharavius is "*probably*" the same person as Albucasis. Is there any reason whatever to doubt it? and is it not perfectly well known that Abu'-l-Ka'sim (or *Albucasis*) was surnamed Az-Zahráwí (or *Alsaharavius*), because he was born at Az-Zahrá, a small town near Cordova? Dr. Adams says (vol. i. p. 281) of Galen, that, during the plague in the reign of M. Aurelius Antoninus, "it appears that he fled from Rome for fear of infection." We know that this reproach is commonly brought against Galen by modern writers, and that his conduct in this particular is quoted as a parallel to that of our own Sydenham in similar circumstances; but, as we are loath to believe such a charge against so great a man without sufficient evidence, we should have been glad if Dr. Adams had fortified his statement by a reference to some ancient authority, as hitherto we have never been able to find any which appears to us to be conclusive. (See Smith's *Dict. of Greek and Roman Biography*, vol. ii. p. 208.).

We cannot help quoting two other sentences, which (coming from a man like Dr. Adams) are so extraordinary, that we are almost inclined to suspect either the existence of some typographical error, or that we have misunderstood the writer's meaning: 1. he says that from a statement of Haly Abbas, "it may be inferred that Alexander Trallianus and Alexander Aphrodisiensis were the same person"! (vol. i. p. 368); and 2. that "Jesu Haly was the son of Haly Abbas"! (Vol. ii. p. 283.)

The work is deformed by typographical errors which we should hardly have expected to find; e. g. *Nemesitanius, Apicus* (vol. i. p. 140), *Tertulianus* (i. 141), *Agatharcides* (i. 157), *Kaau Boerhaave* (i. 266), *cæliac* (i. 527), *Emperic* (ii. 8), *Isagogue* (ii. 17), *Chöepheræ* (ii. 19), *Bullonensis* (ii. 21, which appears to be considered as a man's name), *steotoma* (ii. 94), *perastis* (ii. 184, and note), *Anfidius* (iii. 546), &c. The errors in the Greek words and in the Arabic proper names are too numerous to specify; the latter are excusable, considering the present limited diffusion of oriental literature, but the former are not creditable either to the editor or the printer.*

We had wished to have said something about the materia medica contained in the third volume, and the results at which Dr. Adams has arrived in his attempt to verify each substance mentioned, in most of which he is probably quite successful; but this is so extensive and difficult a subject, that we have not space to enter upon it; and we omit it the less unwillingly, because there is in many cases so much difference of opinion among the greatest authorities, that, in those instances where we are inclined to doubt the accuracy of Dr. Adams, we know very well that he is quite as likely to be correct as ourselves. We may, however, mention one instance where the translator appears to us to have committed a slight oversight. He says of μαμιρᾶς (vol. iii. p. 239), that "this article is not mentioned by Dioscorides, Pliny, Galen, or Oribasius," which is so far correct, that the word itself is not to be found in any of those authors. However, it is in fact probably only another name for *Chelidonium majus*,

* To prove that we are not exaggerating, we may state, that, of the first fifty Greek words in vol. iii, no less than *fifteen* contain one or more typographical errors.

a plant which was of course perfectly well known to the ancients. He goes on to say, that he believes it is not mentioned "by any of the Arabian writers, with the exception of Avicenna;" but this also is incorrect, as he would have found it noticed by Ibn Baitár. (vol. ii. p. 487.)

But after all, these are only blemishes arising from human frailty and imperfection, not from ignorance or incapacity; for no one can, we think, examine the work without perceiving that great pains have been bestowed upon it, and that Dr. Adams may well be considered, in his own line, as "doctissimus Britannorum." Nor will our appreciation of the extent of his studies be diminished when we remember that it has not been acquired in a learned university or in a large capital well stored with libraries, but in a small country town in Scotland; nor, again, surrounded by all the comforts of literary ease and quiet, but amidst the toil and turmoil of active life. We therefore gladly conclude this notice of his work by pointing out some of those parts which may perhaps be found most generally interesting.

Book i. Sect. 16, 17. on Exercises; Sect. 51, 52. on Baths; Sect. 73. &c. on Dietetics; Book ii. Sect. 12. on the Galenic System of the Pulse; Sect. 35, 36. on Epidemics and Contagion (quoted with approbation by Dr. Copland in his Dictionary, under *Pestilence*); Book iii. Sect. 6. on Phrenitis; Sect. 9. on Lethargy; Sect. 55, 56. on Spermatorrhœa; Sect. 77, 78. on the Treatment of Arthritic Diseases by Colchicum; Book iv. Sect. 1-10. on Skin Diseases, as they appear in warm climates; Sect. 46. Treatment of Malignant Ulcers, especially of *Noli me tangere;* Book v. Sect. 1. General Principles of Toxicology; Sect. 30. on Poisoning by Cantharides; Sect. 42. on the Modus Operandi of Opium, and treatment in cases of poisoning; Sect. 44; on Mandragora, and the anæsthetic means used by the ancients (see also i. 98; vi. 84; and vol. iii. p. 240); Book vi. Sect. 33. on Laryngotomy; Sect. 40. on Venesection; Sect. 88. on Military Surgery, especially the extraction of weapons; Sect. 89. &c. on Fractures and Dislocations generally, especially on Luxations of the elbow, knee, and hip joint; on the use of waxed bandages in fractures. &c.; Book vii. Sect. 2. on the General Action of Medicines; Sect. 4. on the Modus Operandi of Purgatives and their uses; Sect. 7. on the Treatment of Hypercatharsis; Sect. 10. on Emetics, particularly on Hellebore (see also p. 107); Sect. 11. Account of the Theriaca. Some of the more elaborate articles in the materia medica are *μανδραγόρας, ὕσσωπος, χάλκανθος, χαλκῖτις, χρυσοκόλλα*, &c., together with the Appendix, "On the Substances introduced into the Materia Medica by the Arabians."

If any one will read over some of these chapters candidly, with the Commentary of Dr. Adams, we are inclined to think that he will allow, that, even in a *practical* point of view, the work does not deserve the contemptuous ridicule with which it was at first received by some of the members of the Sydenham Society; and that, on many points of medicine and surgery, the ancients knew a great deal more than the moderns are in general willing to give them credit for.

Art. V.

1. *First Report of the Commissioners appointed to inquire whether any and what special Means may be requisite for the Improvement of the Health of the Metropolis; with Minutes of Evidence.—London,* 1848. 8vo, pp. 430.
2. *Second Report of the Commissioners, &c. &c.—London,* 1848. 8vo, pp. 144.
3. *A Disquisition on Pestilential Cholera, being an Attempt to explain its Phenomena, Nature, Cause, Prevention, and Treatment, by reference to an extrinsic Fungous Origin.* By Charles Cowdell, M.B., M.R.C.S.—*London,* 1848. 8vo, pp. 210.
4. *A Discourse on the Asiatic Cholera.* By Thomas Henry Starr, M.D., Physician to the Warwick Dispensary.—*London,* 1848. pp. 95.
5. *British Cholera; its Nature and Causes considered in connexion with Sanitary Improvement, and in comparison with Asiatic Cholera.* By Spencer Thomson, M.D.—*London,* 1848. 8vo, pp. 110.
6. *Du Choléra; Moyens préservatifs et curatifs, ou Philosophie des Grands Epidémies.* Par M. Bureaud-Riofrey, D.M.P., &c.—*Paris,* 1847. pp. 112.
7. *Report of Alexander Thom, Esq., on the Causes, Character, and Treatment of Spasmodic Cholera in H. M. 86th Regiment, at Kurrachee, in June,* 1846. Ordered by the House of Commons to be Printed, 21st March, 1848.

In disturbed times, the records of events are irregularly kept. Amidst the crash of thrones and the collision of races, men forget to mark the occurrences which bring with them no social change, and no political catastrophe. Amidst the destruction of empires, and the disappearance of dynasties, the evil which threatens the lives of individuals only is unheeded.

Absorbed in the discussion of events of extraordinary interest, or intent on discerning the probable bearing of a troubled and gloomy future, the journalists of Russia and of Germany have apparently forgotten to note the onward passage of that terrible disease which a few months since engrossed so much attention, and excited such keen apprehensions. Or it may be, that there are other and more satisfactory reasons for this silence; the plague which threatened us may have been averted, and the clouds which menaced us, dispersed. After advancing into the heart of Russia, and traversing step by step the well-known and devious path of its first advance, the cholera may have been checked by the intense cold of the northern winter, or by some of those obscure and mysterious meteorological changes, which have so often limited its progress. It has, at any rate, ceased in Constantinople and in Moscow, and in several of the provinces of Russia; and although no certain information respecting it has been lately received in this country, we may venture to believe that this cessation is common to all the parts of Southern Russia.

Supposing that this opinion be correct, and that the cholera has really died away out of Europe, we must yet remember that experience has too often proved these retirements to be but temporary. It must not be sup-

posed that cholera of necessity spreads regularly onward, and that its progress can be tracked by any invariable and formal rules. It advances, wheels about, retraces its steps, winds a slow serpentine course, now to one side, now to the other, leaving untouched one village, almost depopulating another in the neighbourhood, and attacking the half of a third; then acquiring in some way a rapid increase of volatility, it makes a sudden jump, perhaps leaves a large district almost untouched, and invades a town, the inhabitants of which had flattered themselves they were still at some distance from the enemy. To those unacquainted altogether with its laws, its progress often seems in the highest degree uncertain and capricious; so influenced is its appearance by general atmospheric conditions, by local impurities or contaminations of the air, and by the condition of body of those subjected to the action of its cause. It would be very unwise to mistake a temporary arrest for a complete cessation, and to congratulate ourselves too soon that our anxiety has been premature, and our precautions unnecessary.

Whether or not we are eventually to witness an epidemic of cholera in this part of Europe, we have at least derived great advantages from the bare apprehension of such an occurrence. The causes and propagation of the disease have been again subjected to a keen discussion; the views of its pathology have been rendered more definite and uniform; and the true powers of medicine in its treatment have been more strictly and correctly determined. In addition, the government has set on foot inquiries of high importance; inquiries whose fruits will not pass away with this occasion, but which will always be productive of the greatest benefit to the whole community. We do not hesitate to assert that the labours of the Sanitary Commissioners, chiefly directed as they have been to the subject of cholera, demand the highest approbation of the medical profession. The results of these labours are, at the same time, in accordance with the most scientific and well-founded medical doctrines, and they are susceptible of immediate and practical application. The commissioners have not enunciated barren principles; they have applied those principles to practice; while they deduced the law, they devised means for carrying it into effect; they have formed the plan, and have also supplied the materials for the building. The benefits of this inquiry will not, we trust, be confined to this country. Public attention has been so strongly drawn to a disease which ravages the imperial dominions in Asia to an extent little dreamt of in England, that government cannot stop short in its investigation. The inquiry must be extended to Hindostan; the disease must be subjected to a stricter inspection in the regions of its birth and development; and the science of Europe must, for our own safety, no less than for the sake of our Asiatic fellow-subjects, attempt the removal of the causes, which in Asia, as well as in England, confer on the poison of cholera its terrible and malignant power. We have no doubt that a properly organized commission in India, composed of men well acquainted with the great sanitary principles which have been determined in this country and on the continent, would be able, in the course of a very few years, to do even more for the happiness and comfort of the poorer class of Hindus, than can be expected to be accomplished for the lower orders in Great Britian. The causes of many diseases, and of cholera among the number, which are rife in England, are still more powerful in India; and the absolute power which the Company

possesses in their magnificent empire, could not be better employed than in the removal of the sources of those morbific agents, which, when once produced, are not arrested by the landmarks of the countries which originated them, but propagate themselves to distant lands, and to territories far remote from the place which gave them birth.

We trust, therefore, that the able men who are at the head of the sanitary movement in this country, will not limit their inquiries solely to these islands, but will extend them to all parts of the empire. This is not only a duty which they owe to their country; it is absolutely necessary for the furtherance of their great reform.

We propose, as soon as it is conclusively ascertained that the immediate danger of an outbreak of epidemic cholera has passed over, and when we have received the report of the Russian and German physicians who have on this occasion been brought into contact with the disease, and of the French commissioners who were sent by the late government to watch its progress, to enter into a full analysis of the whole subject, and to arrange, as well as our space will permit, all the facts and doctrines of the disease under their appropriate headings of ascertained, probable, and conjectural. At present we shall confine ourselves to a critical analysis of the works before us. Their publication affords us an opportunity for a discussion on several of the most interesting points connected with cholera. This discussion we may arrange under the following heads:

1. The contagion of cholera.
2. The conditions of spread, and of the liability of individuals to attack.
3. The nature of the active cause.
4. The relations between British and Asiatic cholera.
5. Suggestions of treatment made by the Sanitary Commissioners.

I. CONTAGION OF CHOLERA.

The Sanitary Commissioners state, at the commencement of their Report, that the first subject imperatively claiming their attention, was the reported progress of Asiatic cholera towards Europe. They first inquired what measures had been adopted to prevent the introduction of the disease in 1832. They found that the earliest directions issued by the Central Board of Health appointed by the Privy Council, were measures of strict and rigorous quarantine; as, however, the cholera gradually traversed Europe, with total disregard of all the attempts made to limit it by cordons and barricades, the Central Board were speedily obliged to modify their instructions; and in less than a month after their first proclamation, they stated that, from the accounts they had received from various parts of Europe, and from the careful observations made in Russia by Drs. Russell and Barry, they were enabled satisfactorily to declare—

"That, under proper circumstances of cleanliness and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick, under such favorable circumstances, unless they happen to be particularly predisposed. It will not therefore be necessary, where there is space, and where due attention is paid to cleanliness, and purity of air, to separate members of families actually affected by the disease."

The Central Board after this, issued instructions for the appointment of inspectors, who were to superintend the cleaning, fumigation, and ventilation of the poorer houses of the district, and who were empowered to send

patients to the temporary cholera hospitals, and to supply the remaining members of the family with additional means and comforts.

After this statement of the measures adopted in 1832, the Sanitary Commissioners proceed to state that their investigations have conducted them to certain important conclusions.

(*a*) That the mode of invasion of cholera in the various cities of Europe has been everywhere strikingly uniform. It has almost always made its first outbreak in the lowest and dampest portion of the city attacked. They verify this statement by references to St. Petersburgh, Dantzic, Berlin, Moscow, Breslau, Warsaw, Paris, Sunderland, Carlisle, Manchester, London, and England generally.* The Commissioners remark that it is the combination of humidity with impurity of the atmosphere, which so powerfully predisposes to cholera; cleanliness seems to be capable of counteracting the effect of mere humidity; the scrupulous cleanliness of the inhabitants of Holland was probably the cause of the comparative exemption from cholera which that country enjoyed.

This conclusion is borne out by the evidence collected by the best writers on cholera, particularly by Orton and Jameson.

(*b*) That there is no evidence that cholera spreads by the communication of the infected with the healthy. To prove this, the Commissioners cite instances in which quarantines failed to arrest the progress of the disease, and also cases in which it broke out in too diffused and rapid a way to allow of the supposition of its having been communicated from man to man. They also quote extracts from evidence to prove that, in France and England, the cholera often originated in places to which it could not have been carried by human beings; and that it did not occur in some remarkable instances, in which large populous towns held free and daily intercourse with infected districts. Thus, in 1835, Marseilles suffered severely from cholera, while Lyons remained free, although nearly 10,000 inhabitants of the former city fled for safety to the latter. In 1832, Birmingham remained untouched, although at Bilston, eight miles distant, and having hourly communication with Birmingham, the disease was more severe than in any other town in England. The Commissioners also state that—

> "Every witness, with one exception,† examined by us, appears to have arrived at the most clear and decided conviction, from what was uniformly observed of its progress in the metropolis, that the disease did not spread from the communication of the healthy with the infected." (First Report, p. 15.)

During the past year the opinions of the medical press on the subject of the contagion of cholera, have been singularly accordant. Both our predecessors, the British and Foreign, and the Medico-Chirurgical Reviews, as well as several other Journals, devoted elaborate articles to this subject. We shall not therefore occupy more space by a repetition of the arguments therein urged, or the examples therein cited, than is necessary for the clear elucidation of the subject. The conclusion arrived at by almost the whole medical press, was that the doctrine of contagion, in the strict sense of

* This point has been also insisted upon by one of our predecessors. See 'British and Foreign Medical Review' for April, 1847, p. 338.

† Mr. French, who believes cholera to be "like typhus, propagated by contagion under unfavorable hygienic conditions." (p. 121.) We presume, therefore, that, under favorable hygienic conditions, Mr. French would consider the disease non-contagious.

the word,—that doctrine which supposes that there *must be* contact or proximity between man and man, in order that the morbid poison may pass from one to the other,—cannot explain the phenomena of the diffusion of cholera.

This statement was proved, not only by a very full and complete examination of the opinions of those writers who had had the fullest opportunities of observation, and by an analysis of the reputed instances in which contagion had been deemed active, but also by a consideration of the peculiar mode in which cholera attacks a place, or spreads from one district to another. It was in this way shown: 1. That the vast majority of the most eminent authors disbelieved in the contagion of the disease. 2. That the reputed instances of contagion were often erroneously reported or interpreted, or were very indefinite or doubtful, or were decidedly untrue. 3. That the peculiar mode of spread in a certain direction, and the attack of the disease at one time, or its localization at another, were not explicable on the ordinary doctrines of contagion, that is, on the supposition that the active cause was generated and augmented only during, and by means of, its action on the living animal system. At the same time it seemed to be generally admitted, that it would require further research and deeper observation, before we could venture decidedly to affirm that the poison of cholera *could not* multiply itself during its passage through the body; that it could not, like smallpox, find in the human organization the materials for its growth and reproduction, but that its subtle principle was exhausted in the effect it produced, and was annihilated in the system which bore evidence to its potent agency. But if such a reproduction did possibly occur as an exceptional and unusual case, it was argued that, practically, it was unnecessary to regard it; "the question, as far as legislation and prevention are concerned," writes one of the reviewers referred to,* "may be considered settled. There can exist no doubt, that even if cholera be contagious, it cannot be localized and restrained by quarantines; theory and experience both demonstrate the inutility of measures of this kind."

On this point we cannot be too explicit; cholera does not *require* human frames for its transit and its multiplication; it is not bound in by lines, nor circumscribed by empty spaces; wherever it finds its conditions of existence it can spread, although for miles no man is found, whose frame may be the test of its power. This point we firmly believe is settled; it requires no qualification; it needs no further discussion; henceforth it must be a question which it would be a work of supererogation to reopen.†

* British and Foreign Medical Review for April, 1847, p. 333.

† In the Lancet for March of the present year is a report of a discussion at the Westminster Medical Society, on a paper of Dr. Ogier Ward's, in which that gentleman maintained, with some slight modifications, the strictly contagious nature of cholera. We have a great respect for Dr. Ogier Ward, but we beg him to go over his evidence again more accurately, and he will find it less conclusive than he thinks. We can assure him that he will not find an unequivocal case of contagion in the works of his authorities, viz. Russell and Barry, Kennedy or Orton. We entreat him also, not to rely upon Moreau des Jonnès, who is the most inaccurate of writers; and who, although a very able and clever man, is not to be trusted in any delicate question. We are astonished that Dr. Ogier Ward should assert still, that quarantines can arrest cholera, after the overwhelming evidence to the contrary, and after the point has been surrendered even by the most eminent of those who still believe in contagion. In the Lancet for April, is a continuation of the discussion on Dr. Ward's paper. Several able speakers advocated the non-contagion of the disease. On the other side, Dr. Copland brought forward the evidence which he has already published in the work on Cholera, and in his article in the Dictionary.

We may, however, very briefly, and more as a matter of curiosity than because the question needs it, allude to the additional arguments against contagion, which can be drawn from recent epidemics. Thus, in their second Report, the Sanitary Commissioners quote the Report sent to Vice-Consul Stevens by two Italian physicians (Drs. Sapi and Borg), respecting the attack of the disease at Trebizonde in September, 1847.

"During the month of August," says the Report, "among other prevailing diseases, were particularly to be noticed bowel complaints, which yielded, with some obstinacy, to the ordinary remedies. Considering this predisposition to the cholera morbus, its proximity, and the conflicting opinions afloat respecting its mode of propagation, it was deemed advisable to prevent communication between the Russian ports which were infected and our coast, and to this effect vigorous measures were issued from the sanitary office.

"These precautionary measures proved as ineffectual in preserving us, as those taken for the health of the place. On the 9th September a mechanic died, whose body on inspection presented unequivocal signs of the cold plague. On the 10th we visited several patients, who presented all the symptoms of cholera. None of those persons were near each other, and their dwellings were situated in different parts of the town. . . . . Several families who had isolated themselves in the town, or in the villages, in spite of their precautions, were attacked. Those families which, during the period of the epidemic, observed a rigorous system of diet, notwithstanding their direct or indirect communication with cholerics, escaped. Those who took refuge in elevated places in the country also escaped." (Second Report, pp. 5, 6.)

Here, then, quarantines were ineffectual; isolation gave no security—contact produced no apparent increase of liability.

The Reports of the Russian physicians at Astrakhan and Moscow, in 1847, confirm those from Trebizonde.

Quarantines were at first established at Astrakhan; but, as the disease appeared in spite of them, they were entirely given up. At Moscow, the Report states that—

"The authorities had altogether abandoned the old theory of the spread of the disease by contagion, a theory which had led, in 1830, to the most extravagant hopes from the benefits to be derived from cordons and quarantines." (Op. cit. p. 8.)

Instead of these measures, excellent sanitary precautions were adopted, which we shall hereafter have to detail. The Report continues:—

"It will be seen by the measures taken to check the disease, that it is not now regarded as exhibiting the same strong contagious principle as was formerly everywhere believed to be the case when it first spread its ravages through Europe. Among all the physicians of Moscow, there is certainly not one who believes that a cholera patient communicates the disease by the touch. Daily experience is too decided on this head. As yet not a single death has occurred among the numerous physicians occupied in the sickness, or at the dissections connected therewith.

"The Swedish Commissioners, we are also informed, bear their decided testimony against the opinion that the diffusion of cholera is at all dependent on the communication of the healthy with the affected." (Op. cit. p. 11.)

The Commissioners also refer to the evidence adduced by Dr. Parkes in his work on the Asiatic Cholera. This evidence may be thus compressed. In 1843, Dr. Parkes witnessed a severe epidemic of cholera in the Tenasserim provinces. The disease passed in a regular course through the country, nearly from north to south; its introduction into a place was never traced to intercourse; medical attendants and hospital servants

were not attacked in undue proportion; corps having free intercourse with infected districts wholly escaped; some portions of the principal town also escaped, although not isolated from the affected parts. In addition, it is mentioned that the inhabitants of villages, when attacked, universally left their homes and travelled into the jungle, with the result of stopping the disease; although it is argued that, under the privations and exposure of such a flight, the villagers must have been more predisposed to the attacks of the disease, and the poison of the disease must have been heightened in its contagious property, had it really possessed it.

But the most striking instance is recorded in the Report, by Mr. Thom, of the epidemic which attacked the 86th regiment at Kurrachee, in Scinde, in 1846.* The fearful mortality of this epidemic, which destroyed in a few days 700 fighting men and several thousand civilians, has made a profound impression on the profession and on the public.

In our first Number we quoted from Dr. Milroy an account of the attack, and to this account we may refer our readers. It appears, however, that there has been some misconception, which ought to be corrected. It has been stated, that the epidemic which visited Kurrachee was an isolated attack, occurring altogether separately from any general epidemic affecting the neighbouring districts. This is a mistake. Long before the outbreak in Scinde, cholera had been traversing in various directions Affghanistan, Hindostan, and the outlying countries. It had been spreading gradually from Madras towards Bombay and the coast of Malabar.

"Long before its visit to Kurrachee," writes Mr. Thom, "we heard of its appearance in the Madras Presidency, and subsequently at Poonah, Bombay, and Ahmedabad, Deesa, and finally arriving by the coastward to Scinde, where it first broke out at the sea-coast, and from thence gradually extended upwards to Hydrabad and Sehwan, where its further progress appears to have been stopped, as it never reached Sukkur or Upper Scinde." (Report, p. 8.)

The presumed isolated and sudden attack at Kurrachee, was not merely evidently a part of the general epidemic, but cases of undoubted cholera had occurred at intervals for some months before, proving the gradual development of the epidemic constitution. It has also been said, on the authority of a Bombay paper, that the attack commenced at a particular hour, after the appearance of a black cloud and a tremendous thunderstorm. It now appears, however, that, not only had the disease prevailed for a fortnight in the native town, but that, for two or three days previously, cases had been coming into the 86th hospital, and that more than twenty cases had been admitted before the storm.

"An absurd cause," says Mr. Thom, "passed current in camp, and has been re-echoed by the Bombay papers; viz. that the disease broke out after 'a black cloud' and dust-storm, which took place at five o'clock p. m. on the 14th; while, in reality, twenty cases had been admitted, and arrangements had actually been made to move our regiment, before this not uncommon phenomenon terrified the crowd. As it was next morning before the awful condition of our corps was generally known in camp, a few futile imaginations attributed the disease to what they had seen, and others accepted this, because it was full of mystery." (Report, p. 13.)

* We shall have frequent occasion to refer to this excellent Report, which reflects the greatest credit on its able author. It may be taken as an instance of the high qualifications of our brethren of the army department. It is, indeed, most gratifying to think what valuable information is being gradually collected under the admirable management of Sir James M'Grigor—information which has in part been made available in the Army Reports, and the remainder of which will, no doubt, in its proper season, be brought before the public.

Mr. Thom gives the following account of the outbreak of the disease, which, excepting in its terrible rapidity, coincides with the general mode of attack:

"It was equally apparent," he says, "that there was nothing contagious in the nature of the disease; for, instead of a few cases appearing first, and then the disease gradually spreading, it suddenly burst forth in a few hours in every European regiment, whether in camp or barracks, in every tent, and in every house; and it was at its acme in forty-eight hours after; when, instead of spreading further, it gradually and steadily declined. Now, it appears that, for some days, or even weeks, a few cases had appeared in the native town of Kurrachee, but there, also, at the same period, the malady became *suddenly* general over the whole place." (Report, p. 13.)

If its outbreak was thus opposed to the idea of contagion, so also was its future progress. The native town of Kurrachee, where the disease first appeared, is about a mile from the cantonment. In June, 1846, two native regiments were quartered in barracks at the point of the cantonment nearest to the town; some way further removed from the town were the officers' quarters; still further off were the barracks of H. M. 60th; and to leeward of all, most remote from the town, and half a mile from the 60th barracks, were H. M. 86th regiment and the Bombay Fusiliers, encamped on a low plain. For about a fortnight, cholera was prevailing to some extent in Kurrachee itself, and between this place and the nearest point of the cantonment, occupied by the native regiments, there was daily and hourly communication. But when the attack commenced in the cantonment, it did not commence in these Sepoy lines, as must have been the case had it been propagated by contagion, but it passed over *all*, and "burst forth with unparalleled fury in the 86th regiment, quartered at the most leeward and remote point. It then turned back in the teeth of the wind, and broke out in the 60th barracks as suddenly as in camp; and a few days later it *recrossed* the officers' lines, and appeared among the native troops nearest to the town. These facts," says Mr. Thom, "are alone sufficient to show that contagion had nothing to do with the spread of the disease." It also appears, and the point is a most interesting one, that the officers quartered between the 60th regiment and the Sepoys, and therefore with pestilence raging on either side of them, and "all exposed by the nature of their duties to have caught the disease, had nevertheless almost a complete exemption from its deadly influences." The condition of these officers differed from that of the men in three particulars—1, they were in houses detached and isolated, and therefore permitting a freer current of air; 2, they were probably not so exposed to night duty, although, of course, they had their share of this; 3, they were well fed, and perhaps better nourished than the men. But if contagion had been operative, these circumstances could never have secured them the nearly complete immunity they enjoyed, although hourly in contact with the sick. This observation is even stronger than the one the Commissioners quote from Dr. Parkes,—that in 1845, at Madras, while cholera was very fatal among the dense masses of the Hindoos in the Black Town, it was less prevalent among the soldiers in Fort St. George, and was not seen at all in the scattered and well-ventilated houses of the English merchants and residents, whose duties in many cases called them daily into the Fort or the Black Town.

Mr. Thom also states, that "medical officers and attendants did not suffer more from the disease than others, although fatigue caused a greater numerical proportion to fall sick with diarrhœa."*

It appears almost tiresome to multiply instances in which an attack of cholera could not have arisen from the ordinary operation of a contagious virus; but we will cite one more, which seems to us peculiarly strong. In 1842, H. M. 9th Lancers proceeded up the Ganges in two divisions, *en route* to Cawnpore. The regiment had just arrived from England, and the men were in rude health. The left wing marched on the 22d September; an unfavorable time for a voyage up the Ganges, on account of the rapid subsidence of the inundations, after the heavy rains of the monsoon. As they proceeded, a few cases of fever and of dysentery occurred, and on the 25th of October they had the first case of cholera. Admissions from cholera soon became general, while the fever cases disappeared. The cholera was of the worst kind, with few spasms, and no great amount of vomiting and purging. It reached its height in about four days. It was, at the same time, frightfully prevalent in the villages on the banks of the Ganges; and, on arriving at Monghyr, they found the bazaars deserted, or depopulated by the disease, which had raged for some months. The mortality on board the squadron of boats was great, and the medical officers recommended the speediest possible transit through the infected districts. Accordingly, every exertion was made to pass rapidly up the river, and in about twelve days the disease began to decline in severity. This change was coincident with their arrival in a country not suffering from cholera in so great a degree. A few days later they emerged altogether from the affected district, and at the same time the cholera completely left them. There was no evidence of contagion; the boats, 30 or 40 in number, were equally attacked, and the sick were necessarily mixed up with the other patients on board the hospital boat, yet the disease did not spread to these latter, or to the attendants.

But the important point is, that about a month after this, the right wing and head-quarters left Calcutta, and proceeded in the same way up the Ganges. The voyage was an exact counterpart of that of the other wing—*they were attacked by cholera at the same point, pushed rapidly on, and lost it where the first division lost it*. The severity of the disease, the mortality, &c., were exactly the same. The disease still continued to prevail in the villages on the banks.†

We shall not occupy more space by details on this subject,—our readers may possibly think we have already been too prolix. Let us simply repeat our conclusions, that cholera does not ordinarily spread by contagion, that is, by generation of its poison by the human body *only;* that it recognises other laws and very different modes of increase; and, as is proved both by theory and practice, that it cannot be arrested and bound in by quarantines and cordons.

We have already said, however, that it is a question of a different kind,

* Ibid., page 375. The proportion of deaths among the "medical subordinates" of the 86th Regt. was only 4 out of 40, or at the rate of 100 to 1000 of strength.

† These particulars have been obligingly communicated to us by Mr. Bostock, of the Fusilier Guards. It is an interesting point, that the medical officers of the left wing decided against bleeding, on account of the low type of the disease; in the right wing, however, bleeding was resorted to in several cases; but this difference of treatment made no difference in the mortality, which was absolutely the same in both divisions.

whether the poison of cholera can ever be given off from an infected person;—there is no reason why it should not; there is some real evidence to prove that it sometimes is. But then this mode of increase is certainly very rare; many excellent observers have never met with a case, although intimately acquainted with the disease; and many of the recorded cases are, we are prepared to show, quite inadequate to warrant the conclusion drawn from them. It is an undecided point, on which no man is warranted in giving at present a positive opinion. Fortunately, however, it is a point of little practical value; for of what consequence is it, if, once in a thousand times, or, it may be, in fifty thousand times, reproduction of the poison occurs in this way? No art can guard against it; no precautions avert it. And it would be the extreme of absurdity to look at cholera only from this narrow point, and to legislate for one case rather than for nine hundred and ninety-nine. It is a scientific curiosity; not a thing for statesmen and legislators. And this appears to be the view taken of it by the Sanitary Commissioners. They seem to have considered that their Report, intended to be the basis of preventive measures, should contain only recommendations which were practicable, and principles which could be used; they did not wish to theorise till all became uncertain, and to refine till limitations and distinctions were blended into obscurity by qualifications and provisos. They offer to the Government a certain and well-ascertained fact; the *usual* mode, say they, in which the cholera spreads, is not from man to man; and, consequently, against this disease other appliances and other precautions are to be used, than measures which are applied only to one multiplying source, and that the most uncommon, viz. the human body.

At the same time, although it is advisable not to discuss with a non-professional audience scientific points of such extreme difficulty as this, viz. whether a poison which ordinarily spreads without acting on the body, does in rare instances multiply itself in this way,—still, it must not be supposed that we underrate the importance of this inquiry. It is, both as regards cholera and as regards other morbid poisons, particularly those of plague and yellow fever, one of extreme interest. Well-weighed and well-authenticated facts are necessary before it can be affirmed or disproved,—at present, the weight of the argument is on the affirmative side.

There is another point in the propagation of cholera which requires deeper examination. There can be no doubt that cholera often spreads, if it finds its conditions of increase, over a district which may be nearly uninhabited, and that it thus arrives at one place from another, without any intercourse whatever having taken place. But, in some cases, the poison has certainly appeared to be transported by a body of men—it has become "portable," to adopt a term which has been used, we observe, by Dr. Watson. As a good example, we would quote the fact of regiments in India, which have been attacked at a certain place, continuing to be attacked during their march through an unaffected district,* and, at

* Orton (a contagionist) writes; "In many cases it is distinctly proved, that the towns and villages through which the infected corps passed were quite free from the epidemic at the time—the diseased body passed on in a state of most intense suffering, bearing with them the seeds of disease and death, while the peaceful inhabitants of the country were enjoying perfect health." (On Cholera, 2d edition, p. 395.) It must be remembered, also, that marching heightens immensely the predisposition to the disease.

a period of time too remote to allow us to attribute it merely to an incubative period, unusually prolonged. These cases are not common, but they do occur; and the explanation does not seem to us to be very difficult. Wherever the poison of cholera meets with its conditions, it will propagate itself; these conditions seem to be furnished in a high degree by the effluvia and emanations of camps or bodies of men;* the poison once introduced among such a body, finds in the moving camp the necessary conditions, as easily and as abundantly as in the stationary city. Those who know the multifarious crowd, which, under the name of camp followers, tracks a regiment on the march in India, will not for a moment doubt that such an uncleanly and impure assemblage must inevitably furnish to the poison ample materials for its support. We are willing to admit that there may be an actual transport of poison in this way, and no doubt some of the stories of contagion have thus arisen. But, of course, such a mode of transmission as this, comparatively rare as it is, cannot be confounded with contagion, that is, with the increase of the poison by its action on the body. The two things are quite different; and though it should be hereafter proved that a poison may increase in both ways, they are totally distinct. A custom is common in India, which may at first sight seem to contradict this explanation of the conveyance of cholera. It is not an uncommon thing, when a corps is severely attacked in barracks or camp, to shift ground from day to day, and so, as it were, to elude the enemy. This practice has really been followed by good results; after a few days' marching and countermarching the disease has disappeared. Now, it might appear strange, that cholera should at one time be got rid of by marching, and that at another time it should be carried for a long distance. But the explanation is very easy: —when a corps moves out of barracks to avoid cholera, it takes no baggage; it is in light marching order; it has few camp followers; the marches are short, and do not fatigue the men; there are plenty of tents, and room to pitch them; there are none of the circumstances attending a regular march, when the distances are great, the halting-grounds perhaps low and damp, and the camp is pitched as close as possible, and is surrounded by an innumerable crowd of camp followers, who sometimes reach the astonishing number of eight or ten for every fighting man.

It may be urged, that, in a case of this kind, when a regiment conveys the poison, and might thus introduce it into a place where it can find its conditions of increase, quarantines and cordons might be advantageously employed. This we are not disposed to contest, although we should be puzzled to find instances proving the benefit of such measures. And, on the other hand, we are overflowed with examples, in which regiments, suffering terribly from cholera, have been permitted to enter a place, and have not communicated the disease to the inhabitants. Thus, in 1843, the right wing of H. M. 63d regiment was attacked with cholera on the march from Madras to Bellary; on arriving within eight miles of the latter place they were forbidden to enter, and therefore encamped outside, but changed their ground from day to day. The heat was intense, the thermometer stood at 110° to 115° in the tents, and the air was perfectly motionless;

* This point, as well as the conveyance of the poison by bodies of men, was first noticed by Jameson, in the Bengal Report, but the extent to which it is really true is only now beginning to be suspected. See British and Foreign Medical Review, April, 1847.

in two days the disease had increased so much, that the surgeon reported, in the strongest terms, that unless the men were put into suitable buildings, the entire wing would be annihilated—accordingly, they were permitted to enter the fort at Bellary; the men were comfortably accommodated, and, to use the words of the surgeon, "the disease at once assumed a different character." Before marching in, they had lost 76 non-commissioned officers and men out of a strength of 600; afterwards there were only 7 more deaths. The disease did not spread in the town or fort, and no ill consequences whatever followed.* This example is only one of many which we might quote; we have taken it because it is very instructive, as it proves how really amenable cholera is to sanitary measures. The disease was raging furiously till the men were placed in proper hygienic conditions; then it was at once arrested. It is a good illustration of what correct principles of prevention will hereafter do, in mitigating the ravages of cholera.

As to the employment of quarantines in the case where a large body of men, affected with cholera, approach a place, it must be left to individual discretion. If the place is in good sanitary order, and if the new comers could be comfortably put up in well-ventilated, and not crowded, quarters, they might be at once admitted. If the place is one in which cholera often prevails, quarantines may be advisable, and, at any rate, all measures should be on the side of caution. But, in thus admitting a possible use of quarantines, we are aware that we are rather deducing a rule from uncertain and ill-ascertained premises, than from the examples and records of experience. To judge by these latter only, we should deny all efficacy to quarantines; when we advocate their use in the case referred to, we go on the uncertain ground of the possible and the contingent.

The Swedish Commissioners appear to attach greater importance to this mode of propagation than we do.

"After hearing," write the Sanitary Commissioners, "their decided testimony against the opinion that the diffusion of cholera is at all dependent on the communication of the healthy with the affected, the Swedish Commissioners state that, in their judgment, there is still another subject of inquiry. 'Another question,' they say, 'equally important, remains. How is the disease transported from one district to another? Does this also take place entirely miasmatically, or may we not suppose that it is often carried by a person?'" (Second Report, p. 12.)

The Sanitary Commissioners appear to doubt this mode of conveyance, and point out the sources of fallacy which may arise from coincidences, in a country with many towns in close proximity, and keeping up a free and constant communication with each other. We are also of opinion that the probability of the frequent or constant conveyance by a single person is as yet quite unsupported by evidence. That a person may convey the disease occasionally, we have already admitted, when we allowed that the poison might, in a very small minority of cases, be reproduced by the human system; but that its mode of travelling is usually in the way supposed by the Swedish Commissioners, we cannot, without more evidence, allow. If it were so, quarantines would have been of greater service.

We shall now leave the subject of the contagion of cholera, and pass on to the Commissioners' third conclusion:

(*c*) That cholera observes in its progress the laws of ordinary epidemics,

* Vide Madras Medical Journal, vol. vi, pp. 122-3.

being influenced by the same physical conditions, and attacking similar classes of persons.

The consideration of this conclusion leads us to our second head:

II. CONDITIONS OF THE SPREAD OF CHOLERA, AND OF THE LIABILITY OF INDIVIDUALS TO ATTACK.

The Commissioners remark that—

"When that change takes place, by which certain diseases become epidemic, though science has not hitherto afforded sufficient light to guide us to the knowledge of the physical circumstances in which the change consists, it is a matter of universal experience, that certain physical conditions promote both the intensity and the extension of such diseases. These conditions may be comprised in impure and humid air, and unsuitable food, or—what more rarely occurs among a population in which upwards of £24,000,000 per annum, or more than five times the amount of the poor-rates, is spent in ardent spirits alone, and nearly an equal amount in tobacco and fermented liquors,—want of means to obtain sufficient food. Added to these are, unsuitable or insufficient clothing, sometimes ill-constructed dwellings, and defective appliances for the regulation of warmth or protection against cold." (First Report, p. 16.)

We shall at the present time imitate the Commissioners in forbearing to inquire into the nature of the "change which occurs when a disease becomes epidemic." It is evident that, at times, certain atmospheric conditions, certain exhalations from the soil, and certain electrical states, concur and form an "epidemic constitution," through whose agency a morbid poison propagates itself with unusual rapidity and intensity. The disease may be common marsh fever, or yellow fever, or bubo-plague, or cholera, according to circumstances of locality and habitat. In the olden time it might have been sweating sickness, or morbus cardiacus, or the black death, or any other of the numerous forms of disease, which have now nearly died out from among us. For our present purpose we shall restrict ourselves to a single branch of the subject, and shall inquire what are the general conditions furnished by a single locality, which seem essential to the development or the increase of cholera. At the same time we may express our conviction, that the conditions which locally favour its spread are analogous to those which favour the epidemic diffusion. But this point is one of no little difficulty; and it is exceedingly puzzling to account for the way in which great pestilences are occasionally ushered in or are followed by other diseases, as plague by malignant fever, cholera by diarrhœa, and both diseases by influenza.

But, whatever be the electrical or the atmospheric conditions of the air during epidemic visitations of cholera, there is no doubt that, in any given country, we can with tolerable certainty foretell both the locality and the class of people which will be chiefly affected.* The locality will be that in which, from situation, or from the habits of the inhabitants, the air is damp from the exhalations from rivers or marshes, and is at the same time rendered impure by the animal and vegetable exhalations which stream up from a crowd of people, ignorant or careless of sanitary precautions; and the class of people will be those who are subjected to these influences. These simple principles—so simple, that it appears almost

* To avoid repetition, we may refer our readers to the British and Foreign Medical Review for April, 1847, in which will be found a good deal of information on the conditions of diffusion.

unnecessary thus formally to announce them—have been proved by a multitude of observations, both in this country and in India. To some of these observations we shall have occasion presently to refer.

It must not be supposed, however, that, in addition to the immense influence of impure and humid air, the poison of cholera is not affected by other circumstances, which require much investigation. Thus, there is no doubt that a great influence is exerted upon it by soil;—it often adheres to the soil, and refuses to cross roads and narrow rivers—it has been supposed to avoid volcanic soils more than others—and it is a remarkable fact, that, in 1832, it is said to have made quite the circuit of Auvergne, which is altogether volcanic, but to have never penetrated into it. Sandy soils are also unfavorable to its spread; and this has been attributed to their absorption of water, and the consequent comparative dryness of the atmosphere.

But to the real or supposed influence of these several circumstances, we must return on another occasion; and we shall now proceed to analyse the evidence which the Commissioners bring forward, to prove the influence of impure and contaminated air on the spread of cholera.

"The disease which may be taken," write the Commissioners, "as the type of the entire class of epidemic diseases that infest this country, is typhus fever. The habitat of typhus is that of the class; and the conditions which favour the spread of this disease, and which convert it into a pestilence, and those which locate to a great extent in these very places all other pestilences that come, and which give them their fearful fatality, are, as far as we have any knowledge of them, precisely the same. It is now universally known, that in the metropolis, as in every town and city, the places in which typhus is to be found, from which it is rarely if ever absent, and which it occasionally decimates, are the neglected and filthy parts of it; the parts unvisited by the scavenger, the parts which are without sewers, or which, if provided with sewers, are without house drains into them; or which, if they have both sewers and house drains, are without a due and regulated supply of water for washing away their filth, and for the purposes of surface cleansing and domestic use. The evidence that the track of typhus is everywhere marked by the extent of this domain of filth, has been so often adduced that it is needless to repeat it; but the evidence that, during the prevalence of cholera, this was also everywhere the precise track of this pestilence, is not so well known." (First Report, p. 17.)

In the second Report, issued after the late epidemic of influenza, the Commissioners state that not only are the tracks of typhus and cholera identical, but that the same districts were those in which influenza manifested itself most severely.

"The cholera districts, the typhus districts, and the influenza districts are the same; and the local conditions which favour the spread, and increase the intensity of these and all kindred maladies, are everywhere similar. The proof of this is found in the fact, that in the districts in which we have already shown that cholera principally prevailed, and from which typhus is rarely if ever absent, influenza was twice, and in some instances, four times as fatal, as in the more salubrious parts of London." (Second Report, p. 35.)

It is necessarily understood that these remarks of the Commissioners are not to be interpreted quite literally. It cannot be meant to be asserted that cholera or influenza only prevails where typhus is; for such an assertion is contradicted by daily experience. There is a great difference in the volatility of these poisons, and in the rapidity of their diffusion; cholera is no more limited in this country to the localities where typhus

prevails, than it is to the countries where alone typhus is known. All that is meant is, that it so happens that the poisons of typhus, of cholera, and of influenza, all agree in this one respect, that contaminated air is most favorable for their development; and consequently, that, in such unhealthy districts, the deaths from cholera and fever will be found to bear a determinate ratio to each other.

As an illustration of this, we may cite the third Table given by the Commissioners.* In this table, 30 metropolitan districts are taken, and arranged in the order of the fatality of fever, in 1838, extracted from the Registrar-General's second Annual Report. The fatality of the cases of cholera in each district, taken from the Returns made to the Privy Council, in 1832, is then arranged in another column. The first and most unhealthy district is St. Saviour and St. Olave; here, in 1838, out of every 160 persons, 1 died from fever; in 1832, out of every 114 persons, 1 died from cholera; in other words, the deaths to population from fever and from cholera were as 1 to 160 and as 1 to 114. In Whitechapel, the deaths from fever were as 1 to 165 of population; from cholera as 1 to 113. In Bermondsey, the deaths from fever were as 1 to 206 of population; from cholera as 1 to 142. In St. George-in-the-East, the deaths from fever were as 1 to 208 of population; from cholera as 1 to 313. These returns may be contrasted with those from the healthier districts. Thus, in Kensington, the deaths from fever were as 1 to 522 of population; from cholera as 1 to 561. In Marylebone, the deaths from fever were as 1 to 479 of population; from cholera as 1 to 546. In St. George's Hanover Square, the deaths from fever were as 1 to 424 of population, and from cholera as 1 to 786. In Hackney, the deaths from fever were as 1 to 999 of population; from cholera as 1 to 916. Or, by classing together the 30 districts in two divisions of 15 each, it is found, that, in the most unhealthy division, the deaths from fever, in 1838, were as 1 in every 273 individuals; from cholera, in 1832, they were as 1 in 253 individuals; while, in the healthier division, the deaths from fever were only as 1 to every 494 of population, and from cholera only as 1 to 358. These approximative results are sufficient to bear out the assertion of the Commissioners; but, as if to show how cautious our generalization should be in such matters, there are some curious exceptions to this statement. Thus, in Shoreditch, the deaths from fever were as 1 to 256; from cholera only as 1 to 1203 of population. In St. Pancras, the deaths from fever were as 1 to 269; from cholera as 1 to 933 of population. In these two districts, therefore, the prevalence of fever surpassed that of cholera. On the other hand, in St. George's Southwark, the deaths from fever were as 1 to 321; from cholera as 1 to 91 of population; showing an extraordinary preponderance in favour of cholera. And other instances might be quoted did our space permit. But, in spite of these anomalies, it is abundantly evident that, as a general rule, in the unhealthy districts there is a preponderance of cholera as well as of fever cases.

It is a very remarkable circumstance, and one proving the uniformity of the poison of cholera, that the ratio of mortality to attacks of cholera is as great in the healthy as in the unhealthy districts. The average varied between 1 in 3·6 and 1 in 1·1 of those attacked. But, in the case of the

* First Report, p. 26.

the two divisions above referred to, the proportion of deaths to attacks was as 1 to 2·1 in the most unhealthy, and as 1 to 1·9 in the most healthy section.

On the point the Commissioners remark:

"Difference of social grade less exempts the individual from the attacks of cholera than of fever; and cholera more often, and apparently more capriciously, bursts its usual boundaries, and attacks the inhabitants of comparatively healthier districts, amongst whom it then proves little less mortal than when it ravages its accustomed haunts. If, as is justly remarked by the Registrar-General, in the present social condition of the civilized world, the vast populations of different and distinct nations are intimately united,—if it be true, that, were the health of India sound, Europe might be safe, and hear no more of the epidemic which is now traversing Russia,—if the lives of thousands depend on the condition of the Pariahs of Jessore,* much more in one and the same town and city, must the health of the wealthiest portion of the inhabitants depend on the salubrity of the poorest."† (First Report, p. 28.)

The districts we have cited as being those chiefly affected both by typhus and cholera, are, it is needless to say, disgracefully distinguished by a total disregard of sanitary regulations. Thus, the streets of St. Olave and St. Thomas, a district which stands first on the list of unhealthy places in the table referred to, are stated by the medical officer, "to be a disgrace to the civilized world." In Whitechapel, we are informed, "that the great majority of its confined and crowded streets, courts, and alleys remain without air, water, or any arrangements for the removal of its accumulated filth." (Second Report, p. 28.) The whole district of Bermondsey is reported "to be intersected by open ditches of the most offensive character, and abounding in several parts with 'fever nests.'" (Ibid., p. 28.) In Lambeth, where cholera also prevailed severely, it is stated that "there are numerous open ditches of the most horrible description; and, in general, the cleansing, paving, and water supply, and consequently the internal cleanliness, is either totally wanting or grievously defective." (Ibid., p. 28.)

We are extremely concerned to find how little has been done in the shape of sanitary improvement in these parishes since 1832. In Lambeth, Bermondsey, and Whitechapel, hardly anything has been amended:—Wandsworth and numerous other places remain without a single sewer. The Sanitary Commissioners remark that,—

"The improvements reported since 1832 have not been even commensurate with the increase of building and population, so that, in the present sanitary condition of the metropolitan districts, not only is there no reasonable ground to suppose, that, were cholera to reappear in them, it would be less extensive and mortal than on its former visitation; but, on the contrary, as the density of the population is in many parts increased, there must be a proportionate increase in the density of the deleterious agencies in operation." (Second Report, p. 29.)

Throughout England generally, the same influences of impurity and humidity of the air produced the same result. Thus, in Manchester and in the Lancashire towns generally, it is stated, in the evidence by Mr.

* We may here correct a slight error. Cholera appeared in several places, nearly at the same time, in the provinces of Dhacca, Sylhet, Chittagong, and Behar. It was months before it reached Jessore; but from this place the first official report was made.

† Dr. Starr's pamphlet, which is well and ably written, appears to be chiefly intended to show the necessity of sanitary regulations. It is written as much for the public as for the profession, and gives a good and sensible view of this part of the question.

Roberton, to have manifested itself chiefly along water-courses, including docks, wharfs, districts occasionally flooded, &c., and with peculiar violence at the outlet of drains.* In Sunderland, Leeds, Liverpool, Musselburgh, Edinburgh, Easter-Ross, and other places, the same fact was universally true—the greatest ravages of cholera occurred in places notoriously in a bad sanitary condition.

On the continent, the same law holds equally true. Perhaps in no town in Europe was cholera so fatal as in Paris, and perhaps in 1832 there was no town of so large a size and with such pretensions to civilization and refinement, which was so celebrated for its effluvia and its emanations. The internal arrangements of every house were of the most miserable kind; each house was dirty, ill ventilated, ill supplied with water, and wretchedly accommodated with privies and sewers. The single house was the representative of the city; the houses collectively repeated, on a gigantic scale, the nuisances of the separate habitation. In 1832 cholera commenced in the "Quartier de la Mortellerie," and in other places similarly badly situated and peopled by a miserable population. The mortality greatly preponderated in the low undrained localities.

We had believed that Paris had greatly improved in point of cleanliness since 1832; but M. Bureaud-Riofrey thinks otherwise.

"It would seem, at first sight," says he, "that one need little fear epidemics in our grand cities. At Paris, we admire the houses so splendidly built, the apartments so luxuriously furnished; we admire the richness of the furniture, the gilding, the glasses, the mirrors, the bronzes, the statues, the vases, and most recherché woods; everything would seem to exclude the idea of insalubrity. Yet, in spite of this brilliant exterior, Paris is perhaps the most unhealthy of capitals.—Paris wants air, water, cleanliness, above all, privies and sewers. It wants air in this sense, that almost all the houses are narrow, and the ceilings are low; in a Parisian apartment, we feel imprisoned. It wants water, because this is sold at an extravagant price; water is six times as dear in Paris as in London, where everything is so dear. Paris wants cleanliness, for hygienic measures are incomplete or powerless. Each house, and in every house, each flight, are the 'foyers' of vapours more or less fetid. The remains of food, the water of washings corrupted and surcharged with decomposing and fermenting organic matters, the emanations of workshops, the residue of manufactures, the accumulation of all excretions cast into the street, and turned and returned a hundred times daily by the seekers for rags; the dung deposited in heaps by the wayside, and diluted by the urine of a population little accustomed to restrain itself; this accumulation of ordure or of excretions in fermentation alters, in a very great degree, the purity of the air of this capital, which believes and proclaims itself to be at the head of the civilization of the world." (Du Choléra, &c., pp. 43-44.)

If this be so, and if, as there seems every reason to believe, a period of great social distress is impending over France, it is as well for Paris that the cholera has halted in its march, else the second visitation might be more fatal even than the first.

The beneficial result of good sanitary measures has been very apparent at Moscow during the present epidemic. In 1830, the disease extended to every part of the town; in 1847, it was confined chiefly to the districts south of the river, while in the other districts the cases have been few and scattered. The Russian physicians attribute this to the fact, that the population south of the river consists of the poorer

* First Report, p. 8.

classes, who subsist on miserable or raw kinds of food, and have only the impure river water to drink; while on the north side, the inhabitants are not only of the better class, but have most excellent drinking water, which is brought from springs 18 wersts from the city, by waterworks which have been erected since 1830. In Gallicia, the disease has spared the German villages, which are distinguished for their cleanliness,—while in other places it has been very fatal. It is reported that the colony of Sarepta, noted for the cleanliness of its inhabitants, and for its salt springs, has escaped in 1847 as it did in 1830.

If we pursue the same course of inquiry into Asia, it is found that, with certain modifications, the same rule may be laid down, and that the greatest ravages of cholera occur in places notoriously deficient in hygienic precautions. But it is at once evident that the rule must be qualified to a certain extent, and that the habitat of cholera cannot be so clearly foretold in India as in England. It is, we think, certainly established, that humid and contaminated air is the medium in which the poison chiefly propagates. But then it is evident that the contamination may be of different kinds. In this country the impurities are chiefly connected with our towns, and consist of the effluvia which stream up from dense masses of people paying little attention to cleanliness and to the removal of decaying substances. In India there is not only this source of impurity; there are also various exhalations from the soil. From jungle and marsh lands, from alluvial and cotton soil, from rice and indigo fields, from the muddy and slimy banks of the broad oriental streams, are poured into the air the various miasmata, which, in a greater or less degree, can cause the increase, as under extraordinary atmospheric conditions, they can perhaps cause the development, of the poison. In that country the necessary humidity of the air is abundantly supplied by the monsoons; and the burning sun, it may well be supposed, volatilizes into greater subtlety the gases of decaying organization. We expect then beforehand to discover a greater prevalence of the disease, and a more uniform diffusion. No town, hardly a village, situated in the lowlands, can be supposed to be so perfect in its sanitary conditions, as to be equally free from cholera with a town moderately well drained in temperate Europe.

And yet, there is here also abundant evidence to prove, that a bad or good sanitary condition produces a corresponding result of greater or less prevalence of the disease. This point was most clearly recognised by one of the best and earliest writers on Indian cholera, Mr. Jameson, the Editor of the Bengal Report. He was struck with the circumstance, that in 1817, the town of Muttra, situated forty miles higher up the Ganges, and more remote from the approaching pestilence, than Agra, was yet attacked first, and suffered more severely, both as to severity of symptoms and to actual mortality. Mr. Jameson remarks that, "Muttra is a filthy and crowded town, Agra, a dry and airy town." After his attention had been once drawn to this circumstance, Mr. Jameson took every opportunity of collecting information on this point. He found that in Sylhet the influence of situation was extremely well marked, and that the natives had universally recognised the fact, that the villages most unhealthy and exposed to the malaria from marshes and lakes suffered most. The same circumstances were found to be attended by the same results throughout

the whole of India; and a great deal of valuable evidence on this point is contained in the works both of Jameson and Orton.

We cannot afford space at present to enter into the evidence on this point, but we believe few people would credit the influence which in India these circumstances, *which are to a great extent easily removable,* have upon cholera. Hereafter we shall return to it; at present we must content ourselves with continuing the analysis of Mr. Thom's Report, and try to discover the causes of the terrible attack at Kurrachee, in Scinde, in 1846.

"The climate of Kurrachee has only been known for a few years, but in that time every European regiment that has been stationed here, has been more than once decimated by cholera, often within a few days." (Report, p. 10.)

What, then, are the causes of this great insalubrity? Do we find here the same influences of humid and impure air, which in Europe and in other parts of India play so prominent a part in the diffusion of cholera? Apparently we find them in a high degree; the climate of Lower Scinde is remarkable for its great humidity.

"It is curious," says Mr. Thom, "that rain hardly ever falls at this place, yet the air is for six or eight months very humid, and contains more aqueous vapour suspended in a cubic foot than that of Sierra Leone or the Mauritius, an island surrounded by a hot ocean, and subject to heavy rains. The evil of the climate, in my opinion is, because the atmosphere will not deposit its load of vapour in the form of rain, to cool the surface of the earth, and to lower the dew-point." (Report, p. 11.)

In addition to this great humidity, the thermometer in June and July stands as high as 88° to 94° Fahrenheit, at 3 p.m., and has a daily range of not more than 7°. Immediately before the outbreak in June 1846, these characteristic features of the climate existed in an intense degree.

"The thermometer was unusually high, being 90° to 92° in good houses, and in the tents of our soldiers it rose to 96°, 98°, and 104°. Secondly, the quantity of moisture in the atmosphere was greater than I ever saw it before in any part of the world, or at any season, the dew-point being at 83°, and the thermometer in the shade at 90°, the lowest range; even this gives 12·19 grains of vapour in each cubic foot of air. The mean heat in the 24 hours was such as to suspend an unusually large proportion of vapour in the air, always near, but rarely or ever reaching, the point of deposition. Even at the equator, with the sun overhead, I never saw the point of deposition above 78°."

The winds were westerly or from the sea, and were unusually light, instead of being, as in the two previous years, strong, steady, and cool. With regard to impurity of the atmosphere, the country about Kurrachee is dry and bare, almost entirely destitute of vegetation, marshes, or jungle, with the exception of a few stunted Euphorbiæ thinly scattered over the desert; and the town is about sixty miles from the Indus, and therefore probably removed from any influence in that direction. So far, we do not see much evidence of decaying organization; but in the town itself, and in the cantonment, are found abundant sources of effluvia.*

* It is necessary to state here, that Mr. Thom does not adopt the hypothesis of a specific agent for cholera, but believes that the atmospheric conditions of intense heat and humidity produce the disease. We shall hereafter discuss this point; at present we describe the conditions which Mr. Thom would consider as exciting causes, as furnishing a part only of the conditions of the atmosphere necessary for the diffusion of the poison.

The streets are narrow, crooked, without free current of air, and are very filthy; and a dirty bazaar is situated a few hundred yards to windward of the barracks. As Mr. Thom remarks, these circumstances were not the *causes* of cholera, as the disease had disappeared before they were remedied; but they must be allowed to have had great influence in favouring the necessary condition of the atmosphere.

Such, in June 1846, was the condition of Kurrachee: a still, hot atmosphere, overloaded with moisture; a dirty town and cantonment, the emanations from which were not precipitated by showers of rain. Slowly, in its mysterious course, the poison was spreading itself towards it, across the peninsula from the Madras Presidency, which it had ravaged the year before.

How was the English force prepared to receive it? In what state, as to health and condition, were the men? In what state, as to sanitary arrangement, was their cantonment? As to this latter point, the arrangement was certainly very bad. The barracks were built in rows; the canteen, schoolroom, and other high buildings, were in front of all, and nearest the sea; then the rooms for each company were ranged behind them; by this arrangement, the sea breeze was totally arrested by the front line, or if it travelled interruptedly and in languid puffs to the second, it never reached the third. Ventilation in the barrack-rooms was not apparently conducted on sound principles, and the soldiers were accustomed, at the time of the attack, to close both the doors and windows at night.* But if the barracks were badly arranged in 1846, the tents in which the 86th regiment and Bombay Fusiliers were quartered, appear, from one or two expressions of Mr. Thom's, to have been still more badly placed, and still more crowded. But on these points, as Mr. Thom's hypothesis leads him to attach little importance to hygienic conditions, he is not so explicit as we could wish.

We must here give an account of the force at the time stationed at Kurrachee.

(*a*) *There were stationed in barracks,*

1. 2d Troop Horse Brigade—135 strong. They lost 5 men from cholera, or at the rate of 37 per 1000 of strength.

2. Four batteries of Artillery—375 strong. They lost 37 men, or 96·6 per 1000 of strength.

3. H. M. 60th Rifles—980 strong. They lost 75 men, or 76·5 per 1000 of strength.

4. Bombay Sappers and Miners—249 strong. They lost 4 men, or 16 per 1000 of strength.

(*b*) *There were stationed in tents,*

1. H. M. 86th Regiment—1091 strong. They lost 238 men, or at the rate of 218 per 1000 of strength.

2. Bombay Fusiliers (Europeans)—764 strong. They lost 83 men, or 108·6 per 1000 of strength.

(*c*) *There were stationed in mat huts,*

1. 3d Regiment Bombay Native Infantry—991 strong. They lost 36 men, or 36 per 1000 of strength.

2. 12th Regiment Bombay Native Infantry—985 strong. They lost 66 men, or 67 per 1000 of strength.

* Report, p. 45.

3. 16th Belooch Battalion—820 strong. They lost 93 men, or 113·4 per 1000 of strength.

Or to take the different items together.

1. Those quartered in barracks were 1739 strong. They lost 121 men, or at the rate of 69·10 per 1000 of strength.
2. Those quartered in tents were 1855 strong. They lost 321 men, or at the rate of 172·84 per 1000 of strength.
3. Those quartered in mat huts were 2796 strong. They lost 195 men, or 69·20 per 1000 of strength.*

It thus appears that the two classes in barracks and mat houses had about an equal mortality; while in the regiments in tents, the mortality was considerably more than doubled.

And here we may inquire, in the first place, to what point the mortality of the whole force could have been reduced, with men in good bodily condition and under perfect sanitary arrangements. We believe that, under such circumstances, it would have been reduced to almost nothing. For example, the fearful loss of 637 soldiers out of 6380, or nearly 100 per 1000 of strength, strikes one with astonishment, when we compare it with the officers' loss. There were 200 officers, and only 3 deaths,† or at the rate of 15 per 1000 of strength; so that if the 86th regiment had suffered only an equal loss with the officers, there would have been 16 deaths instead of 238.

In the same way, the soldiers' wives suffered a loss of 23 out of 159, or 144·6 per 1000 of strength; while out of 42 ladies there was not a single case of cholera.

How far, then, would it be practicable to reduce the mortality to the point named? To answer this question, we must inquire into the causes which, in India in general, and in this attack at Kurrachee in particular, have caused such a disproportionate mortality among the men as compared with the officers.

In the tables above given, we included Europeans and Asiatics, but, for the purpose of simplifying the inquiry, it will be advisable to leave the latter people at present out of consideration.

Three English regiments, four batteries of artillery, and a troop of horse brigade, composed the European force. The artillery and the horse brigade, being comparatively small bodies of men, may be also left out of the account, so that there only remain the three European regiments to consider.

These regiments agreed in the following particulars:

1. They were composed of men of the same nation.
2. Their internal organization, duties, and habits were presumably the same.
3. Their diet and clothing were absolutely the same.
4. They were subjected to exactly the same influences of climate; they felt the same temperature, breathed the same air, and were exposed to the same winds.

And yet the 60th Rifles lost by cholera only at the rate of 76 per 1000

* This table is compiled from the staff-surgeon's return, at p. 20 of the Report.

† Of these three officers, "one died of congestion of the brain, subsequent to the attack of cholera; another was in so weakly a state, that any disease must have carried him off; so that only one case remains without palpable modification." (Report, p. 27.)

of strength; the Bombay Fusiliers lost at the rate of 108 per 1000 of strength; and the 86th regiment at the rate of 218 per 1000 of strength.

How is this astonishing difference to be accounted for?

It can *not* be accounted for by the following circumstances:

1. By any fancied exposure to contagion on the part of the latter regiment; because, as we have already proved, contagion has nothing to do with these attacks, and if it had, the 86th regiment had in this instance less intercourse with the town of Kurrachee than the 60th Rifles.

2. By any peculiar misconduct on the part of the most affected regiment. Mr. Thom states that the 86th were not more intemperate than the other two corps.

3. By any peculiar maltreatment of the sick, as this, of course, would not account for the number of admissions, and it is a curious circumstance that the ratio of deaths to admissions of cholera were only 39 per cent. in the Bombay Fusiliers, 58 per cent. in the 86th regiment, and as much as 63 per cent. in the 60th Rifles, the most healthy regiment of the three.

The cause of the disparity between the 60th, on the one hand, and the Fusiliers and the 86th, on the other, must be sought for in some common condition of the two latter regiments.

The first discoverable cause is, that, while the Rifles were in barracks, the other two corps were encamped in tents, "on a low, hot, and arid plain." Ten or twelve men "were cooped up in a 14 foot square tent."* Here, exposed to the burning heat of the sun, with the thermometer at 96° or 100° Fahrenheit, they remained from sunrise to sunset, unable to move out, in a still sultry air, and altogether deprived of the influence of the sea breeze.

Such was evidently one cause; a deficiency in proper accommodation led to the death of hundreds of men. Twelve men were breathing in a tent hardly large enough for one, the tent itself carefully secured from all means of ventilation, and the season unfortunately one in which the winds fell peculiarly light. In any point of view, this situation was hygienically bad. As regards the poison, it must have found in that close and contaminated air, with the humid atmosphere all round it, the very conditions most favorable for its rapid increase. As regards the human beings on which that poison acted, they must, in a variety of ways, have been rendered peculiarly susceptible to its influence.

These things are clear as daylight, they cannot be gainsaid; an immense increase of mortality was then caused by circumstances which could have been most easily removed.†

But of the men in tents, one regiment suffered much more than the other. What additional causes were then active here? The Bombay Fusiliers and the 86th regiment agreed with the 60th Rifles in having the same duties, dress, diet, habits, &c., and in being exposed to the same atmospheric agencies; they differed in the situation of their camp, hence a cause of difference of health; but the Fusiliers and the 86th agreed in

* Report, p. 26.

† It is, of course, understood that we make no objection to the use of tents, if numerous and properly pitched. Several times, when men have been crowded in barracks, small, old, and uncomfortable, as too many of them unfortunately are in India, and cholera has broken out, it has been checked by putting the men under canvas, and moving them about; but then, in this case, the very principles are followed out, for which we are contending,—ventilation and cleanliness are favored by the change.

the situation of the camp, and yet between these corps was a striking difference of mortality. How was this?

To answer this question we must briefly allude to one of the most important points in the whole history of cholera; the nature of the predisposition which permits the action of the specific cause. This point is not only important, but it is most difficult; because we are often at a loss to know whether the severity of an epidemic is owing to the intensity of the poison, or whether the state of the system is merely unusually favorable to its action. For the same causes may increase the intensity of the agent, and augment at the same time the susceptibility of the subject.

But in the present case, the intensity of the choleraic virus must have been nearly the same for the 86th regiment and for the Bombay Fusiliers, as they were encamped side by side, and equally under canvas. The Bombay Fusiliers had the advantage of being less strong than the 86th; and it clearly appears, from the observations of Dr. Lorimer, that the liability to attacks of cholera increases, cæteris paribus, in an arithmetical ratio to the number of men.

This could not have had much influence, however; and we are therefore compelled to look for the mortality of the 86th, not in the virulence of the poison, but in some cause which increased the susceptibility of the men.

What are the causes which, both in England and in India, give a predisposition to cholera?

1. As regards strength or weakness, it is not mere debility* as has been asserted by many, and especially by the writer of one of the works we are reviewing. The leading idea of M. Bureaud-Riofrey's work is the endeavour to explain the action of cholera by proving a previous debility on the part of those attacked. We doubt this exceedingly: first, because the evidence is very unsatisfactory; secondly, because if a person, no matter how robust, live in the choleraic district, he may be attacked; and thirdly, because in India there is positive evidence that the sick in hospital are not attacked in undue proportion.

2. As regards age, the data are not yet sufficiently accurate to enable us to lay down any rule. It has been always noticed that infants and children are comparatively exempt. In the 86th regiment, only 1 child out of every 14 was seized, and 1 in 18½ died; while the mortality among the women was 1 in 6, and among the men 1 in 4. Mr. Thom thinks that there is an increased susceptibility between the ages of 18 and 35; but we think that his numbers in the higher ranges are too few to warrant the generalization of his observation.

3. As regards sex, women generally suffer less than men, and this is obviously owing to diminished exposure. Among the women of the 86th, there was great exposure, and a proportionably increased mortality.

4. As regards previous disease, or previous habit of body, constitution, or temperament, nothing certain is known. Diseases which diminish oxygenation, as emphysema, chronic bronchitis, obstructive heart diseases, have been supposed to cause the predisposition. But the evidence is

* Mr. Thom says on this point: "Throughout the regiment it was the most powerful, muscular, and robust men that most speedily and generally fell victims to the malady; a fact that, with us, admits of no doubt. Any one who ever saw the splendid men in our flank companies, would appreciate the truth of this." Dr. Parkes says: "Impaired health is not a great predisposing cause, nor is robust health an effectual safeguard—the peculiar predisposition is independent of these circumstances." (Researches into the Pathology of Asiatic Cholera, p. 199.)

deficient. Bowel complaints probably predispose, particularly those having a choleraic character; but then these are perhaps more rightly considered as the early stages of the disease, and not as mere phenomena unconnected with, except in so far as they lead to, the developed disease.

5. As regards habits, intemperance has been considered a great predisposing cause; but here, too, the evidence is not stringent. In this country, it must be remembered that the poor, the badly fed, the badly clothed, the crowded, and the dirty, are generally intemperate. It is from these classes that the chief victims of cholera come; but are we to overlook the bad food, the bad air, the excretions both of lungs and skin impeded by want of cleanliness, and to fix only on a particular habit as the cause of the evil? The predisposition arises in such a case from composite causes, not from a single custom.

At Kurrachee, the men of the 86th regiment were not more intemperate than the Bombay Fusiliers,* or these than the 60th Rifles. It has been found also that in India, teetotallers do not show a greater diminution in the per centage of attacks, than can be accounted for by the fact, that they are often steady men and non-commissioned officers, and therefore placed under more favorable circumstances than the privates. We do not, however, deny that intemperance does in some measure predispose; but it is not a powerful cause in producing the constitutional state which permits the action of the poison. Mr. Thom makes some very good practical remarks on the system of moderate dram-drinking, in which soldiers indulge in India; but this is common to all regiments.

6. As regards diet; the influence even of this is doubtful. Europeans and natives in India pursue the most diverse plans, and yet all suffer; the Hindoo even more than the Englishman. The use of fruits, or of rich dishes producing diarrhœa, may occasionally have some effect; but we have no doubt that too much influence has been attributed to this cause.

7. As regards mental causes, the fear of the disease has been considered a predisposing cause; and we may safely allow this to have an effect in a few remarkable cases.

8. It is also apparent that, looking at the point from the most elevated situation, climate has little to do with the predisposition to cholera. For this disease has prevailed in every climate and in every season. It has ravaged towns in Russia, with the thermometer below zero of Fahrenheit; in temperate Europe its attacks have on some occasions equalled in violence the death-blasts which depopulate Asia.

We must consider, it appears to us, the predisposition as a peculiar unrecognised state of the body, resulting from the composite effect of age, strength, sex, previous health, temperament, diet, habits, and climate upon the system.

Let us then inquire if the 86th at Kurrachee had been subjected to any influence which may have heightened this predisposition over that of the 60th Rifles, or the Bombay Fusiliers,—regiments composed of materials so similar, and one of them at least placed in an analogous situation.

It has been long known that the state of the body produced by prolonged marching has an astonishing effect in increasing the predisposition.

Mr. Orton, who discusses this point minutely, adduces much important evidence; but adds that, although his "statements are abundantly suf-

* Report, p. 25.

ficient to show the increased prevalence of the disease arising from marching or travelling, they give no idea of the dreadful extent to which this simple, and apparently harmless, circumstance is capable of operating to the destruction of human life." (On Cholera, Second Edition, p. 383.)

Dr. Lorimer,* in his excellent Report, proves, by a series of very conclusive tables, that the number of attacks of cholera occurring on the march, increases regularly according to the number of miles, and to the number of days. The liability to attack increases so rapidly, that at the end of the 3d month the attacks of cholera occurred in the ratio of 61 per cent. of all the marches performed by the Madras Infantry; while the attacks in all the native part of the Madras service were 46 per cent. in marches over 600, and under 800 miles, and 75 per cent. in marches over 800, and under 1250 miles. Whatever be the causes of this susceptibility, there is no doubt that it is in part attributable to the great fatigue and exposure; since the officers, who are mounted, suffer comparatively little, and cavalry suffer less than infantry.†

To this powerful influence the 86th regiment was exposed in 1846.

"During four months," says Mr. Thom, "the men were under canvas, and moved by forced marches over 600 miles to Bhawulpore, 300 back to Sukkur, then for 9 or 10 days cooped up in fearfully crowded steamers on the river Indus, and after this marched 60 miles more into Kurrachee. In accomplishing this, the men had broken sleep, long marches, and few halts. The ordinary distances daily gone over, varied from 12 to 19 miles. The men were usually roused at one or two o'clock in the morning, in order to strike tents and get to the next encamping ground by seven, a.m. to avoid the morning sun. On arrival, they had generally to lie down wet with perspiration, till the tents and baggage came up. At the same time, while undergoing this fatigue, they were exposed to extreme vicissitudes of temperature." (Report, p. 25.)

During this march the regiment was healthy, and reached Kurrachee towards the latter end of April; the attack commenced on the 14th of June. Although in most cases the attack of cholera commences in a corps while yet marching, and ceases when it gets into comfortable quarters, yet, in the case of the 86th, after such a march, it may well be supposed that the influence would not speedily be dissipated. This unlucky regiment had been previously, it appears, peculiarly unfortunate.

"It landed in India in July 1842, and remained at Bombay in the wet monsoon of that year, and within a few months lost 100 men almost entirely from cholera.‡

* Report on the Epidemic Cholera as it has appeared among the Native Troops of the Madras Army, on the line of march from one station to another, from 1820 to 1844. By Alex. Lorimer, M.D. Published by Order of Government. Madras, 1846.

† When we state that marching has so much effect, we must admit that this general rule is subject to exceptions. The following is, perhaps, an apparent, rather than a real exception. In 1839, the right wing of H. M. 13th Light Dragoons marched from Bangalore to Bellary; during their halt at Bellary they suffered severely from cholera; returning to Bangalore, they joined the left wing, and the regiment marched to Madras. During this second march, cholera broke out. The left wing, which had been stationary at Bangalore, was 320 strong; it lost 35 men, or at the rate of 109 per 1000 of strength. The right wing, which had performed the fatiguing march to Bellary, only lost 4 men out of 208, or at the rate of 19 per 1000 of strength. (Report on Cholera in the 13th Light Dragoons, by Dr. Mouat, contained in one of the early volumes of the Madras Medical Journal.) It is very probable that the first attack of cholera had carried off the men most susceptible to the disease. It is well known that a regiment has ceased to suffer from cholera, simply from the exhaustion of proper subjects, while the virus has remained unaltered, as is proved by a fresh corps arriving and immediately commencing to furnish cases. In Mr. Orton's work are many instances of a similar kind.

‡ It is an interesting point, which we mention here, as we may not be able to return again to it, that, at Kurrachee, out of 50 men who had suffered from cholera at Bombay, 21 were attacked, and 9 died, or at the rate of 180 per 1000 of strength.

On being sent to the cool station of Belgaum, there was much dysentery at first, and just as the corps was getting healthy, it was removed to Scinde, where at Hydrabad, in 1844, it was again cut up by a summer's residence in that place, and lost 80 men in a few months. In 1845 it had a year's quiet and restoration in barracks at Kurrachee." (Report, p. 24.)

Even after their long march in 1846, the men were allowed no rest:—

"After months of active work," writes Mr. Thom, "a forced indolence, the usual animal diet, and large libations of bad arrack, did their work. Surely these were predisposing causes, and, as if not sufficient, brigade field-days once or twice a week were persisted in up to within a few days of cholera appearing in June. Had these field-days been in the morning, little mischief might have occurred, but they were in the evenings, so that after the lassitude of a long day in a hot tent, the men had to commence preparations for parade at 4 p.m. when the thermometer stood above 90°." (Report, p. 25.)

Predisposing circumstances with a vengeance; when will rulers learn wisdom, and believe that men are not made of iron? We recommend this passage to the consideration of the authorities at the Horse Guards; we should like to know what is there thought of field-days with the sun overhead, and the temperature in the shade at 90°.

The Bombay Fusiliers, on the other hand, were in a much better state of health; they had not long before arrived in Scinde, and though they had been to Sukkur, they went there and returned by steam, and only marched from Latta to Kurrachee, a distance of 60 miles. The 60th Rifles, again, had arrived in Scinde only six months previously, and had gone at once into barracks without making a single march.

It therefore follows that, both from our knowledge of the general laws of cholera, and also from the circumstances attending this very epidemic, we are entitled to draw these conclusions:

1. That the excess of mortality of the 86th regiment over the Bombay Fusiliers was attributable to the bad condition of health in the men of the former corps, consequent on hard service and long marching.

2. That the surpassing mortality of both these corps, compared with the 60th Rifles, was attributable to want of good hygienic arrangements, whereby the poison of cholera was allowed to increase in virulence, and the predisposition of the individuals exposed to its influence was also increased.

But we must now consider this mortality in the 60th Rifles a little more closely. In this corps, out of a strength of 980, 118 were attacked, and 75 died. Could this mortality have been reduced to that of the officers? Of 200 officers, 9 were attacked and 3 died; and of the 9 attacked, 4 belonged to the Bombay Fusiliers, and had been living in tents. So that, had all the officers lived in detached well-ventilated houses, the per centage might have been reduced still more. The houses of the officers were between the 60th barracks and the native lines, and therefore the exemption was not owing to more healthy locality.

It is unlikely that the hygienic condition of soldiers in India can ever be made equal to that of the officers; but still the inequality may be reduced. The barracks of the Rifles were ill ventilated and crowded.

"I feel persuaded," says Mr. Thom, "that this corps would have had very little sickness, had they been less crowded in barracks, and their rooms ventilated by

better arrangements, such as a medical board recommended at the moment the disease was breaking out, and had beer been issued instead of ardent spirits." (Report, p. 28.)

Of this we also are persuaded, and for this reason. The mortality among the natives in the town of Kurrachee was 1 in every 10 of population, that is to say, 1500 died of cholera in 6 weeks, out of a population of 15,000. This town, we are informed by Mr. Thom,—

"Consists of mud houses, with mere crannies as windows, or means of ventilation, while the houses are built so closely together, and the streets, barely wide enough to allow a loaded camel to pass, are so very tortuous, and inaccessible to currents of air, that all ventilation must be arrested, unless during a perfect gale of wind." (Report, p. 18.)

A new bazaar has lately been built at some distance from the town, in the cantonment, and is laid out in large "compounds, divided by wide streets, straight and at right angles to each other, and the houses and stores are well built, so that the general ventilation of the place is secured." The natives, however, and the servants of the officers have built their "narrow close huts" between the stores; and yet, in spite of this, ventilation is so much improved, that the mortality among these men, exactly of the same class as those in the native town, was only 1 in 30, or less than this. (Report, p. 18.)

Compare now these different rates of mortality:

1. The natives in the town lost 1 out of every 10 of population.
2. The natives in the bazaar lost 1 out of every 30.
3. The 60th Rifles, the healthiest of the three regiments, lost 1 out of every 13.
4. The officers lost 1 out of every 66.

Now we said we agreed with Mr. Thom, although we never saw the 60th barracks, for this reason. How comes it that the mortality in the 60th Rifles, so much less even than that in the other two regiments, was so fearfully above that of the natives in the bazaar, which was situated close to them? We cannot reply,—because they were Englishmen, and predisposed to the disease,—for two reasons. Because, if so, how was it the English officers escaped; and why did the natives in the town have a still higher mortality? It is a fair inference, which we do not see how to avoid, that the same cause which raised the mortality in the town, and diminished it in the bazaar and officers' quarters, gave it its standard in the barracks; and this cause was the relative purity or impurity of air. If this be the case, is it not disgraceful that our soldiers in India should be placed under such hygienic conditions? Does it not call for immediate inquiry, why one of the finest regiments in the service sees itself decimated by cholera, while close to it a body of natives suffer little more than a third of its loss?

Before closing this history of the Kurrachee epidemic, we must glance at one or two important points yet unnoticed. The remaining European force, to which we have not yet alluded, was composed of (*a*) a troop of Horse Brigade. These men had made the march to Bhawulpore, but being mounted, suffered little fatigue; they were placed in good barracks on their return, and lost only 5 men out of 135, or at the rate of 37 per 1000

of strength; (*b*) 4 batteries of Artillery, stationed in good barracks: they lost 37 men out of 375, or 98·6 per 1000 of strength, giving a mortality over that of the 60th Rifles. To account for this, Mr. Thom mentions that 3 of these batteries had just returned, we presume with the 86th, from a march of 1000 miles. We regret that we are not informed of the amount of loss in the battery which had remained at Kurrachee. If, however, it was really the march which produced the increased mortality among these men, it gives a kind of measure of the influence which marching exerts. Thus, even after a march of 1000 miles, these men in good barracks, suffered considerably less than the Bombay Fusiliers who had not marched at all, but who were exposed in tents.

The mortality among the native regiments varied considerably, as will be seen by reference to the table already given. The highest is that of the Belooch battalion, viz. at the rate of 113 per 1000 of strength, or over that of the Bombay Fusiliers; these men were in huts, and had returned from a march of 1000 miles. The 12th regiment Bombay Native Infantry lost at the rate of 67 per 1000 of strength; they had also returned from the same march, and were partly in barracks, partly in mat huts. We do not know to what to attribute this diminished mortality, unless to the circumstance of one wing being in barracks; but we should not like to assume this, as, unfortunately, we are not told the relative loss of this wing. But in the case of the other regiment, B. N. I., which lost only at the rate of 36 per 1000 of strength, we are entitled to attribute it to the fact of their having been stationary at Kurrachee for six months. It is also a curious circumstance, as regards intensity of disease, that the native regiment, as well as the European, which lost fewest men, yet lost most compared with the number of admissions; thus, in the Belooch battalion, the deaths to admissions of cholera were 47 per cent.; in the 12th B. N. I. the deaths to admissions were 50 per cent., while in the stationary and healthy regiment, the deaths to admissions were 55 per cent.

Although we attach so much importance to the purity or impurity of the air, as the only mode of explaining completely the differences of mortality in men under similar circumstances, we must also note that Kurrachee, independent altogether of cholera, must be an unhealthy station. This arises probably from the extraordinary humidity and heat of the climate. Among other diseases, scurvy is commonly seen at Kurrachee; in fact, till lately, lime-juice was issued to all the troops in Lower Scinde. And it is very interesting to learn, that scurvy became very prevalent after this terrible burst of cholera. Spongy gums, livid spots on the body, and great muscular weakness, occurred in many men who had not had cholera, and also in many convalescent from this disease. The 3d B. N. I., the freest from cholera of the native regiments, suffered more severely from scurvy than any; but in all the regiments the scorbutic diathesis was more or less marked. This is not the first time that scurvy has been noticed to follow cholera; the 22d regiment in Scinde, after losing 150 men from cholera, had a great many cases of scurvy.*

After prevailing at Kurrachee, the cholera spread towards Upper Scinde; but it never reached Sukkur, having apparently died away in the dry air of this province. It was moderately fatal in Hydrabad. Cholera is common at Latta on the Indus.

* Report, p. 34.

But as our space will not permit us to dwell longer on this Report, let us sum up our conclusions; in which, if we have stated our argument clearly, our readers will at once concur.

1. At Kurrachee, among the most favoured class of Europeans, as far as regards hygienic condition, viz. the officers and ladies, the mortality was trifling.

2. Among the most healthy of the three European regiments, the mortality was enormously increased over this ratio, apparently from bad accommodation.

3. The mortality was still further heightened among the other two regiments, 1st, by an insalubrious camp, and, 2dly, in the 86th regiment, by a low condition of health of the corps, brought on by causes well known to predispose to cholera.

4. There is good reason to believe that the mortality of the latter classes might be nearly reduced to that of the former; in other words, that the varying mortality in these several bodies of men is explicable only on the principle, that the poison of cholera derived its terrible power chiefly or entirely from the accessory circumstances which attended its attack. When these were absent, the poison became powerless.

Examined in this way, this epidemic at Kurrachee loses all its terrors. We see no longer the terrible burst of a mysterious plague, which seemed to be ushered in by whirlwinds and thunderstorms, and, like these mighty powers, to perform its work of destruction, unrestrained by human efforts. On the contrary, we see that it is our ignorance which has given it power, our carelessness which has prepared its easy prey. Although the necessity of service could not prevent the long march to Bhawulpore, we might have counteracted the effect of that march, instead of aiding it; although barracks could not have been prepared, we need not have crowded twelve men into a tent; although discipline must be preserved, we might have had some compassion on the men who had marched 1000 miles, and not have buckled them up for a field-day twice a week. We do not, for a moment, attach the chief blame to the authorities at Kurrachee; this frightful mortality does not lie at their door alone; we must all take our share of reproach. The medical profession is the most to blame; because it has not hitherto sufficiently recognised the simple causes which give such astonishing activity to morbid poisons. We have contemplated epidemics through an atmosphere of prejudice, which has distorted their form, and obscured their real proportions. But we are now beginning to learn; and if we do not deceive ourselves, we are, at last, on the right path to disarm of its malignity one of the most terrible plagues which has ever ravaged the earth. We have reassumed our proper character of preventers as well as curers of disease.

We can only spare a few lines to inquire into the means by which the Sanitary Commissioners propose to remove or lessen the humidity and impurity of the air, which aid the development of typhus, influenza, and cholera. In the metropolis, of course, the first thing is to ensure a more uniform and effectual system of drainage. Upon the practical details of this point, the Commissioners have accumulated much valuable information; our present inquiry, however, will not permit us to go into the analysis of this part of the Report. We regret to say that the difficulties in the way of a revision of the Metropolitan Sewerage, are neither few

nor small: but we trust, most sincerely, that energy and determination may vanquish the opposition of ignorant, apathetic, or interested men; and that the contest for pure and wholesome air may eventually triumph. Since the visitation of influenza, the Commissioners have directed their attention to the suburban districts. They discover, in the first place, that three of the suburban districts suffered as great mortality from influenza, as the most crowded part of London. Inquiring into this circumstance, they come to the following conclusions: the general characteristics of the places where the inhabitants suffered least from influenza, are dryness, warmth and shelter from the humid atmosphere; model lodging-houses, well-regulated prisons, and places of a like kind, having enjoyed a marked exemption. In the Westminster House of Correction, only 6 prisoners out of 700 were attacked. In the Pentonville Model Prison, the average of attacks among the prisoners was 1 in 6, among the attendants who sleep outside, 1 in 2½. In none of the prisons of the metropolis did a single death occur from influenza; but of course this observation is to be qualified by the fact of the very young and very old being excluded from the prisons. It also appears that the deaths from influenza in the clay districts of the metropolis, compared with "gravelly districts within a few miles of London," were as 3 to 1. Proceeding upon their belief, that influenza prevailed most in humid and exposed cold situations, the Commissioners call attention to the immense quantity of undrained or half-drained marsh land in the neighbourhood of London, particularly on the eastern and Essex side. The Essex and adjoining marshes contain about 3500 acres, or 5 square miles of undrained land, and upwards of 200 acres of ditches, many of them very offensive from the addition of sewage water. It appears, indeed, that even when the marshes are attempted to be drained, this is done in the most primitive way by open ditches; in one district in the Poplar Marsh, out of 520 acres, there are 21 acres of ditches; in the marshes of Greenwich, upon an area of 450 acres of land, there are 13 miles of ditches, or 1 acre of ditch to 24 acres of land. In the districts of the Surrey and Kent Commissions of Sewers, there are 70 miles, or upwards of 60 acres of ditches. The Sanitary Commissioners bring forward evidence to prove that in places drained within the last few years, the temperature has been raised, fogs have disappeared, and influenza, marsh fevers, rheumatism, and neuralgic pains have much diminished. They point out also the ineffective mode of the present system of draining, and the great loss of good land arising from the use of ditches; they therefore recommend, on the evidence of those well-known drainage authorities, Mr. Smith of Deanston, and Mr. Josiah Parkes, the abolition of ditches and the employment of covered drains.

Earnestly do we hope that the principles proclaimed by the Sanitary Commissioners, which, with necessary modifications of execution and detail, are applicable to all countries, may be encouraged in that land, from which issues the disease that has stimulated the labours of the present Commission. If, as we believe, a wise recognition of the conditions of existence of the poison of cholera can lead, in some measure at least, to their removal, and to a proportionate diminution in the mortality from this terrible scourge, then we hold it incumbent on the great Company, whose magnificent dominions are the garden of the earth, to avert from the Hindoo, in his close and fetid village, from the Englishman, in his

confined, crowded, and ill-constructed barrack, some portion of the evil, which has acquired magnitude and strength from our ignorance and neglect. The Company cannot alter the laws of Nature; they cannot prevent India from being the country, more than all others, subject to cholera, on account of its marshes, forests, plains, and rivers, its heavy rains, its burning sun; but they can do much to weaken the force of the poison, and to strengthen the frame against its influence.

But we must pass on to a very brief consideration of the next division of our inquiry.

III. NATURE OF THE SPECIFIC CAUSE.

The phenomena of great pestilences occur on so vast a scale, their consequences are so stupendous, their immediate action so impregnated with an almost universal suffering and fear, that men have found nothing to compare them with, but the most terrible and awful convulsions of Nature. The earthquake and the volcano seemed appropriate metaphors for diseases which changed the destinies of nations, and produced greater political and social changes than have flowed from the contentions or passions of man. In the middle ages, the comet or the volcano soon passed from a simile into a reputed cause;—in those times, when the social condition of the mass of the people of Europe was in the most degraded and miserable state, diseases assumed an extent of spread, a malignity of action, and a variety of type, unknown in these happier days;—coincidences between the outbreaks of such plagues, and the appearance of comets or the shocks of earthquakes, often happened; and the occasional real effect of a flood, or an unusually dry season, confirmed the writers of that period in the belief they had adopted.* And even at the present time, many of our best writers entertain something of the same belief. Everybody knows the lofty opening of Hecker's history of the Black Death;—there, as harbingers of great pestilences, the powers of creation are represented as coming into violent collision; the thunder mutters from beneath the earth; fiery meteors blast the fruits of the soil; the atmosphere burns with a sultry and unbearable dryness, or overflowing waters send up unwholesome mists; Nature spurns the ordinary alternations of life and death, and over the doomed people the destroying Angel waves his flaming sword.

And yet, after all, is there no poetical exaggeration in this? Is Nature indeed so hard a stepmother to us? Are these pestilences altogether independent of the passions, and uninfluenced by the condition, of man? We do not believe it. The causes of the devastations of diseases lie lower than the historian deems; it is in our false habits, our pernicious customs, our disregard of the fundamental laws of health and vigour, that we are to seek for the true reason of the mortality of pestilences. It would lead us too far to prove this, as may most easily be done, in reference to two of the great diseases of the day, bubo-plague and yellow fever; with regard to the third plague, cholera, our readers will, probably, think we have been already too minute.

But then if peculiar conditions increase the mortality of epidemic

* A good account of these coincidences is to be found in the "Chronik der Seuchen in Verbindung mit den gleichzeitigen Vorgängen in der physischen Welt, and in der Geschichte der Menschen. Von Dr. F. Schnurrer." Tubingen, 1825. Noah Webster has also collected, in two singular volumes, much curious detail on these points.

diseases, we are still as far as ever from accounting for them. These conditions are not the primary and specific agencies; they are only the accessory causes which give them power. What, then, is the nature of the indispensable cause of cholera? We think it a fair assumption that there is a specific cause or agent, because all the atmospheric causes that have been assigned may be excluded either in one epidemic or another, and yet the disease remains with unchanged characters. It is not great heat with moisture which causes cholera, because it has prevailed in cold countries in the period of their greatest cold; it is not excessive moisture, because it has prevailed with every degree of this, short of absolute dryness; there is no evidence that it is any electrical condition of the air, for this, at present, cannot be proved to undergo any change during an epidemic. But other and stronger arguments still, support the hypothesis of a specific virus, which we shall presently adduce. The Sanitary Commissioners do not touch on the nature of the specific cause of cholera; this was not in their province—they dealt only with conditions. Mr. Thom, on the other hand, devotes much space to a discussion on this point, and we shall now give his argument in as condensed a form as we can.

"By a combination of atmospheric agencies," says Mr. Thom, "peculiar to certain seasons in the peninsula of India, the human system is subjected to changes engendering a choleric diathesis, which, by great intensity of the remote causes, may run into the disease in its worst form. . . . . . . The atmospheric causes to which I refer are high temperature, associated with a degree of humidity of the atmosphere approaching to saturation, assisted by a decrease in the pressure of the air, and diminished proportion of oxygen." (Report, p. 4.)

The mode in which this atmospheric condition acts, is supposed to be in this way: the air charged with watery vapour is rendered specifically lighter, and, consequently, in any given bulk contains a lessened proportion of oxygen; thus air saturated with moisture has at 90° a specific gravity of ·88260, whereas at 32° the specific gravity is 0·99637; consequently, at the former temperature the volume of air inhaled by the lungs contains much less oxygen than at the latter, and taking into account that at 90° the quantity of water in saturated air is 0·04766, Mr. Thom calculates the diminution of oxygen to amount to about one sixth. At Kurrachee, in June, the dew-point nearly approached saturation. The quantity of oxygen is also diminished, by the fact that, in the tropics, the number of inspirations per minute is lessened.

The consequence of this deficient inhalation of oxygen is, that the Englishman continuing to introduce the same quantity as usual of carbonized and azotized matter into the system, which cannot be eliminated by combining with oxygen, carbon accumulates. The increased action of the liver and skin, however, diminishes this. But the secretion from the skin, which, in hot climates, is vicarious of the urine, is checked by the diminution of evaporation in the moist atmosphere. This state of things, then, produces the "choleric diathesis." The most intense condition of this state of things merges the choleric diathesis into an actual attack of cholera.

To the obvious objection that "a stagnant, hot, and moist atmosphere" does not prevail in all the countries which cholera has visited, Mr. Thom replies that—

"Affghanistan, Persia, Turkey, Southern Russia, and Central Europe form a

chain of countries which are exposed to alternations from cold and dry, to hot and moist currents of wind, resembling those of the Indian peninsula, but in a modified degree, except in extraordinary hot years, when the similarity will be perfect."

And at these times, therefore, we presume cholera will, according to the hypothesis, prevail in these places.

Such is the hypothesis* which is qualified in the next page, in rather an illogical manner. Alluding to the difference in mortality at Kurrachee, in the several corps, Mr. Thom remarks that—

"It will be necessary to direct attention to the circumstances, peculiar to certain classes, which in one case gave the malady an unexampled virulence and fatality, and in another, complete immunity, and such as happily are, in a considerable degree, under our control. The general atmospheric causes cannot be prevented; but, in all probability, it very rarely happens that these are in such an intense degree as to produce severe and prevalent cholera, without the *usual accessories, which appear to have lent a far more dangerous character to it than even the purely primary causes alone.*" (Report, p. 10.)

So that here the accessory causes which we have already detailed are absolutely raised in importance above the primary causes. But leaving this, the hypothesis appears to us in all points very doubtful:—it is not proved that in hot climates carbon accumulates in the system; it is not proved that the liver excretes it;† it is not proved that, even if it accumulates, it can produce cholera with its peculiar symptoms. One assumption is necessary after another—each one more doubtful than the former. Moreover, the arguments against the hypothesis appear to us very strong, even if we admit the truth of the assumptions.

1. Thus, how came it that at Kurrachee the officers and ladies nearly escaped, subjected as they were to the same atmospheric causes, and the former class not preserved by any material difference of diet from the men, or, at any rate, not protected to the extent we must admit to explain their exception?

2. How came it that the natives suffered from cholera, who were not introducing large quantities of carbonaceous food into the system?

3. How are we to account for the way in which these presumed atmospheric causes manifested themselves—first, cases appear isolated and apart from each other in the town for a fortnight; then they appear in the 86th regiment and in the Bombay Fusiliers, not for some days in the 60th Rifles, and still later, in some of the native regiments. The atmospheric conditions were common to all these classes. Mr. Thom cannot reply, that the early cases were those in which the "choleric diathesis" was most developed, and therefore that these were first taken ill; else why should not these have been distributed with a greater degree of uniformity over the whole place? If it is difficult to explain the succession of the several attacks by the hypothesis of a specific virus, it is still more so to explain it by the aid of atmospheric causes only.

But if in this attack at Kurrachee (which was so sudden and violent,

* It is curious enough that arguments, almost identical, have been adduced to prove that yellow fever arises from the action of heat, with or without humidity; English cholera and several diseases of the liver have been ascribed to the same cause. The true relation of heat to these diseases is only partially seized.

† It is by no means certain that the secretion of bile is increased in the tropics; if it is, still, as the bile is mostly absorbed again into the circulation, we do not see how carbon is to be got rid of in this way.

that it has seemed to those not acquainted with the details to be capable of arising only from atmospheric causes) there is good reason to infer the presence of a specific virus, undergoing, from peculiar atmospheric conditions, a development unusually rapid, in the general history of cholera are to be found much stronger and indeed conclusive arguments in favour of such a view.

Our space will allow only a very short enumeration of these:

1. Cholera has prevailed in a variety of atmospheric conditions; not merely in those presumed to be capable of retaining carbon in the system.

2. It has often been absent when the presumed atmospheric causes have been present in their greatest intensity.

3. It seldom commences or extends like an atmospheric disease; in illustration of this, we may compare it with influenza, which either arises altogether from atmospheric causes or possesses a specific poison of extraordinary volatility. Influenza appears at once over wide tracts of country, it attacks numerous individuals at the same moment; it is not perceptibly checked by seas; it is not bound to the soil, or connected with its exhalations, for it has attacked a ship's crew in the middle of the Atlantic; on the other hand, cholera travels slowly, often preferring the lines of communication between countries, probably because it finds there its conditions of increase most abundantly; it often adheres to the soil, and is checked by rivers or open spaces; when it enters a place, it selects almost always a particularly damp and dirty locality; it attacks a few individuals, then increases for a certain number of days, and then declines, or, if the conditions be favorable, it remains located in a place for months, and during this time it may ravage only a comparatively small section of a town.

4. This localization of cholera has always appeared to us the strongest argument in favour of the specific poison. Every one has seen cholera attack one side of a street, or one end of a town, and leave the other untouched, although atmospheric causes are common to the whole district. How is it explicable, except on the hypothesis of a specific poison, that a regiment in India shall be attacked with cholera in one locality, and shall throw it off, by marching a few hundred yards to a better locality? Or how can we explain the fact that the disease does not pursue straight lines, and will sometimes oscillate, so to speak, in a country, passing through it, and then returning through the portions it had left untouched, as in the Island of Salsette, in 1818? Or how can we account for a small section of land remaining often unattacked, although round it the disease is raging, the inhabitants in each place being of the same nation and having similar customs, as in the cases of Surdrep in the Sunderbund, Kristofsky at St. Petersburgh, or the Faubourg Leopoldstadt at Vienna?

Or take the march of regiments in India.—How, on the supposition of atmospheric causes, can we account for one wing of the 9th Lancers, as in the case formerly given, taking cholera at a certain locality, and losing it at a certain locality, and a month afterwards, the other wing arriving at the same districts, commencing to suffer, and ceasing to suffer at the same places?

What, indeed, is a common mode of attack in regiments on the march in India? A regiment is in perfect health till a certain date; it then hears that the next march will bring it into a district suffering from

cholera: as the district must be got through, the commanding officer pushes hurriedly on. In two or three days, however, cases appear, not in the regiment itself, but in that motley and ill-regulated assemblage in the rear, which, under the name of camp-followers, attend the march. It may be very fatal among these people before it attacks the fighting men, or it may be confined to them. Now in such a case, the atmospheric conditions may be absolutely the same for the infected and non-infected districts, and for the two classes of men.

5. Another argument is derived from the necessity of including cholera among the class of epidemic diseases, all of which arise from peculiar and specific poisons.

6. The progress of a case of cholera, its regularity, its similarity of feature to every other case, even the general uniformity of its mortality, point to a special agent.

7. Again, if cholera be ever contagious and propagated from one body to another, this can arise only from a specific cause.

Additional arguments and illustrations crowd upon us, but our space forbids us to enlarge upon them; our readers will doubtless be able to supply many more. Let us repeat that we are content to ground the argument for a specific virus on this single plea; that in a certain town cholera will occupy one section of it, will prevail here and here only for months, and will last through several seasons, thereby clearly indicating that its *existence* is independent of general atmospheric causes, although its *diffusion* is influenced by them.

Holding this opinion firmly, we make two qualifications:—First, we have argued the point on the supposition that atmospheric causes must be general, that is, must be diffused over a general extent of country—but in this we may be wrong; if in a given small space, either from humidity, from emanations from the soil, or from the relative bearings to each other of river and land, there may be some disorder in the atmospheric combinations, some allotropic condition of the oxygen (as that which produces ozone), produced either from the action of heat, electricity, or some similar agent, then the argument for a specific virus must be defended on other grounds. But of such a local change in the atmosphere there is no evidence.—Secondly. It is just possible that the cause may be generally diffused, and yet the inhabitants of a special locality only be affected, because, from the circumstances of that locality, only those individuals are possessed of the requisite predisposition; but such an explanation, on many accounts, is improbable, and indeed at variance with well-known phenomena.

But now, admitting, for the sake of the argument, this hypothetical virus,—what is it? what is its nature? is it organic or inorganic? is it a chemical agent produced by the catalytic action of an organic or inorganic power? or does it spring from germs which have a regular existence, and a certain life and death?

To answer these questions, it is evident that we must not look only at cholera. Cholera is but one of the links in that vast chain which connects all epidemic diseases, whether of man, of animals, or of plants. The inquiries which eventually will solve these difficult problems must have reference to an immense series of processes, of which cholera is both a section and a type. The theme is too extensive for our limits, as

it is too stupendous for our powers. On the present occasion, we shall content ourselves with giving a simple abridgment of Dr. Cowdell's views on the "fungous origin of cholera," reserving as far as we can a critical review of them for another occasion.

But we may remark,—

1. That it is almost impossible this agent should be a gas, else why does it not diffuse itself according to Graham's law? why does a road limit it, or why, like marsh miasmata, does it sometimes sweep along the lowest stories of a range of buildings, and refuse to rise many feet from the ground.*

2. That the power of reproduction apparently possessed by the agent of cholera, in common with all the poisons of epidemics, looks more like the property of an organic than an inorganic substance. It is hardly conceivable, that a mere chemical agent should multiply itself by acting on the living body. Dr. Babington, in the preface to Hecker, seems to doubt the fact of such a development.

"We are constantly furnished with proof," he says, "that that which affects life is not itself alive; and, whether we look to the earth for exhalations, to the air for electrical phenomena, to the heavenly bodies for an influence over our planet, we can perceive nothing which resembles the regular succession of birth, growth, decay, death, and regeneration, observable in organized beings."

But surely in smallpox, with its wonderful regularity of course, its incubation, its maturescence, and its decline, we see something like a fixed term of power and activity. How, except by regular growth, can we suppose the poison of smallpox, which we choose for illustration, as being the most definite of epidemics, to acquire suddenly, in the change from a cool and dry air to a hot and moist one, a most astonishing increase in the extent of its spread, and the violence of its attack?

But not to spend more time at present on these difficult questions, we will pass to Dr. Cowdell's hypothesis of the cryptogamic nature of the cause of cholera.

Feeling dissatisfied with the common and indefinite notions of epidemic agents, as well as with that hypothesis which attributes pestilences to microscopic animalculæ,† Dr. Cowdell was led to inquire what kind of analogy existed between the origin and progress of cholera, and the phenomena attending the development and diffusion of the lowest orders of the vegetable kingdom. Pursuing this conception, he found an apparent agreement between the origin, increase, and effects of the two classes of agents; and he has now furnished us with an analogical argument in favour of their identity. The same conception has been discussed by Henle and others, and in a manner somewhat similar; but this does not at all detract from the merit of Dr. Cowdell's ingenious and interesting hypothesis.

We need not spend much time over the first five chapters, which are occupied with an analysis of the history, nosology, nature, cause (as far as contagion is concerned), and treatment, of cholera—the writings of

* A good instance of this is related by Livingstone, in speaking of cholera in China. A number of individuals, sleeping on the *floors* of apartments, were attacked with violent cholera; others, in the same apartments, sleeping in beds, wholly escaped. (Transactions of the Med. and Phys. Society of Calcutta, vol. i.)

† Vide British and Foreign Medical Review, No. XLVI, p. 334.

Copland and Budd are taken as the foundation of the deductions under these several heads; and, of course, there is nothing very new in them. The chapter on contagion is the worst, because Dr. Cowdell has depended too much on his authorities, and has not consulted the original writings; had he done so, he would have omitted many statements to which he now attaches credence.

In the sixth chapter, the more original part of the work commences. The author first inquires what is the nature of fungi, and quotes a description of them from Lindley and Berkeley. The chief points insisted upon, are the simple form of their organization, which consists chiefly of cells and fibres; the minuteness and subtlety of their sporules, and their rapid growth; thus, in a single individual of Reticularia maxima, Fries has counted above 10,000,000 sporules, and some species have been known to grow several inches in the course of a night. They are frequently "meteoric, that is, springing up after storms, or only in particular states of the atmosphere," (Lindley). Their rapidity of growth, and their small specific gravity, ensure a very extensive diffusion, even if we admit that they cannot be spontaneously generated by special conditions of light, heat, earth, and air—a point for which many of the highest authorities have contended.

After these preliminary observations, Dr. Cowdell proceeds to arrange his argument under five heads, which we take in order.

1st. *The aptitude of fungi for entrance and localization in the human body*. The author starts with the remark, that the protein compounds of the body are nearly identical in composition with fungin, the basis of these low vegetable organisms. Therefore, the elements of the blood would be easily assimilated by beings of a fungoid or protophytic organization.*

He then alludes to the fact that we have evidence of fungi vegetating in living animals, and quotes from Dr. Carpenter the fact of fungous growth in the bodies of a species of polistes, the wasps of the West Indians, which are occasionally seen flying about with plants of their own length projecting from the surface, which have arisen from fungi introduced through the lateral breathing pores: the disease called "muscardine" which attacks silkworms in the south of France, and which is due to the growth of a minute fungus within the body, and which fungus can be inoculated, is also adverted to. Dr. Cowdell then observes that even in beings of the highest organization fungi have been found; as in the fluid in pyrosis, in the abdominal exudations of puerperal fever, in the exudations of dysentery, &c. He then quotes from the British and Foreign Medical Review, Berg's observations on thrush in children. It is hardly necessary to remind our readers that Berg describes the white coating of thrush as composed of thickened epithelium-cells, a molecular albuminous deposit, and a parasitic fungus, consisting of nucleated cells of various sizes, which afterwards develope into fibres; there may be either a preponderance of these cells (sporules), or a preponderance of ramifying stems or fibres, according to circumstances. Berg believes that these sporules may float in the air, and find a fitting nidus in other living systems; he has also succeeded in inoculating thrush. Admitting the truth of all these

* We are not certain, however, that we should not expect the poison of cholera to be something absolutely dissimilar and antagonistic to the organic compounds. It is the *opposition* of properties which produces the greatest results.

statements, Dr. Cowdell believes that in some at least of these cases, in the exudations of puerperal fever, for example, these fungi or their germs must have passed through the blood.

The only remarks we have to make on this chapter are: 1, that the observations are very few in number; 2, that the relative connexion of the disease and the fungi in these and other cases, as in favus, has not yet been determined. The disease may merely give them their habitat, as, in spite of Berg's experiments, we believe to be the case in some forms of thrush, in which the local disease of the mouth is but a sign of a general disease, which may also exist altogether without aphthæ. At any rate, the point is not yet cleared up.

Having thus proved that fungi *may* exist in the body of man, Dr. Cowdell proceeds to consider—

2. *The known effects of fungi.*—He first alludes to the "vegetating fungi," such as the dry rot of wood, the contagious "ropiness of bread," &c.; he then passes on to yeast, and adopts the opinion that the fermentation is owing to the growth of the Torula cerevisiæ. He also believes that all ferments will be found to depend on the presence of an agent, analogous to the yeast-plant. He quotes Mr. Graham's account of the peculiar putrefaction of the Wurtemberg sausages which become poisonous, and, being then taken into the stomach, probably enter the blood, and impart, by some catalytic power, their peculiar action to the constituents of that fluid. He thinks it probable that this is owing to a fungus; and concludes that all these changes, and perhaps even more recondite transformations, such as occur in animal nutrition, are accomplished by the catalytic agency of organized agents,—thus raising the importance of fungi in the economy of Nature to a very high point.

3. *The capability of fungi to produce the phenomena of pestilential cholera.*—The author does not profess to be able to be very complete in this part of his argument. "It will not be expected," he says, "that we should be able, from merely analogical proofs, to supply the minutiæ by which the doctrine of a fungous origin of pestilential cholera would be perfectly explanatory of all the known symptoms of that disease." (p. 142.) He first calls attention to the fact that all animals and plants are equally composed of cells, and have nearly the same ultimate composition. He then asks:—

"Why may not the germs of a fungus or alga be absorbed into the blood, and be so absorbed at a time when an extraordinary impulse has been communicated by external concurring conditions, probably meteorological, the blood, meanwhile, being in such a bio-chemical (electrical) condition, as to be able to oppose little resistance to the reproduction of fungi within it? And as it has been shown that these *can* live and grow within the human body, and in the fluid of it, we may naturally expect to find some of the effects described as fermentation taking place within the blood itself." (pp. 145-6.)

He then goes on to remark that in cholera, almost all writers believe that the first impression of the *materies morbi* is upon the blood. Now it appears probable, that the well-recognised poisonous fungi act also upon the blood, and in many cases produce symptoms resembling cholera; such as in the case of the Amanita muscaria and citrina, the Hypophyllum sanguineum, and ergot, which is said to produce "a gangrenoid state of the blood," &c. Ergot is next considered with its active agent, a fungus named by Quekett "Ergotætia abortifaciens." Perhaps the most

important argument in this chapter is derived from the action of the Amanita muscaria. A certain quantity of this fungus produces a peculiar drunkenness; this passes off in a very few hours, apparently from the active principle being excreted by the urine, for the urine becomes so highly impregnated with it, as to produce intoxication if drank by another individual. The effects of a single fungus, eaten by one person, have been propagated through five individuals, by the second drinking the urine of the first, the third of the second, and so on. So that here there really seems to be an increase in the quantity of the fungus during its passage through the living system.

The author also adduces other arguments, which, as we are not certain we could correctly interpret, we give in his own words. After alluding to the disease called Ergotism, and to the experiments of Mr. Quekett, and to Wiggers' analysis of ergot, he continues—

"Besides the disease here described as ergotism, symptoms of a peculiar kind have not unfrequently been observed to follow the medicinal use of ergot. . . . . . . In Mr. Quekett's experiments of inoculating healthy plants with the sporidia of ergotætia, we have an instance of contagion; while, in the viscous exudation from the flower, we have, perhaps, a phenomenon representing the viscosity of the peritoneum, or the albuminous exudation on the mucous membrane of the alimentary canal in pestilential cholera, the latter corresponding probably also in nature and in similitude of cause to the albuminous, aphthous crust, constituting thrush. In Wiggers' analysis of the excrescence of ergot, the undoubted product of a fungous growth, we have seen that there was a large proportion of fixed oil; in the account of the Wurtemberg sausages, we have read that the *increased greasiness* was one of the most remarkable of the visible alterations which had taken place in this variety of food, while the consequent wasting of the muscular fibre and of all the constituents of the body similarly composed, may be almost paraphrased by Dr. Christison's description of similar symptoms in gangrenous ergotism. In some infectious diseases, oiliness of the blood has been commonly remarked, as in plague and cholera. An abundance of fat has also been found in the fæces discharged in abdominal typhus." (pp. 158-9.)

Now we must say we think this line of reasoning rather far-fetched; it is, in fact, an instance of the great fault of the treatise, a desire to push analogical argument to extremes. Why should we thus violently connect together the fact of oil existing in the ergotætia and the Wurtemberg sausages, with the doubtful observations of oil being in abnormal quantity in the blood of plague or cholera patients, or in the fæces in cases of "abdominal typhus?"

On the whole, in this chapter, Dr. Cowdell is not so happy as in the former part of his work. The arguments he has adduced from the diseases produced by well-known fungi taken in large quantity, are not, it appears to us, applicable to pestilences. Dr. Cowdell then arrives at his next proposition.

4. *The consideration of the circumstances which probably concur to produce from the germs of fungi the effects of pestilential disease.*—Starting with the opinion that fungi spring up in particular states of the atmosphere, the author remarks that in all pestilences certain remarkable meteorological changes have preceded and attended them. With regard, more particularly to cholera, he refers to the attack at Kurrachee, which we have already discussed, and gives the erroneous account of it, to which we have referred as derived from the Bombay Times. He then

takes Kurrachee and Jessore in Bengal as good examples of the places, which may be presumed to be most prolific in fungi. In both places, he says, the moist climate, the alluvial soil, the incredible quantity of vegetation, giving rise to great electrical disturbance, would probably give occasional impulse to the wonderful reproductive power of these beings.* He then alleges that the state of electricity, which evidently exerts a great effect over the production of fungi, may have an effect also in predisposing to, or shielding the human body from, their influence; as an illustration of his meaning, he refers to the adherence of certain vegetable powders to the lines traced by a magnet on wood, while from other parts of the wood the powder is easily blown away.

The eleventh chapter of the book is on the "identity of remedies recommended in pestilential cholera with anti-fungic agents." Bichloride of mercury, zinc, copper, tartar emetic, aromatic substances, volatile oils, are all anti-fungic agents, and all have been found useful in cholera. Dr. Cowdell justifies his use of this argument, which seems to us peculiarly weak, by a reference to the diagnostic effect of colchicum in gout, or of bleeding in certain diseases, in which the good effect of the remedy is held to justify the diagnosis. But in cholera, unfortunately, no remedy is unequivocally and specifically useful.

Looking back now to the whole course of the argument, we are inclined to come to the following conclusions:

1. That the phenomenon of a rapid cell-growth appears to be a good type of what we may suppose to be the mode of increase of the specific virus, presuming it really to exist.

2. That the simple fungi appear, from their rapid growth and their known diffusible properties, to be the most likely class in which to seek for agents multiplying themselves in this way; and that the remarkable catalytic power of these organisms renders it not unlikely that, if absorbed into the blood, they might, by germinating there, produce changes somewhat analogous to fermentation.

3. But that even the study of this class has not led us to believe that any of its species can be of the almost infinitesimal size, which it is clear epidemic agents must be; hence that any species could produce the symptoms of cholera and of other epidemic diseases is, of course, a mere assumption unsupported except by analogical argument.

4. That in the case of poisoning by fungi, it is the entire plant which is poisonous, and not the sporules; and then it must be taken in doses of enormous quantity, compared with the amount which could be introduced into the system were the fungus transported by the atmosphere.

5. That we must entertain with great reservation the argument from analogy, that because fungi *may* find a nidus in the human or other body, and that some fungi are poisonous, or produce, when germinating, peculiar chemical effects, therefore there may be other more minute fungi still more powerful and poisonous, and capable of finding a nidus in the blood of patients in a particular electrical state.

6. Still, with all the doubts which every one must feel in these difficult matters, we think the doctrine of the fungous origin of cholera more pro-

* We may remark here, that cholera commenced in several places before Jessore, in 1817; and that it is very doubtful whether, as Dr. Cowdell supposes, it was generated *de novo* at Kurrachee, in 1846, since it had been gradually approaching that place from Bombay. At Kurrachee itself, also, there is no great excess of vegetation.

bable, based on stronger arguments, and on more scientific foundations, than the animalcular theory of cholera.

The great difficulty under which Dr. Cowdell has laboured, has been want of sufficient material; his hypothesis, if true, is as yet insusceptible of determination; it cannot be proved or disproved; it must abide its time; still, with the scanty number of facts yet known, Dr. Cowdell has made a strong argument, and has written an interesting and instructive work. We should like him to apply his hypothesis to all epidemics. He would perhaps find yellow fever and plague still more to his purpose than cholera.

But our space warns us that we must hurry on to our fourth heading:

IV. RELATION OF ENGLISH AND ASIATIC CHOLERA.

We may, first of all, give a short epitome of the contents of a clever little book by Dr. Spencer Thomson, on British Cholera. We cannot, however, at this time enter so fully into the subject of English cholera as its importance deserves.

Dr. Thomson introduces his subject with a few observations, in which he points out the importance of the disease, and the method he has pursued in investigating it, which has been mainly by an attentive study of the earliest periods of the disease, before the advent of vomiting and purging, which he considers to be merely the natural terminations of the affection. Curiously enough, Dr. Thomson takes somewhat the same view of English as Mr. Thom of Asiatic cholera, and believes it to depend upon the accumulation of carbon in the system, athough he has not proved this by chemical analysis. Dr. Thomson's observations were chiefly collected during the summer of 1846—a season remarkable for great and unusual heat; English cholera, as is well known to all, having been in the autumn of that year unusually prevalent. The Registrar-General's returns, ending September 1846, show an excess of mortality over the corresponding quarter of 1845, of 15,227 deaths. In London, the average mean temperature for this quarter was 63·10, being an excess of 6° over the corresponding quarter of 1845. The excess of deaths in London was 1567; and of these no less than 1303 were from cholera, diarrhœa, and dysentery. Therefore the author concludes that the high range of temperature, and the prevalence of cholera, stand related to each other as cause and effect.

The mode of the relation is thus explained: almost all the waste carbon of the system appears, if Liebig be correct, to be got rid of by the lungs, where it combines with oxygen—a certain balance being always maintained between the one element and the other; our readers are too well acquainted with Liebig's views, to render it necessary for us to go into detail. But as the quantity of inspired oxygen varies with the temperature and density of the air, the amount of watery vapour, the number of inspirations, &c., in hot seasons and in hot climates, so, if carbon be still introduced in the same quantity as in cold climates and seasons, it must accumulate in the system. Whether this be the case or not, the symptoms said to be present in early periods of cases of cholera, such as overpowering drowsiness, headache, dusky skin, high-coloured urine, and catamenia, are such as would result from an excess of carbon; and, in a mild degree, are very similar to those which result from poisoning by charcoal, or are seen in some forms of jaundice, in cyanosis, &c. Even intense cold and drowning produce effects evidently of an analogous kind.

The author then remarks, that, although heat is the great primary cause, its action is greatly assisted by certain subordinate causes. He first alludes to diet, quotes Liebig's well-known opinions on this point, and insists strongly on the pernicious effect of alcohol in hot seasons and countries, as it not only increases the necessity for oxygen, but also diminishes the excretion of carbon. He then enters on the subject of the purity of the atmosphere, particularly as to the presence or absence of carbonic acid, which, with other compounds of carbon, he believes to be generated in large quantity from sewers, cesspools, dunghills, &c., during summer. Lastly, the author alludes to the function of the skin, and dwells on the increased necessity of keeping this in a fit and proper state during seasons of elevated temperature.

We may sum up the author's hypothesis in a few words. Great heat diminishes the excretion of carbon by diminishing the volume of air and the number of inspirations. In addition, this state of things may be aggravated by the ingestion of more carbon with the food than can be burnt off, and by the fact of the air, during hot seasons, being contaminated, particularly in large towns, by the evolution of different compounds of carbon.

The symptoms of English cholera are described with care, particularly the premonitory stage, which we shortly detail. This stage, it is stated, may last for weeks before those violent symptoms of vomiting and purging ensue, which are usually considered to be the disease.

"All the premonitory symptoms indicate derangement of the liver; there is general feeling of lassitude, and inaptitude for exertion; headache, especially after meals, heavy sleep at night, and *constant, insuperable drowsiness in the daytime;* mouth clammy, dry, and tasting disagreeable and bitter; a painful sense of fulness and distension, extending from the epigastrium into the hypochondrium; pain constant or intermittent between the scapulæ, extending up the back of the neck; bowels irregular, occasional griping; urine generally deeply tinged, and depositing urate of ammonia, of various shades of colour, or deep-coloured uric acid; skin dusky, not yellow." (pp. 10-11.)

The author states, that, in all the cases of bilious cholera these premonitory symptoms occur; at length the accumulated carbon finds its outlet in the violent bilious vomiting and purging which is the natural cure of the disease.

We need not describe the severe attacks, as we observe nothing novel in their description.

In the chapter on Treatment, the author refers particularly to a proper diet, and advocates the use of fruits, as prophylactic measures. If medicine is necessary, mercurials, which often act with some violence, may be taken. In the severest forms, when the disease simulates Asiatic cholera, opium is the remedy he chiefly uses. His other measures we need not detail.

The author next enters upon the connexion between Asiatic and British cholera, and argues the question with much skill. As this is more germane to our subject than the previous chapters, we shall enter into it a little more minutely.

Before two diseases can be compared, it is necessary to have some tolerably definite opinions about the etiology and pathology of both. If we are not sufficiently acquainted with a disease, to distinguish primary

from secondary symptoms, if we raise incidental phenomena to the rank of constant and necessary occurrences, and if at the same time the causes are obscure, and the anatomical signs unusually indistinct, we are almost certain to fall into some error in comparing such a disease even with one which has only a superficial resemblance to it. Now Asiatic cholera is certainly only partly described; we understand something about it, and we know the probable direction of future discoveries; but we are not yet sufficiently acquainted with its pathology to speak without reservation on a single point. We shall, therefore, merely allude to the more obvious differences between it and the disease commonly called British cholera.

1. The causes seem to be different in the two diseases. Nothing is more certain than that Asiatic cholera spreads and extends itself; it does not appear at any particular time of year, and is limited to no locality. English cholera is remarkable for the tendency to occur only at the end of summer; it occurs then, as Sydenham says, as regularly as swallows in spring. This character has always appeared to us to favour the opinion that Sydenham's description applies not to Asiatic but common English cholera, which, like all other epidemic diseases, was more severe in his day, from the bad hygienic condition of the population.* English cholera, moreover, does not travel, and is evidently connected with increased temperature, in some way or other.

2. The symptoms of the two diseases are in most cases strikingly dissimilar. Thus as to the manner in which they commence: the attack of Asiatic cholera is sometimes sudden; if preceded by a premonitory period, this is of short duration; whereas it appears, if Dr. Spencer Thomson's observations be correct, that in English cholera there is a long period, during which the system is becoming more and more out of health under the influence of hygienic conditions, and not from the incubation of a specific poison, until the outbreak takes place, which is the cure of the disease. Again, in the future progress of Asiatic cholera, it has been clearly proved that the vomiting and purging are only secondary symptoms, and in the purest forms of the disease may be absent. In addition to the evidence already before the public, we may detail in this place Mr. Thom's observations on the forms of disease seen at Kurrachee in 1846—observations which correspond exactly with those made by preceding writers.

"1. Among the first 100 cases which occurred," writes Mr. Thom, "many died in a few hours, and some in less time; one man, I am told, went off in less than an hour. In these vomiting and purging were not always present. Sudden collapse, ending in profuse sweating, were the most prominent symptoms—in fact, as if asphyxia had already taken place. It was often found that the pulse had ceased at the wrist, the eyes were turned up, and the voice hollow and feeble, before the natural hue had fully given way to that horrible lividity which is so characteristic of the disease—so instantaneously was the power of life arrested. Spasms of the muscles were very generally, but not always, present, in these early and more severe forms of the disease; in fact, not a few sunk almost without much suffering or complaint, but lay down to die with an apathy scarcely credible.

"2. The next class of cases was those in which the first seizure was equally sudden, and the collapse preceded the vomiting and purging. There were sudden

* Although we have not entered into the question as to the nature of the cause of British cholera, our own opinion inclines to the belief that this disease does not arise from heat only, but from that and some other conjoint causes, connected particularly with our sanitary deficiencies and our peculiar climate.

faintness, prostration of strength, restlessness and anxiety, accompanied by vertigo, deafness, loss of vision, alteration or hollowness of the voice, weak and slow respirations, performed convulsively or in sighs. These were followed by nausea, vomiting, and purging of conjee-water stools, sensation of burning heat at the præcordium, intense thirst, and desire for something cool. The circulation became impeded, especially in the extreme vessels and capillaries, and still more in the vena portæ; the collapse rapidly progressed, the expression was of extreme anxiety, amounting to agony, accompanied by restlessness and jactitation, the features became shrunk and cadaverous, the lips and skin assumed a livid colour, and the whole body, bathed in profuse cold sweats, soon yielded to dissolution. The respiration was slow, feeble, and irregular, being performed with convulsive starts, and with very little dilatation of the chest to receive any quantity of fresh air. Perhaps nothing was more uniform than the desire to be exposed to fresh air or a current of wind, and to have every covering removed from the body. The spasmodic action of the muscles was most severe in the first stages, and was not confined to the extremities, but affected every voluntary muscle in the body.

"3. Another class were attacked with vomiting and purging of rice-water stools and copious sweating; cramp of legs, arms, and abdominal muscles, rapidly producing prostration of strength, and collapse, with all its train of symptoms, were consecutive on the discharges.

"4. Lastly, as the disease was subsiding, cases began to exhibit signs of reaction. In general, these cases were much more manageable than the others; and when the purging and vomiting were once arrested, a reaction in fever took place."*

Of the	first	100	cases,	79	died.
„	second	100	„	66	„
„	third	100	„	50	„
„	fourth	100	„	50	„

Mr. Thom calls particular attention to the exceeding abundance of the sweats. He also states that, out of the 400 cases in the 86th regiment, "30 were followed by relapse once, and about 8 of these by a second, which generally proved fatal." We should extremely like to have the clinical detail of these cases. Mr. Thom also remarks that "the greater proportion recovered without any consecutive fever, this I was not prepared to expect; the number attacked with fever, which was of a low congestive typhoid type, was as 1 in 8."†

This description agrees closely with the general opinion as to the real nature of the disease, which consists in a more or less complete arrest of the circulation, with escape of some of the blood-constituents from free surfaces, in the inverse ratio to the degree of arrest of the circulation, and consequently to the intensity of the disease. The vomiting and purging are then, in Asiatic cholera, only secondary symptoms; in English cholera, they mark the utmost severity of the disease: when English cholera kills, it is by the exhausting nature of these discharges, and not from the action of some agent, whose highest influence is quite unattended by discharges in any notable degree.

The nature of the discharges is, too, in many cases, quite different. Bilious purging never occurs in Asiatic cholera; in fact, the liver appears but little in fault, whereas in English cholera we certainly look to some deranged action of the liver, as well as of the gastro-enteric membrane generally, as being the precursors of the disease.

We shall only devote a few words to the subject of our fifth heading.

* Report, pp. 30-1. † Ibid., p. 41.

V. AS TO CERTAIN SUGGESTIONS OF TREATMENT PROPOSED BY THE SANITARY COMMISSIONERS.

The chief novelty in these suggestions consists in the importance which the Commissioners attach to the premonitory symptoms, particularly purging of watery fluids. They bring evidence to prove that a certain period occurs in many instances before the advent of an attack of cholera, in which the system is gradually approaching the state favorable to such an attack; this period is denoted by the occurrence of watery diarrhœa, and if this can be arrested, the impending disease is also often turned aside. They, therefore, recommend the establishment of dispensaries, at which, as a matter of precaution, every person may apply immediately on the occurrence of symptoms of diarrhœa. There can be no doubt of the existence of a premonitory stage in many cases: it was well described by Annesley, and by several writers since his time; and it is also certain that it can be often, and at once, arrested. But whether it would be practicable, in a dense population like that of London, to act on this knowledge, we do not know.

The Commissioners object to the establishment of cholera hospitals, on grounds which do not appear to us altogether sound. Thus they state that in the cholera hospitals, the mortality was much greater than in private houses. This we can readily believe; because the class of patients who went to the hospital, were the destitute, the friendless, and the poverty-stricken; they were often taken great distances, and the hospital which received them was no better than their own miserable homes. As an illustration of this we may extract some portion of the evidence.

Mr. Bowie is asked by the Commissioners,—"What were the character of the hospitals that were provided?—They consisted of dwelling-houses, taken for the purpose of affording temporary accommodation; the one that came particularly under my notice was very badly ventilated, and situated in a bad locality, that is, close to the Hermitage, and near the bone vessels already spoken of."*

Mr. Hooper states that he was surgeon to the cholera hospital in St. George's Fields in 1832,—out of 138 cases admitted, 117 died. "They were not, however, taken there till they were in the last stage of the disease."† This gentleman says, the hospital was "constantly full, partly from those who had no friends, and partly from those who were sent there by families that were afraid of contagion."‡

Mr. Simpson, of Bloomsbury, says, "the hospital did not seem to lessen the average number of deaths. Its being in the same locality, and subjected to the same atmospheric influence, prevented that result."§

Dr. Wright is asked as to the beneficial result of the cholera hospital. He replies, "we had the means of giving much greater attention to the cases, than the poor could have received at their own houses, and I certainly think some degree of benefit was experienced by this alteration."‖

Mr. Wagstaffe says, "by the congregation of great numbers in the cholera hospitals, I think they must have been injurious."¶

It does not surprise us to learn that the mortality in the London cholera hospitals was so great, when we find, from the Commissioners' evidence,

* Appendix, No. 1, Report, p. 93.
† Appendix, No. 2, Report, p. 109.
‡ Ibid., p. 111.
§ Appendix, No. 6, Report, p. 118.
‖ Ibid., Appendix, No. 10, p. 132.
¶ Ibid., Appendix, No. 4, p. 116.

that they were badly situated, badly ventilated, very much overcrowded, and were filled with the worst cases from the poorest and most miserable population.

But we conceive the Commissioners have made out no case against good hospitals in good districts, in which cases could be received before the latter stages, when removal, if not immediately dangerous, is eventually hurtful. We conceive it to be almost impossible, that during an epidemic the poor could be attended at their own houses, without an enormous staff of medical men and nurses. In an hospital there are, both for the attendants and the patients, all the advantages of combination. Besides, we hold it to be an important indication to remove cholera patients from the locality in which the poison which has attacked them still exerts upon them its fatal influences; and even apart from this, those who know the confined and baneful dwellings of the London poor, will deem it necessary for the favorable issue of cholera, as of almost any disease, to insure them, in the first place, one of the important necessaries of life and health,—an abundant supply of pure and wholesome air. How can this be accomplished in the present day without cholera hospitals?

But we must draw this article, already too long, to a close. We will conclude it with a single remark.

The true philosophy of the science of medicine is the knowledge of the causes of disease. Or, if these causes be too subtle and refined for our gross senses, it is the knowledge of the several conditions, external or internal to the body, which give those causes power. In the future history of medicine, we shall see men returning to the principles promulgated by its earliest founders. They will perceive that the treatment of the fully formed disease is at the same time the most difficult, and the least useful part of their noble profession. They will learn to arrest the evil at the fountain-head, and not to dam the current swollen by a thousand tributaries. And if the principles which we have analysed in this article be correct, it will not be the least triumph of this philosophy, that it has indicated the true mode in which the great epidemic of our time can be most easily and most effectually controlled. It bars out the disease, not with quarantines and cordons sanitaires, but with a cleanly people and uncontaminated air. The evil which springs from the bosom of Nature only needs for its removal an observance of the rules which Nature herself reveals.

ART. VI.

1. *On the Archetype and Homologies of the Vertebrate Skeleton.* By RICHARD OWEN, F.R.S. With two Plates and numerous Wood-Engravings.—*London*, 1848. 8vo, pp. 203.

2. *Comparative Osteology; being Morphological Studies to demonstrate the Archetype Skeleton of Vertebrated Animals.* By JOSEPH MACLISE. With fifty-four Plates.—*London*, 1847. Folio.

THERE is no one branch of philosophic anatomy so important or so interesting as that which treats of the neural skeleton. It constitutes, in fact, the very basis of all studies having for their object the structure of the vertebrate sub-kingdom; whilst, by the nature and extent of the inquiries essential to its proper cultivation, no less than by the fundamental

principles it reveals, it reflects a clear and steady light upon the whole science of organization. This being so, it is a matter of much congratulation, that at this particular epoch, when, as by common consent, a new and more enlightened spirit is being infused, not only into the higher departments of medicine, but even into its more elementary branches, a master-mind has arisen among our ranks, admirably qualified to give to the rising members of our profession sound and scientific instruction in human and comparative osteology. We render nothing more than a fitting tribute to justice, when we affirm that no individual has contributed so much to create and to sustain, in this and other countries, an elevated taste for anatomical research, as Professor Owen.

In the present year our distinguished countryman has presented to the scientific world unquestionably the most complete and philosophical work that has ever appeared in this or any other part of Europe, in relation to organic science. We allude to the treatise on the archetype skeleton, which embraces all his earlier researches, first communicated in the Hunterian lectures, on the osteology of the class pisces, together with those subsequent investigations into the other vertebrate families, which formed the subject-matter of the celebrated "Report" presented to the British Association in 1846. Medical literature is thus enriched with a separate and complete essay on this deeply interesting inquiry; and we are only rendering our readers a service by directing their attention to the publication now before us. To all medical and scientific libraries and societies this volume is quite indispensable; and we trust, for the best interests of science, that its riches will speedily become accessible to all our professional brethren.

Although this subject has been noticed by both our predecessors, yet as Professor Owen, in the present work, has introduced a most interesting historical and critical review of the opinions of former investigators, we are desirous to avail ourselves of this occasion to offer some general remarks on the vertebral theory, before noticing the new matter contained in the treatise which has lately issued from the press. And this has become the more necessary at this particular time, since the increasing attention that is directed to the higher branches of organization renders it desirable that just conceptions should be formed of the nature and scope of the inquiry. It has happened, and in this country especially, that the opinions of those who have engaged in the study of philosophic anatomy have almost exclusively been influenced by the teleological principles of Cuvier. Now, although it is impossible to overrate the importance of those principles in the investigation of the uses and inter-relations of the animal organs, yet, in the interpretation of structure, in its essential and typical characters, the doctrine of final causes, if assumed as the fundamental guide, is sure to mislead. To those of our readers who have vividly impressed on their memory that brilliant chain of argument by which the great modern Zoologist traced out, from the inspection of a jaw, or even part of a jaw, the general features and habits of the animal to which it belonged, it may, indeed, appear a somewhat strange assertion that the principle on which the whole of this ratiocinative process reposes is, after all, only a subordinate one; restricted, therefore, in its application, and incompetent to the solution of that fundamental question which, in every class of organs, arises as to the typical construction. And yet a little reflection will show that, as the

Cuvierian doctrine only embraces questions of modifications and special adaptations to peculiar modes of existence, it can only apply to species, or to particular classes of animals, not to whole sub-kingdoms, and still less to the entire animal creation. The insufficiency of Cuvier's hypothesis is well set forth, as regards the cranium, in the following extract:

"The attempt to explain, by the Cuvierian principles, the facts of special homology on the hypothesis of the subserviency of the parts so determined to similar ends in different animals—to say that the same or answerable bones occur in them, because they have to perform similar functions—involves many difficulties, and is opposed by numerous phenomena. We may admit that the multiplied points of ossification in the skull of the human fœtus facilitate, and were designed to facilitate, childbirth; yet something more than such a final purpose lies beneath the fact, that most of those osseous centres represent permanently distinct bones in the cold-blooded vertebrates. The cranium of the bird, which is composed in the adult of a single bone, is ossified from the same number of points as in the human embryo, without the possibility of a similar purpose being subserved thereby in the extrication of the chick from the fractured egg-shell. The composite structure is repeated in the minute and prematurely born embryo of the marsupial quadrupeds. Moreover, in the bird and marsupial, as in the human subject, the different points of ossification have the same relative position and plan of arrangement as in the skull of the young crocodile, in which, as in most other reptiles and in most fishes, the bones so commencing maintain throughout life their primitive distinctness. These and a hundred such facts force upon the contemplative anatomist the inadequacy of the teleological hypothesis to account for the acknowledged concordances expressed in this work by the term 'special homology.' If, therefore, the attempt to explain them as the results of a similarity of the functions to be performed by such homologous parts entirely fails to satisfy the conditions of the problem, and if, nevertheless, we are, with Cuvier, to reject the idea of their being manifestations of some higher type of organic conformity, on which it has pleased the Divine Architect to build up certain of his diversified living works, there then remains only the alternative, that special homologies are matters of chance." (On the Archetype and Homologies of the Vertebrate Skeleton, p. 73.)

The secret, then, of the construction of the skull is not to be discovered by any mere teleological principle: we must turn for the true solution from the French to the more deeply-thinking German school, which, in the theory of the *cranial vertebræ*, all obscure, and even fantastical, as for a time it appeared, developed one of the profoundest truths in organic science. How interesting is the description of the way in which the first germ was discovered! "The gifted and deep-thinking naturalist, Oken, obtained the first clue to this discovery, by the idea of the arrangement of the cranial bones of the skull into segments, like the vertebræ of the trunk. He informs us that, walking one day in the Hartz forest, he stumbled upon the blanched skull of a deer, picked up the partially dislocated bones, and, contemplating them for a while, the truth flashed across his mind, and he exclaimed, 'It is a vertebral column!'"

Subsequent research matured and confirmed this happy inspiration, and Oken published, in 1807, his beautiful generalization, as it is properly termed by Professor Owen, in a now very scarce introductory lecture, entitled, "On the Signification of the Bones of the Skull." The following illustration of his views is so admirable, and so well expresses the leading points of the theory, that no apology is needful for presenting it to our readers:

"Take," he says, "a young sheep's skull; separate from it the bones of the

orbit, also those cranial bones which take no share in the formation of the 'basis cranii,' ex. gr. the frontal, parietal, ethmoid, and temporal; and there will remain an osseous column, which any anatomist, at first glance, would recognise as three bodies of a kind of vertebra, with transverse processes and foramina. Replace the cranial bones, with the exception of the temporals, for, without these, the cavity is still closed, and you have a cranial vertebral column, which differs from the true one only by its more expanded neural canal. As the brain is a more voluminously developed spinal cord, so is the brain-case a more voluminous spinal column. As the cranium includes, then, three vertebral bodies, so must it have as many vertebral arches." (Op. cit., p. 74.)

Oken, avoiding an error into which some of his successors fell, did not confound those bones of the skull which lodge the organs of sense, especially the 'pars petrosa,' and which have been since called by some writers of the German school 'inter-vertebræ," with the true elements of the cranium.

"Reverting to the petrosal," observes Professor Owen, "Oken thus beautifully and clearly enunciates its essential nature and homology: 'You will say, I have forgotten the pars petrosa. No! It seems not to belong to a vertebra, as such; but to be a 'sense-organ,' in which the vertebral or ear-nerve loses itself; and, therefore, is as distinct an organ from a vertebral element as is any other viscus, or as is the eyeball itself. The cause of the delusion (as to the homology of the petrosal) lies in this—viz. that it must be ossified agreeably with its nature, just as the eye must be crystallized.'" (Op. cit., p. 75.)

This was the first definite announcement of the theory of the cranial vertebræ; and although it was subsequently modified variously and even sometimes ridiculously by its advocates, and vehemently contested by its opponents, it advanced year by year, till, as it would seem, it has received its final completion at the hands of Professor Owen. We recommend our readers carefully to study the whole of this "Historical Introduction," as being most instructive, and quite essential to those who desire to master the whole subject.

The distinguished author enters very fully into the subject of nomenclature; and we entirely coincide in his opinion, that the best interests of science will be promoted by the reformation he has so ably undertaken, and so happily completed. The introduction of new terms into a branch of knowledge so overlaid with names as that of anatomy, is, it is true, only justifiable on the ground of making the language of a science harmonize with its leading facts and fundamental principles; but all who are acquainted with the extraordinary progress of late years, will agree that the time is come, when a systematic nomenclature for one of the deepest and most intricate branches of anatomy is urgently required. We can for ourselves aver, that the terms of Professor Owen are admirably calculated for the teaching of human anatomy; they are concise, expressive, and equally adapted for the mere demonstration of a bone, and for the exposition of the deepest truths of embryology and philosophic anatomy. Having this conviction, we would take the liberty of suggesting to all who are occupied as teachers, the great advantage of substituting the nomenclature now presented to their notice for the terms in ordinary use. The principles which have guided the author in its construction, will be gathered from the following extract:

"After maturely considering this subject in its various relations, I have arrived at the conviction that the best interests of anatomical science will be consulted by

basing the nomenclature applicable to the vertebrate sub-kingdom upon the terms and phrases, in which the great anthropotomists of the sixteenth, seventeenth, and eighteenth centuries have communicated to us the fruits of their immortal labours. For it is only on this firm foundation that we may hope to avoid that ceaseless change of terms which follows the device of a systematic nomenclature, significant of a given progress and result of scientific research. But the names of the parts of the vertebrate animals, so based on or deduced from the language of anthropotomy, must divest themselves of their original descriptive signification, and must stand simply and arbitrarily as the signs of such parts, or at least with the sole additional meaning of indicating the relation of the part in the lower animal to its namesake or homologue in man. It is an old maxim, accepted by the best logicians, that no name is so good as that which signifies the total idea or whole subject, without calling prominently to mind any particular quality, which is thereby apt to be deemed, undeservedly, more essential than the rest.

"The chief improvement which the language of anatomy, based upon that of anthropotomy, must receive, in order to do its requisite duty, is the substitution of 'names' for 'phrases' and 'definitions;' and this is less a change of nomenclature than the giving to anatomy what it did not before possess, but which is absolutely requisite to express briefly and clearly, and without periphrasis, propositions respecting the parts of animal bodies. Such names should be derived from a universal or dead language, and, when Anglicised, or translated into other modern equivalents, ought to be capable of being inflected adjectively." (Op. cit., p. 3.)

It is impossible to conceive of any process of mental training better adapted to overcome that *stupor et incompetentia sensuum*, which Bacon condemns, than the study of the typical formation of the skeleton in the animal series, aided by the new nomenclature of Mr. Owen. Such an inquiry demands, as the first condition, the rigid subordination of the senses: form, size, position, the most obvious circumstances that appeal to the eye, here have no signification; the neural spine may, as it is more familiarly known in the dorsal vertebra of man, be a long projecting process, or, as in the mesencephalic arch, it may be expanded into the broad overarching ossa parietalia. And again, the hæmal spine, which in fishes is known as a sharp spiculum, must in the higher vertebrata be sought for, in the chelonia in their hugely expanded sternum, and in birds in their keel-shaped breast-bone. In determining what are "homologous" parts, it is even necessary, in some cases, to reject the evidence of function; for organs may be homologues of each other, and yet have very different offices, as the lung of the mammal and the swimming-bladder of the fish.

Mr. Owen points out a very serious error that is often committed, even by eminent writers, and to which, as correct notions on this point are so essential, it is desirable briefly to allude. The mistake consists in overrating the importance of the embryonic divisions of the several bones of the skeleton. Thus Geoffroy St. Hilaire, and after him Cuvier, asserted that in determining what ought to be considered as a bone, every primitive osseous centre existing in the fœtus should, strictly speaking, be so considered. There is no doubt that in the case of many bones this principle applies; thus, as regards those of the cranium, the fœtal divisions of several do correspond strictly to the separate bones of the lower vertebrata, especially fishes. But in the instance of the long bones, Cuvier's doctrine, if admitted, would lead to the most profound errors; since, as Professor Owen points out, according to that principle the humerus ought to be counted as three, and the femur as four bones,

because there are so many epiphyses in the mammalia, whilst in birds and reptiles the femur is developed from a single centre. A similar difficulty would likewise arise as regards the lower jaw, which is single in mammalia, but multiform in fishes. The source of this error is, that the French anatomists did not distinguish in the osteogenic process between those centres of ossification that have homological or essential relations, and those that have only teleological or incidental relations. (See Brit. and For. Med. Review, 1847, p. 478.)

Other sources of error connected with the same branch of anatomy are thus noticed:

"Dr. Reichert seems to have been unduly influenced by the idea of 'analogy or similarity of development in the determination of homologous parts,' when he rejected the parietal and frontal bones from the system of the endo-skeleton, because they were not developed from a pre-existing cartilaginous basis, or because they could be easily detached from subjacent persistent cartilage in certain fishes; the essential distinction between these and the supra-occipital in regard to development being, that, whereas the cartilaginous stage intervened in the latter between the membranous and the osseous stages; in the other, usually more expanded, cranial spines, the osseous change appears to be immediately superinduced upon the primitive aponeurotic histological condition.

"M. Agassiz seems, in like manner, to give undue importance to similarity of development in the determination of homologies, where he repudiates the general homology of the basi-sphenoid with the vertebral centrum, and consequently its serial homology with the basi-occipital, because the pointed end of the chorda dorsalis has not been traced further forwards along the basis of the cranium in the embryo osseous fish than the basi-occipital. But the development of the centrum of every vertebra begins, not in the gelatinous chord, but in its aponeurotic capsule, and it is in the expanded aponeurosis, directly continued from the 'chorda' along the 'basis cranii,' that the thin stratum of cartilage-cells is formed, from which the ossification of the basi-sphenoid, presphenoid, and vomer proceeds." (Op. cit., p. 6.)

In the present treatise Mr. Owen has rendered a most important service, by stating and answering the objections which have been urged, especially by Cuvier and Agassiz, to the theory of the cranial vertebra. The whole of this argument is worthy the closest attention of all who desire to master this abstruse and difficult question. Our space will only allow us to present a brief exposition of the leading points, which are so ably handled in the work itself. We wish, however, to premise one introductory observation, which, as it seems to us, must have great weight with all unprejudiced persons; it is this:—that every kind of evidence demonstrates the fact, that the posterior segment of the skull, comprising the several component parts of the os occipitis, is truly and indisputably a vertebra. This is even evident on simple inspection, its parts essentially corresponding to those of the bones below; but besides this, in some of the lower vertebrata, as the cyclostomatous fishes, the chorda dorsalis runs continuously on from the spine into the skull, and a similar disposition obtains in the embryonic condition of all the higher classes, so that the break observed at the occipito-atloidal junction does not primitively exist. The identity is indeed so perfect, that most of those who, like the naturalists just named, oppose the general theory, admit the vertebral character of the os occipitis; a fact which is a significant indication that the remaining anterior members of this great chain

of bones, although deviating much more from the ordinary form, are constituted on the same principle.

Passing by the qualified objections, which Cuvier advances against the admission of the occipital vertebra, applying more particularly to the acceptation of the supraoccipital as the neural spine, the second or parietal vertebra may be considered. The great zoologist, in a manuscript which is appended to the posthumous edition of the celebrated 'Leçons d'Anatomie Comparée,' urges an objection, which, on the face of it, seems to have some weight, and which, to our own knowledge, has been felt as a difficulty. In the alisphenoid it is well known that the foramen ovale is usually a complete hole, whilst "vertebræ properly so called," Cuvier urges, "give passage to the nerves only by the intervals that exist between them and the other vertebræ, and not by particular foramina."

The solution of these and similar objections, which will constantly suggest themselves to the mere anthropotomist, must be found in an extended survey and comparison of the vertebral formation in the animal kingdom. Such an examination would soon show, that the neurapophyses present many and most marked deviations from what we are accustomed to see in the human skeleton, and in that of the higher mammals, which are for the most part those collected in anatomical museums, even when attached to important schools. But what are required are skeletons judiciously selected from the more typical forms among fishes, reptiles, and birds, not of course excluding those of mammalia. To answer Cuvier's objection, the student must consider what is the essential office of the neurapophysis; and this is explained, together with some of the more interesting modifications of the process, by Professor Owen in the following passage:—"The most constant functional relation of the neurapophysis is to protect the spinal nerve in its exit from the spinal canal, either by a direct perforation of the neurapophysis (many fishes and some mammals), by a notch in the margin, or (as usually happens) by the interspace between two neurapophyses. This function alone is performed, in reference to the nervous system, at the posterior part of the vertebral column in many animals, where the place of the shortened myelon is occupied by the lengthened roots of the nerves; in the rest of the trunk the neurapophyses protect also the neural axis. The original relation of each neurapophysis to the segments of that axis is determined by the place of connexion of the perforating nerve with the shortened myelon." (p. 97.) It thus becomes evident that the neurapophyses, in relation to the passage of their governing nerves, may be either untouched, notched, or perforated by them, without prejudice to their neurapophysial character. Viewed in the entire series of vertebrata, the cranial neurapophyses are more frequently perforated than notched, those of the trunk more frequently untouched or notched by the nerves in passing through their interspaces. That entire foramina may exist in a single vertebra of the spine is evidenced, among other examples, by the dorsal vertebra of the ox, and the abdominal vertebra of the lophius, whose neural arches are directly perforated by the spinal nerves. (Loc. cit., p. 145.)

Cuvier directs his next argument against the vertebral character of the neural arch of the parietal segment, because its composition differs from

that of other vertebræ, by being composed of five pieces, or even, including the interparietal, of six. It is strange that this should have been used as an objection, since Cuvier does not himself hesitate to reckon the atlas of the crocodile as the first vertebra, notwithstanding it is composed of six pieces, which remain distinct during life. But it is certain that questions of this deep character do not depend for their solution on any mere subordinate circumstances, such as those relating to number; and, consequently, the position assumed by Geoffroy, that "Nature reproduces the same number of elements in the same relations in each vertebra, only she varies indefinitely their form," cannot be received. We rather seek the truth in such philosophical propositions as are set forth by Professor Owen in the following passage:—

"It has been abundantly proved, I trust, that the idea of a natural segment (vertebra) of the endo-skeleton does not necessarily involve the presence of a particular number of pieces, or even a determinate and unchangeable arrangement of them. The great object of my present labour has been to deduce, by careful and sufficient observation of Nature, the relative value and constancy of the different vertebral elements, and to trace the kind and the extent of their variations within the limits of a plain and obvious maintenance of a typical character."

Detailed reasons are then given in support of the existence of this mesencephalic vertebra, and likewise of two anterior vertebræ or arches, the prosencephalic and rhinencephalic. In alluding to the opinion of Cuvier, that the human and mammalian parietals are "particular pieces which have a particular destination," Professor Owen thus eloquently and philosophically expresses himself:—

"What to me has ever appeared one of the most beautiful and marvellous instances of the harmony and simplicity of means, by which the One great Cause of all organization has effected every requisite arrangement, under every variety of development, is the fact, that the protection of the enormous cerebrum peculiar to the higher mammals, has not been provided for by new bones—by bones, ex. gr., developed from centres, so numerous or so situated, as to render any determination of their homologies as vague and unsatisfactory as would result from the attempt to determine those of the dermal ossifications upon the head of the sturgeon, in reference to the endo-skeleton epicranial bones in fishes and reptiles. We might well have expected, had conformity to type not been a recognisable principle in the scheme of organized beings, to have had so many 'particular bony pieces,' and so situated in the expanded human cranium, as would have baffled all our endeavours to reduce them to the type of the epicranial bones of the reptile or fish. Yet the researches of the great comparative anatomists of the present century, and more especially those of Cuvier himself, have proved that there is no such difficulty; and a glance at the Table of Special Homologies, No. I, will show that the bones most modified in relation to the expanded cerebrum and cerebellum of man and mammals, are precisely those of which the determination has been easiest, and respecting the names and nature of which there has been the least discrepancy of opinion. It is with pain and reluctance, which only the cause of truth has overcome, that I am compelled to notice the inconsistencies into which the great Cuvier fell, when his judgment became warped by prejudices against a theory, extravagantly, and perhaps irritatingly, contended for by a contemporary and rival anatomist. After having established, by the clearest evidence and soundest reasoning, in his great and immortal works, that the bones in the fish and reptiles were homologous with those in birds, mammals, and even in man; and, after contending that they ought to bear the same name—under which, indeed, we find him describing them in the 'Leçons d'Anatomie Comparée,' from man down to the fish—Cuvier comes at last

to declare, that, in those animals in which they are separated from the alisphenoid and mesencephalon, they are 'particular pieces which have a particular destination!'" (Loc. cit., p. 148.)

We regret that our limits compel us to omit the equally forcible and satisfactory reasoning, by which the distinguished author controverts the arguments of M. Agassiz. We proceed, therefore, to point out, what is of peculiar interest to the members of our own profession, the segmental constitution of the bones of the trunk of the human body, according to the archetype vertebral formation. In the cervical vertebra, there are a centrum, neurapophyses, neural spine, rudimental pleurapophyses; the atlas is perhaps the only cervical segment, in which the hæmapophyses are ossified; the zygapophyses or articular processes, of course, are present.

"In the seven vertebræ which succeed the cervicals, the pleurapophyses are progressively elongated; they are shifted from their proper centrum to the interspace between it and the next segment above, or in advance, and retain their moveable joints. The hæmapophyses are cartilaginous, and articulate with the ends of the pleurapophyses and with the hæmal spines, which are flattened, slightly expanded, and ultimately blended into one bone, called 'sternum.' The hæmal spine of the first typical segment remains longest distinct: it receives, also, the extremities of the displaced hæmapophyses, and has been called 'manubrium sterni.' The hæmal spine of the seventh segment commonly continues longer distinct, and is later in becoming ossified, whence it is called 'ensiform cartilage:' it probably includes the rudiments of some succeeding hæmal spines. In the four succeeding segments the pleurapophyses become progressively shorter, and the hæmapophyses, still cartilaginous, are severally attached by their lower attenuated ends to the pair in advance, leaving the hæmal arch incomplete below. In the next vertebra (nineteenth from the skull) the still shorter pleurapophyses resume the exclusive articulation with their proper centrum; and the correspondingly short and pointed hæmapophyses terminate freely. Those pleurapophyses and hæmapophyses which directly articulate with hæmal spines (sternum) are called collectively 'true ribs' (costæ veræ), the proximal element being 'the bony part of the rib' (pars ossea costæ), the distal one 'the cartilage of the rib.' The rest of the hæmal arches which are incomplete, through the absence of the hæmal spine, are called 'false ribs' (costæ spuriæ); and the last, which terminates freely in the origin of the diaphragm, is a 'floating rib.' The centrum, neurapophyses, and neural spine of each segment, with freely articulated pleurapophyses, coalesce into one bone, called 'dorsal vertebra' in anthropotomy: these vertebræ are twelve in number. Each of the five succeeding segments is represented by the same elements (centrum and neural arch,) coalesced, that constitute the so-called dorsal vertebræ: they are called 'lumbar vertebræ:' they have no ossified pleurapophyses; and the hæmapophyses of these segments are represented only by the aponeurotic 'inscriptiones tendineæ musculi recti.'" (Loc. cit., p. 158.)

Before quitting the vertebræ of the neck, it will be interesting to the human anatomist to know the true nature of that curious process, the odontoid; it is nothing else than the central and main part of the body of the atlas, as Cuvier announced in his "Ossemens fossiles." The anatomy of the lower animals, as so constantly happens, affords a striking confirmation of the signification of this apparent projection of the axis, since we learn from the late lectures of Professor Owen, that in the wombat (*Phascolomys wombat*) and other marsupials, there is a distinct mark of division in the P. odontoides, indicative of the typical separation of it from the second vertebra.

The homology of the pelvic bones is most instructive, in consequence

of the degradation of some of the vertebral elements, especially the neural arches and spines, and the enormous development of other parts; but it is, at the same time, somewhat difficult of interpretation. To begin with the bones which least diverge from the typical structure, the sacrum and coccyx, they have long been known as comprising the so-called "false vertebræ." Of these, the first four of the sacrum combine the same elements as the neck; viz. centrum, neurapophyses, neural spine, and short, but thick, pleurapophyses; in the fifth sacral vertebra, these last-named processes are absent as osseous rudiments, and the neural spine is commonly undeveloped. The first coccygeal vertebra in man consists of a centrum and of stunted neurapophyses, widely separated above, but having zygapophyses (articular processes), which join those of the last sacral vertebra and diapophyses (transverse processes). The neurapophyses are represented by exogenous tubercles of bone in the second coccygeal vertebra; and the third and fourth vertebræ are reduced to the centrums only. There are some points of interest connected with the history of the coccyx: embryology teaches us, for example, that even in the human being this bone is more developed than is usually suspected. Thus the cartilaginous deposits in the primitive blastema of this extremity of the trunk, indicate a greater number of caudal vertebræ than those which are actually ossified afterwards, so that the rudimental tail is proportionally longer in the embryo than in the adult. Corresponding with this state of the part, it is well known to embryologists, that these primordial coccygeal vertebræ are at an early age in strict relation with the neural axis, the spinal cord extending much lower than at a subsequent period; when it is, as it were, withdrawn by a movement of longitudinal concentration, a process of aggregation which is seen in the development of the spinal centre of many avertebrate animals. These facts afford a striking proof of the rigid unity which pervades the organic process of development, the conformation just noticed evidently approximating the human embryo to the permanent structure of the mammalia.

As regards the more complex and involved elements of the pelvis, constituting in the human adult the os innominatum, Professor Owen, after a careful scrutiny of this formation in birds and reptiles, is of opinion that the first sacral vertebra has its pleurapophyses divided into two parts, the upper of which has already been enumerated as a portion of this vertebra; the lower part, consisting of the so-called "ilium," of course, immensely expanded. The hæmapophysis of this first vertebra is formed by the pubis, the hæmal spine being either absent, or in a rudimentary state. The second sacral vertebra has its hæmapophysis constituted by the "ischium," which is, however, separated from its proper pleurapophysis by the expanded (iliac) portion of that of the preceding vertebra, with which it coalesces, as well as with the preceding hæmapophysis (pubis).

It has been our object to point out in the preceding observations and extracts, the intimate relation of these splendid generalizations, embracing the entire vertebrate sub-kingdom, to human anatomy; and we cannot better conclude this part of the present article, than by quoting the following remarks of Professor Owen, illustrative of the same subject.

"Inasmuch, however, as the different segments of the human skeleton deviate in

various degrees from the common archetype, and as the different elements of such segments differ in their modifiability, anthropotomy has at no period wanted also its 'general terms,' expressive of the recognised extent of such conformity; such terms also indicating, obscurely, indeed, so much perception of the pre-existing model as could be obtained from the study of one form, at a period when that form—the human frame—was viewed as something not only above, but distinct from, if not antithetical to, the structures of the brute creation, and when it was little suspected that all the parts and organs of man had been sketched out, in anticipation, so to speak, in the forms of the inferior animals. Thus, the word 'vertebra' shows, by the number of the segments or parts of segments to which it is applied in anthropotomy, the recognition of the obvious extent to which the archetype is retained in such primary constituents of the human endo-skeleton. And, inasmuch as in some regions (the cervical, e. g.) the 'vertebra' includes all the elements of the typical segment, there developed, it has been retained in homological anatomy, but with a more definite meaning, as the technical term of the primary constituent segment of the endo-skeleton in all vertebrate animals.

"Homological anatomy, therefore, teaches, that the term 'vertebra' should not only be applied to the segments of the human skeleton, in the technical and definite sense illustrated by the figures accompanying this report, but be extended to those modified and reciprocally immoveable segments, which terminate the endo-skeleton superiorly, and are called, collectively, skull.

"The term 'head,' then, indicates a region of specially modified vertebræ, like the terms 'neck,' 'chest,' 'loins,' &c.; and amongst the species of the primary segments characterised by specific modifications, the 'cranial' vertebræ must be added to the 'cervical,' 'thoracic or dorsal,' 'lumbar,' 'sacral,' and 'coccygeal or caudal.'

"Such, with reference to the 'general' term 'vertebra,' seems to be the advance of which anthropotomical science is susceptible, in order to keep progress and be in harmony with anatomy." (p. 163.)

This admirable report concludes by some speculations concerning the nature of the force "by which all the modifications of the vertebrate skeleton, even those subservient to the majesty of man himself, are still subordinated to a common type." The author is of opinion, first of all, that the typical or fundamental character of the endo-skeleton—its division, that is to say, into a succession of segments similarly composed, depends on the principle of mere vegetative or irrelative repetition; a principle which is analogous to the repetition of similar crystals as the result of polarizing force in the growth of an inorganic body.

"Not only does the principle of vegetative repetition prevail more and more as we descend in the scale of life, but the forms of the repeated parts of the skeleton approach more and more to geometrical figures, as we see, for example, in the external skeletons of the echini and star-fishes; nay, the calcifying salt actually assumes in such low-organized skeletons the very crystalline figures which characterise it when deposited, and subject to the general polarizing force, out of the organized body."

As concerns the adaptations or modifications of this common type, according to the requirements of each individual class of animals, these have been explained by two hypotheses: according to the first opinion, they are the result of "vital properties," either peculiar to living matter or common to all, but latent in dead matter; whilst by the second hypothesis they are attributed to the operation of one or more "vital principles," vital forces, dynamics or faculties, answering to the ἰδέαι of Plato, and deemed by that philosopher to be superadded to matter and

mind, and which he defined as a sort of models or moulds, in which matter is cast, and which regularly produce the same number and diversity of species.

Professor Owen conceives, therefore, that whilst this vital force produces the diversity of form belonging to living bodies of the same materials, which diversity cannot be explained by any known properties of matter, the polarizing force pervading all space, appears to be in counter-operation during the building up of such bodies; and to the operation of this force, the similarity of forms, the repetition of parts, which are the signs of the unity of organization, may be mainly ascribed.

This hypothesis, for however probable it may be, it can, at present, be hardly called anything more, is thus applied to the human formation, in which the morphological principle is modified in the highest degree by the teleological:

"The extent to which the operation of the polarizing or vegetative-repetition-force is so subdued in the organization of a specific animal form becomes the index of the grade of such species, and is directly as its ascent in the scale of being. The lineaments of the common archetype are obscured in the same degree: but even in man, where the specific organizing force has exerted its highest power in controlling the tendency to type, and in modifying each part in adaptive subserviency to, or combination of power with, another part, the extent to which the vegetative repetition of segments and the archetypal features are traceable, indicates the degree in which the general polarizing force may have operated in the arrangements of the parts of the developing frame; and it is not without interest or devoid of significance that such evidence should be mainly manifested in the system of organs, in whose tissue the inorganic earthy salts most predominate." (p. 172.)

It is evident that this is a subject which involves the general question, how far the phenomena of living beings are dependent on the forces of common matter; and we need scarcely remind our readers, that every advance in exact knowledge has distinctly shown how large a share those forces have in the actions called vital: the repulsive and attractive forces displayed by the red corpuscles of the blood between themselves under varying and well-known conditions; the endosmotic powers of organic membrane; the diffusion of gases taking place in the ultimate texture of the lungs; the insulated conduction displayed by the primary nervous tubules, are so many striking instances of the application of chemical and physical laws in the processes of life. The time seems to be approaching when this profoundly interesting question will receive its solution; but, in the mean time, it may not be superfluous to point out, that, on mere physiological grounds, irrespective of any higher considerations, it is evident that, however far these and similar investigations may ultimately extend, they can only concern the organic and animal functions; the purely psychical forces directing and controlling these lower phenomena being clearly of a different and distinct nature.

We cannot conclude this notice without calling special attention to the important figures which Professor Owen has introduced into the treatise, whose title is given at the head of the present article, illustrative of "the ideal pattern or archetype of the vertebrate endo-skeleton," and of the modification of its characteristics in the four great divisions of the vertebrate sub-kingdom, viz., fishes, reptiles, birds, mammals, and also in man. In these plates all the typical characters, and their modifications in

the several regions of the skeleton, together with the appendages (the fore and hind limbs), are made apparent to the eye by peculiar markings. Nothing can be better adapted to facilitate the full apprehension of the whole of this profound inquiry than these admirably contrived drawings: to the student they are indispensable; and even to those whose minds have been already engaged in the subject, they will prove most acceptable. We trust it will be permitted us to observe, that no one who professes to teach anatomy in our schools, whether metropolitan or provincial, can be considered to have fulfilled the requirements of an office so honorable and responsible, till he has presented to his alumni these philosophic researches, demonstrated by properly tinted diagrams of the archetype vertebra.

In the work of Mr. Maclise the same object is proposed as that which has been aimed at, and so admirably realised, in the philosophic researches of Professor Owen; and although it would be inconsistent with the discharge of our duty, to say that we approve of the mode of procedure adopted by the author, we are most willing to bear our testimony to the talent and acquirements he has displayed in the treatise before us. Mr. Maclise has, in our judgment, too exclusively centred his attention on the higher forms of vertebrate animals, in which, as occurs without an exception in every class of organs, the fundamental or typical parts are so much concealed beneath the innumerable adaptations demanded by peculiar modes of existence, that they are recognised with difficulty, are therefore constantly liable to be mistaken, or, as so often occurs, to be entirely overlooked. The illustrations of Mr. Maclise are almost exclusively drawn from mammals, and those too of the highest orders, man and the quadrumana. Now, without any wish to depreciate these investigations, the best interests of science demand the expression of our opinion, that the "archetype vertebra" would never have been found in this extremity of the vertebrate sub-kingdom; the discovery could only be made by the careful scrutiny of the lower formations, or those of fishes, reptiles, and birds, joined to the examination of the transitory phases presented by the embryo of the higher classes. And this leads us to observe, that the theory advanced in the volume before us, is evidently based upon the profound inquiries of Professor Owen, though, for some reason or other, the admirable writings of that great zoologist are scarcely even named by the author.

Another great mistake runs through the entire work. Mr. Maclise seems to imagine that the anatomists of the present day, like their predecessors, regard the bone called in anthropotomy *vertebra*, as a perfect or typical whole. Thus he avers, that "the name vertebra has been fixed upon a part of the archetype structure, and comparative science has blindly founded its generalizations upon such a name." In the introduction (p. 11) the author again enlarges upon what he assumes to be the error of comparative anatomists, in attempting to trace the modifications of the typical vertebra, or, as he expresses it, "unity in variety," without having previously "ascertained that form of unity from which variety is struck out;" subsequently asking, "what anatomist is there who may be said to have characterised the form of unity?" This he answers by affirming there is no one of them who has approached this goal of comparative research, and that we are still as blind to the actual

figure of unity, as the Peripatetics were in ancient Athens, under their founder Aristotle. Now all these assertions will appear rather ridiculous to those who are acquainted with that splendid series of generalizations, which, commencing with Göthe and Oken, and more or less happily developed by Spix, Geoffroy St. Hilaire, Carus, and a multitude of others, has at length received from the hands of Professor Owen apparently its final consummation. We need only refer, in support of this, to the important figure of the "ideal typical vertebra," and to the various illustrative figures taken from different animals, for which science is indebted to the distinguished Hunterian professor.

Mr. Maclise perceiving, like his predecessors, that the bone called in anthropotomy "a vertebra" is not an integer or perfect whole, but only a part of some other complete form, or archetype figure, wishes to discover in what this consists, and where it is to be found. Amidst considerable repetition, and a needlessly cramped language, we select the following passage, as perhaps best expressing his meaning:—

"The archetype being the complete form, and a vertebra being known to stand as part of this structure, it follows that, if we found our generalizations upon the figure of that which is a part, we choose a much less capable instrument for such generalizing method, than if we had started at first in the use of the complete form, which must include all forms lesser than itself. In a colonnade, composed of identical or homologous columns, we choose one column as a type of all others standing in series with itself; such column is at once recognised to be a complete form, it is an archetype, and we understand the capital to be a part of this archetype. When we would generalize upon the series of those archetype forms which compose the colonnade, we do not make choice of any part of the archetype forms, such, for example, as the surmounting capital, and then affix to the entire series of archetype columns the name which is proper to the part capital, and thus undertake to demonstrate unity by the use of such a name; but, on the contrary, we at once acknowledge the complete form of an archetype column, taken as a whole, and own it to be that form to which not only the several members of any column of the series may be referred, but to which even the complete figure of any column of such series may also be referred, and find its full and proper homologue. This is what we are to understand by the word archetype, as hereafter made use of." (Introduction, p. 10.)

The archetype vertebra must thus be in itself complete or integral, capable of any degree of subtraction, but necessarily from the idea incapable of addition. The complete vertebra or integer is to be found in the upper segments of the thorax:—"the costo-vertebral thoracic figure is the archetype of series." (Plate 16.) This figure consists of the dorsal vertebra with its various processes, of the two appended ribs, and of the sternum completing the circle. The author has given a most extended series of illustrative figures in outline, amounting to some hundreds, with descriptive details, to demonstrate this archetype, and the various modifications to which it is subject in the several parts of the spine.

In both directions, upwards and downwards, it experiences a metamorphosis or a degradation arising from the subtraction of some of its constituent parts; thus, in the cervical vertebra, the costa of the archetype is reduced to the short piece forming the anterior root of the transverse process, which is sometimes, however, prolonged as a cervical rib; and so again, a portion of the transverse process of the lumbar

vertebra is the "homologue" of the same element. "This metamorphosis or degradation of a whole quantity," observes Mr. Maclise, "would appear to be the law by which Nature creates a serial skeleton axis;" and as "this law of metamorphosis" may subtract an infinitesimal part, the variety of forms is infinite. The departure in the serial spinal axis is most remarkable, of course, in the caudal region, where we find at length but the mere terminal ossicle of the coccyx. In the ophidian reptile, on the contrary, there is a "thoracic skeleton series," consisting from first to last of costo-vertebral quantities.

We must refer our readers to Mr. Maclise's work for further illustrations of his views, as no particular advantage would be gained by transferring them to our pages.

ART. VII.

Principles of Medicine: comprising General Pathology and Therapeutics, and a brief general View of Etiology, Nosology, Semeiology, Diagnosis, Prognosis, and Hygienics. By CHARLES J. B. WILLIAMS, M.D., F.R.S., Professor of the Principles and Practice of Medicine, and of Clinical Medicine, and First Physician to the Hospital, University College, London, &c. &c. Second Edition, considerably enlarged.—*London*, 1848. 8vo, pp. 533.

Elements of General Pathology. By A. F. CHOMEL, Professor of Clinical Medicine to the Faculty of Paris, &c. &c. Third Edition, considerably enlarged. Translated from the French by F. E. OLIVER, M.D., and W. W. MORLAND, M.D., Members of the Massachusetts Medical Society. —*Boston* (New England), 1848. 8vo, pp. 458.

WE always feel a peculiar pleasure in welcoming a new edition of a really good medical book. We have a sympathy with every deserving author, who earns for himself the meed of professional approbation, to which his merits entitle him. And we rejoice at every indication of progress in the collective mind of the profession, such as is afforded by its appreciation of that which is really excellent,—especially when the excellence lies rather beneath than upon the surface. There can be no question that the verdict of the profession upon the abstract merits of a production, so far as can be estimated from its sale, is not always a correct one. It is rare, we believe, for a really trashy book to find much acceptance with the *medical* public; the *general* public, we believe, being the chief readers of such as have an extensive and constant sale, altogether disproportioned to their deserts. But, on the other hand, works of really high character are frequently very far from obtaining the attention they deserve. We could point to several treatises, containing the details of important discoveries having an immediate bearing on practice, which have fallen almost still-born upon the press. Their failure has been due, in some instances, to the circumstance of their being too much in advance of the professional mind of the time; whilst in other cases it has seemed rather to result from some qualities of the books themselves, which rendered them unpalatable to readers, who desire that the information for which they seek should be communicated in the clearest

and most concise form, and who dislike the introduction of personalities into scientific treatises. We believe that the failure of any really good medical work, that is not too expensive for the professional pocket, may be accounted for in one or other of these modes; and we advise every unsuccessful author to consider which is the one that is most likely to be operating to his disadvantage.

Now had Dr. Williams's 'Principles of Medicine' been in this predicament, we should have been at no loss to have assigned the reason; for whilst we entertained a very high opinion of its merits, both as regards matter and manner, we had our misgivings whether it was not *too* good,—that is, too abstract and scientific in its character,—to find favour with a body so eminently practical in its tastes, as is the great mass of the medical profession. The call for a new edition within four years, however, has pleasantly dispelled our apprehensions; and has made us feel that the time is advancing when medicine shall be elevated from the rank of an uncertain art, to a place as near the exact sciences as the peculiarities of its character and objects will permit. It is with a justifiable pride that Dr. Williams is able, in the Preface to this Second Edition, thus to express himself in regard to his efforts at combining science with art, and at placing the practice of medicine on a more rational basis. "In endeavouring to adapt the work to the rapid improvement in medical science, it is most satisfactory to be able to state that in very few instances has it been necessary to retract or supersede the inferences and views set forth in the First Edition; on many subjects they have been confirmed and extended by recent researches, to a degree that has not less surprised me, than convinced me of their truth." And we can fully sympathise with him in the satisfaction which he derives from finding that his scientific views on many subjects "point, as it were spontaneously, to remedial measures closely corresponding with those which the best experience has sanctioned; and that they simplify and facilitate the indications of treatment in a manner that suggests more efficient modes of practice than could be obtained by blind experience."

We are not, however, by any means disposed to abandon our critical function in behalf even of so meritorious a production. We are not, by any means, in accordance with our author upon every one of the many questions which his work embraces; nor do we find the arrangement of the subjects, and the proportionate space assigned to them, in entire harmony with our own notions of what such a work should be. We may express our opinion freely upon all these points, we trust, without being charged with arrogance or dogmatism; the more especially, when we state that they are subjects which have long occupied our own sedulous attention. And that we shall devote so large an amount of our space to placing the distinctive merits of his treatise before our readers, and to the examination of those portions of it which seem to us of most questionable merit, will, we trust, be accepted by its author as a tribute of respectful consideration, which we feel a satisfaction in paying to his labours in this field; a *Second* Edition not being usually regarded as having a claim to more than a notice of the *novelties* it may contain.

"The PRINCIPLES, ELEMENTS, or INSTITUTES OF MEDICINE," observes Dr. Williams, at the commencement of his treatise, "comprise those leading and general facts and doctrines regarding disease and its treatment,

which are applicable, not to individual cases only, but to groups or classes of diseases."

"These principles," he subsequently continues, "may be deduced in part from a knowledge of animal structure and function (anatomy and physiology), conjoined with an acquaintance with the agents which cause and remove disease; but chiefly they are derived from a generalization of facts observed in an extensive study of disease itself, and its effects in the living and in the dead body. But so far as they have been ascertained, they become more intelligible to the student if explained synthetically, by describing first the causes of disease, then their operation on the body, and lastly, the resulting changes in function or structure which constitute disease in its more elementary forms." (p. 1.)

We fully approve of the plan pursued by Dr. Williams in thus making Etiology the first subject of discussion; and we are glad to find such considerable additions to the first chapter, in which the causes of disease are set forth, as render it an excellent epitome of the present state of our knowledge on the subject, enlarged as this has been of late years by the data collected by the promoters of sanitary reform. We must pause *in limine*, however, to comment on the manner in which the subject of Causation is treated; since we here find such a deficiency in that logical precision at which Dr. Williams has aimed, and generally with success, as is very likely to mislead the student on one of the most fundamental points of the whole inquiry.

"A great variety of agents and circumstances may thus act on the body so as to produce disease; but in most instances there is not that uniform and constant relation between these as causes, and the diseases as effects, which we might expect from the analogy of causation in the simpler sciences. In chemistry or in mechanics, effects certainly and uniformly follow causes; in physiology or pathology, no doubt, effects also ensue; but whether these effects shall be manifest as disease or not, will depend on many circumstances, of which we often cannot take cognisance. It is true that when the causes resemble, and act like those of physics or chemistry, their proper effects will not fail to ensue. Thus, a cutting instrument, a red-hot iron, or a corrosive liquid, will not fail to produce disease; because its operation is so energetic, as to overcome all vital properties by physical and chemical force, and disorder must follow. Further, certain poisons and other potent agents, which act on, without destroying, the vital properties of living parts, may also, if of sufficient strength, pretty constantly produce morbid effects. Thus opium taken internally causes somnolency; tartar emetic excites nausea and vomiting; cantharides applied to the surface induces inflammation, &c.

"But the common causes of disease are seldom of this decided and positive character; they are often present without disease ensuing; and they are known to be causes only because disease is observed to ensue in a greater number of cases when they are present than when they are absent. Thus, improper food is a cause of indigestion, and exposure to cold is a cause of catarrh; yet many persons eat unwholesome food, without suffering from indigestion, and many are exposed to cold, without 'taking cold.' But those who *do* suffer from indigestion, observe that they do so more after taking improper food; and those who *are* affected with catarrh can often trace it to exposure to cold. The reason of this uncertainty of action is chiefly in various powers by which the body resists the morbific influence; which powers vary much under different circumstances. The failure or irregular operation of this power constitutes one predisposition to disease." (pp. 5, 6.)

Now the tendency of these statements appears to us to be, to put into the mind of the student the idea, that the relation of cause and effect in what concerns the living organized body is less certain and definite than that which prevails in the inorganic world; an idea alike false and

pernicious. It is false, because the very term *cause* involves the idea of invariable sequence. And it is pernicious, because the physiologist or medical practitioner having once adopted it, is likely to be continually recurring to it for an easy solution of all the difficulties he meets with; and to satisfy himself with the notion that he gives a sufficient account of a variation in the results, when he sets it down to the uncertainty in the action of the causes, and holds back from the inquiry into the *sources* of the variation, which will seldom altogether elude a patient, sagacious, and diligent investigation. We are far from imputing such a tendency to Dr. Williams himself; for the whole of his book is an evidence to the contrary. But a teacher cannot be too careful to give to his pupils at the outset clear and logical conceptions of the fundamental elements of the inquiry through which he desires to guide them; and we shall, therefore, not apologise to our readers for detaining them a little upon this topic. The following quotation from the writings of one of the greatest authorities of the day, places the matter in so clear a light, that we shall have nothing else to do than to illustrate and apply the doctrines there laid down. After stating that "the law of causation, the recognition of which is the main pillar of the inductive philosophy, is but the familiar truth that invariability of succession is found by observation to obtain between every fact in nature, and some other fact that has preceded it;" so that "for every event there exists some combination of objects or events, some given concurrence of circumstances, positive and negative, the occurrence of which will always be followed by that phenomenon;" Mr. Mill thus continues:—

"It is seldom, if ever, between a consequent and one single antecedent, that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of them all being requisite to produce, that is, to be certain of being followed by the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of a cause, calling the others merely conditions. Thus if a man eats of a particular dish, and dies in consequence, that is, would not have died if he had not eaten of it, people would be apt to say, that eating of that dish was the cause of his death. There needs not, however, be any invariable connexion between eating of the dish and death; but there certainly is, among the circumstances which took place, some combination or other upon which death is invariably consequent; as, for instance, the act of eating of the dish, combined with a particular bodily constitution, a particular state of present health, and, perhaps, even a certain state of the atmosphere; the whole of which circumstances perhaps constituted in this particular case the *conditions* of the phenomenon, or in other words, the set of antecedents which determined it, and but for which it would not have happened. The real cause is the whole of these antecedents; and we have, philosophically speaking, no right to give the name of cause to one of them, exclusively of the others. What, in the case we have supposed, disguises the incorrectness of the expression, is this, that the various conditions, except the single one of eating the food, were not *events* (that is, instantaneous changes, or successions of instantaneous changes), but *states*, possessing more or less of permanency; and might, therefore, have preceded the effect by an indefinite length of duration, for want of the event which was requisite to complete the required concurrence of conditions; while as soon as that event—eating the food—occurs, no other cause is waited for, but the effect begins immediately to take place: and hence the appearance is presented of a closer and more immediate connexion between the effect and that one antecedent, than between the effect and the remaining conditions. But although we may think proper to give the name of cause to that one condition, the fulfilment of which completes

the tale, and brings about the effect without further delay, this condition has really no closer relation to the effect than any of the other conditions has. The production of the consequent required that they should all *exist* immediately previous, though not that they should all *begin* to exist immediately previous. The statement of the cause is incomplete, unless in some shape or other we introduce all the conditions." (Elements of Logic, vol. i, p. 400.)

We can scarcely have a better illustration of this principle than is afforded by the analysis we have elsewhere given (p. 81 et seq.) of the circumstances under which the fearful outbreak of cholera at Kurrachee took place. When a destructive epidemic occurs, we are a great deal too apt to fix upon the morbific agent supposed to be conveyed by the atmosphere, or to be communicated from individual to individual, as *the cause* of the fatal results. It is allowed that the influence of this cause may be modified by certain conditions affecting individuals or classes; which render some of them more and others less subject to its operation. But from the prominence of the *event*, we are too prone to overlook the fact that the previous *state* is as necessary for the concurrence of the conditions requisite to produce the result in any one case, as is the atmospheric or contagious poison. The fact that, of a large number of persons equally exposed to the same hypothetical agent, only a certain proportion will be affected by it—some fatally, others comparatively mildly, whilst others escape its influence altogether,—does not by any means indicate an uncertainty in the action of the supposed cause; but merely shows that it is only one out of several causes, all of which must be in concurrent action to produce an uniform result. Thus, in the case in question, we find that the access of the choleraic poison was followed by a mortality of only 6·91 per cent. in the division of the army least affected by it; that the result, in another division of the army, was a mortality of 10·86 per cent.; whilst in a third the mortality was as high as 21·8 per cent. We cannot but be here struck by the fact, that the difference between the highest and lowest rate of mortality should be positively greater than the latter itself; showing that some other conditions must be in operation, at least equally tending to a fatal result, with the access of the cholera poison itself. And from our analysis of the circumstances of each case, we have found reason to believe that the comparative exemption of the first division was due to the favorable character of its residence; whilst in the second and third cases imperfect ventilation of the tents was a concurrent condition; the injurious influence of this being aided in the latter, by the depressed condition of the system induced by previous fatigue. Now it would be just as philosophical to say, that in the first case 6·91 per cent. died of cholera alone,—in the second, 6·91 per cent. of cholera, and 3·95 per cent. of insufficient ventilation,—and in the third, 6·91 per cent. of cholera, 3·95 per cent. of insufficient ventilation, and 10·94 per cent. of previous fatigue,—as it would be to say that, in any one case, death took place from the agency of the choleraic poison *only*. In all cases, a certain concurrence of conditions is requisite; and *all* these conditions have an equal right to the appellation of *causes*. The deficient ventilation of the tents, and the fatigue of a long previous march and excessive drill, were just as direct causes of the severity of the cholera epidemic at Kurrachee, as was the presence of the cholera-poison itself. Neither could have produced the actual result, without the concurrence of the

rest. The cholera-poison could have no more killed 10·86 per cent. of the second division, without the concurrence of bad ventilation, than bad ventilation could have killed them without the cholera-poison; the fatigue induced by marching and drilling could have no more killed 21·8 per cent. of the third division, without the cholera-poison and insufficient ventilation, than these, separately or in conjunction, could have produced this mortality in the same period without the cholera-poison.

But whilst the (presumed) access of the cholera-poison was an *event*, which completed the required concurrence of conditions,—the other conditions being *states* of the bodily system induced by bad ventilation, fatigue, &c.—it is easy to show that it does not thence become entitled to rank as more of a cause than the rest; since it cannot be doubted that the same result would happen, if the order of action were inverted. Thus we will suppose the cholera-poison to become endemic (as it has sometimes appeared to be) in a particular district; so far as regards those habitually exposed to it, therefore, its influence will be a *state*, not an event. Then let a regiment of soldiers, living in a choleraic district, be confined for a time in close tents, or be forced to perform a fatiguing march, or be subjected to both these conditions, concurrently; there can be no question that the mortality will undergo an enormous increase, of which we might seem justified in asserting that these *events* are the cause.

Hence we arrive at the conclusion that it is utterly unphilosophical to single out any one of the conditions whose concurrence is necessary to produce the result, as *the* cause *par éminence*, and to look upon the rest as accidental or subordinate concomitants; whilst it is equally unphilosophical to entertain any doubt as to the certainty of the result, whenever the concurrence of causes or conditions is identically the same. A difference in the results as certainly implies a difference in the causes, in physiology and pathology, as in physics and chemistry. When we bring individual cases into comparison, it is often very difficult to fix upon the points of difference to which the diversity of results may be due; for, on the one hand, they may be so numerous as to leave us at a loss to discriminate between the important and the trivial; whilst, on the other, there may be such an absence of any ostensible variation, as to leave us only the convenient but most unsatisfactory resource of idiosyncrasy or individual peculiarity of constitution. It is in such investigations that statistical data afforded by large numbers of individuals are so important; since they both serve to fix our attention prominently upon differences of results occurring under apparently the same circumstances; and, when their basis is sufficiently extended, they substitute the average of the mass for the isolated phenomena of idiosyncrasy, and force us to the conviction that any decided difference in result (as, for instance, in the mortality of different districts, or of different collections of individuals, during a given period, or during the prevalence of a particular epidemic) must be due to a *discoverable* difference of causation,—such individual peculiarities as are really undiscoverable forming so small a proportion of the whole, that in an investigation of this kind they may be practically disregarded.

It might be thought that our ordinary etiological language is sufficiently correct, to render it unnecessary that we should dwell upon this topic; since it includes the previous *states* and *events* alike under the designation of *causes*, predisposing and exciting. But although this may

be the case in formal scientific treatises, we do not think that the truth is recognised in ordinary professional habits of thought. Thus in the manifold discussions which have taken place on the contagion of fever, cholera, &c., it has been maintained by one party that contagion is *the* cause of the propagation of these diseases, whilst another, with similar exclusiveness, finds in neglect of hygienic principles alone the source of their development and extension. Yet the former cannot but admit that certain predispositions, established by previous inattention to the laws of health, most powerfully concur with the contagious miasm in the production of an epidemic; whilst the latter can scarcely blind their eyes to the proofs, that in the epidemic prevalence of many forms of fever (to say the least) contagion plays a very important part. Each party, when the case is logically stated, must thus admit the action of at least two concurrent causes; and the difference between them lies chiefly in the relative *degree*, in which these are supposed to operate in the production of a given result.

Returning now to Dr. Williams's 'Principles,' we may dismiss the portion of the etiological chapter which relates to the Predisposing causes of disease, with the simple remark, that a very comprehensive, and at the same time clear and concise, summary is given of these; such as will furnish the student with a useful guide in the direction of his observations, without burthening his memory with a mass of details. We are surprised, however, to find no notice under this head of the predisposition to disease induced by defective cleanliness, ventilation, &c.; which are treated under the next head, as Exciting causes of disease. In this section, considerable additions have been made; especially in the portions which treat of mechanical, chemical, and dietetic causes of disease, defective cleanliness, ventilation, and drainage. The following table shows the classification adopted by Dr. Williams, and the range of subjects embraced under this head. The division between predisposing and exciting causes appears to us to be purely artificial; and we are not sure that any real benefit is derived from the perpetuation of it. Many of the causes here set down under the latter head may be as well, or perhaps better, classified under the former. Thus the effects of deficient or improper food, of imperfect ventilation, of mental exertion, &c., very frequently manifest themselves, not in the production of any specific disease, but in predisposing the system to suffer from the attacks of almost any disease whose special exciting cause may be brought to bear upon it. In fact, as we have attempted to show with regard to the Indian cholera, almost any class of causes may be either predisposing or exciting, according to the manner and degree of their operation.

I. Cognisable Agents . .	1. Mechanical 2. Chemical 3. Ingesta 4. Bodily exertion 5. Mental emotion 6. Excessive evacuation 7. Suppressed or defective evacuation 8. Defective cleanliness, ventilation, and drainage 9. Temperature and changes
II. Non-Cognisable Agents	1. Endemic 2. Epidemic 3. Infectious.

The attention that has been recently paid to the subjects of ventilation, drainage, &c. by (we may presume) the great mass of our readers, spares us the necessity of noticing, at any length, the additional matter introduced by Dr. Williams under these heads. We may remark, however, that the personal experience of the practical physician, together with the acute perception of the scientific physiologist, manifest themselves throughout. The latter is displayed in the following suggestion with regard to the cause of the oppressive feelings produced by respiration in a damp atmosphere.

"The ill effects of deficient ventilation are increased by heat and moisture; the former operating not only by increasing the animal exhalations, but also by rarefying the air, and thus reducing the amount of oxygen in a given bulk: moisture probably acts in a degree in like manner, but also, as I conceive, by removing the difference between the air respired and that in the lungs, which promotes that diffusion or interpenetration of gases, on which the access of oxygen to the vesicular structure of the lungs depends. For, be it remembered, the air taken in at each inspiration is not enough to reach far in the tubes; its transfer into the air-cells is accomplished by the law of diffusion of gases, which operates in proportion to the dissimilarity between the gases; and difference in amount of contained watery vapour must exemplify this law." (p. 48.)

The following remarks we quote on account of their practical importance. The medical practitioner may think the subject of them out of his province; but he must learn to think the investigation of the *causes* of disease to be really of more certain benefit to mankind, than his attempts at the *cure* of diseases already established. The results of the removal of the former are capable of being determined with considerable accuracy; those of the latter are as yet problematical, and are liable to be called in question by the opponents of received systems. If a doctor does not think it beneath him to inquire into the drainage of his patient's system, to test his urine, or to inspect his fæcal discharges, we cannot see why he should hesitate to make personal inquiry into the state of the drains of his patient's house, when he suspects that their inefficiency is a cause of his disease. After speaking of the noxious operation of the cesspool, Dr. Williams thus continues :—

"Scarcely less injurious, and more insidious in its operation, because the effluvium is less offensive, is the untrapped drain in connexion with the sewers of large towns. This cause of disease exists extensively in London, not only in the street drains, which are always open, and emitting the gases of the sewer, the bad odour of which is perceptible in certain winds; but also in the drains of houses, which are either intentionally or negligently left open, or are not air-tight, from the absence of water in the traps. Nothing is more common than to perceive the peculiar smell of the drain on entering a house, and in many instances I have found that this has proceeded from the trap left open, or dried up, and therefore inoperative, and requiring only the simplest expedient to stop the evil. When a single trap is open in a house, especially in the winter, when doors and windows are closed, and there is no adequate supply of air for the fires in the house, the foul air is drawn up from the sewer in a strong current, and quickly pervades the house from bottom to top, carrying with it a pernicious influence. It is surprising how ignorant servants and employers, and even professional men, are on this point, which so immediately concerns their health and comfort; and I have visited in many houses where this has seemed to be a cause of illness or impeded convalescence, in low nervous fevers, bowel complaints, influenza, neuralgia, headaches, and other ailments. In some instances, the leakage may be in consequence of the

inroads of rats, or the displacement of the brickwork of the drains. It may be useful to state, that besides by the smell, which is not obvious to every one, the effluvia of drains may be detected by the darkening of white paint, and the early spoiling of meat in the lower basement story of the house." (p. 50.)

We now pass on to the second chapter, which treats of "Pathology (proper)—the Nature and Constitution of Disease;" and as we deem our author's method of discussing this subject to be highly philosophical in its conception, and to be the one best adapted to meet the progressive requirements of both teacher and student, we shall place it before our readers as fully as our limits will allow; not, however, without some criticism upon certain of the details of Dr. Williams's system. He commences by remarking, that as the change in the natural, functional, or structural condition of the body, which we call disease, is generally more or less compound, involving several elementary functions or structures, it is obvious that we cannot obtain an accurate knowledge of the nature of disease as a whole, until we have ascertained that of its component parts. And as the chemist applies the term *primary element* to substances that cannot be analysed or divided further, and the term *proximate element* to compound bodies of simple constitution, which act as single bodies in forming and giving the character to more complex compounds,—so the physiologist has his primary or ultimate elements of structure (the *elementary tissues*), and primary elements of function appertaining to that structure, which, when disordered, become to the pathologist the primary elements of disease; whilst there are secondary or proximate elements of structure (the various *organs*), and corresponding elements of function, which, when disordered, become the secondary or proximate elements of disease. The varieties of disorder affecting the proximate or the ultimate elements may be comprehended under the heads of *degree* and *kind*; degree including *excess* or *defect*, or alterations of plus and minus;—and kind relating to changes not comprised under these heads, but otherwise expressed by the term *perversion*. The following is Dr, Williams's table, representing his classification of the primary or ultimate elements:

TABLE I.—PRIMARY ELEMENTS OF DISEASE.

PRIMARY CONSTITUENT.	FUNCTION.	DISORDER.	STRUCTURAL DISEASE.
Contractile Fibre	Irritability Tonicity		
Nervous Structure	Sensibility Vol. Motion Reflex action Sympathy		
Secreting Structure	Secretion	Excess	Hypertrophy
Constituents of the Blood:— Red particles Fibrin and white globules Albumen Oil Salts Water		Defect Perversion	Atrophy Degeneration, &c.
Changes of the Blood:— By Respiration — Secretion — Assimilation — Foreign matters			

(p. 68.)

We fully admit the difficulty of carrying out the views just expressed with entire consistency; but we think that our readers must be struck, like ourselves, with one glaring error in this table;—namely, that *changes* of the blood are set down under the head of structural constituents. Surely these changes are *functions* of the constituents of the blood, and should have stood opposite to them in the second column of the table. We shall have another fault or two to find, when we have brought the second table under consideration; but it will be but just to Dr. Williams, first to quote his preliminary explanation:

"In the choice of proximate or secondary elements of disease, we must be the more arbitrary and less comprehensive; otherwise we shall encroach upon the domain of special pathology. The vascular system and the nutrient function so intimately connected with it, present us with the best and most important examples of proximate elements, comprising, as they do, two or more of the preceding ultimate elements (irritability, tonicity, constituents of the blood, secretion, &c.) yet so universal throughout the body as to belong to the province of general pathology." (p. 69.)

TABLE II.—PROXIMATE ELEMENTS OF DISEASE.

The Blood in Circulation	Defective—Anæmia	General		
		Partial		
	Excessive—Hyperæmia	General—Plethora	Circulation	Increased—Sthenic
				Diminished—Asthenic
		Partial—Local hyperæmia	Circulation	Increased—Determination
				Diminished—Congestion
				Partly incr. / Partly dim. — Inflammation
	Perverted—Cachæmia			
Nutrition of Textures	Defective—Atrophy			
	Excessive—Hypertrophy			
	Perverted	Degenerations		
		Deposits		
		Growths		

(p. 69.)

Now, it strikes us very forcibly, that Dr. Williams has not by any means carried out his own plan in this distribution of his subjects; as must, we think, be at once apparent to our readers from the repetition in the second table, under the head of "Nutrition of Textures," of some of the very same subjects—for anything, at least, that appears to the contrary—as were comprehended in the first. And we believe that a careful analysis of both tables will show that each includes subjects which properly belong to the other. It is in no fault-finding spirit that we make these remarks; but simply with a desire to aid the laudable endeavours of Dr. Williams in giving to pathological inquiry its right direction. Now commencing with the primary constituents of the animal body, we find contractile fibre occupying the first place; and its two modes of action, designated by the terms irritability and tonicity, hold a corresponding

place under the head of function. The function may be disordered by excess, defect, or perversion; and the structure may exhibit hypertrophy, atrophy, or degeneration. Coming next to the nervous structure, we find, to our surprise, sensibility, voluntary motion, reflex action, and sympathy set down as its elementary functions; no distinction being made between the vesicular and tubular forms of nervous matter, and no recognition being given of the fact that they have distinct and independent functions, which must be considered as the primary or elementary operations of the nervous system, analogous to contractility and tonicity in the muscular; whilst sensibility, voluntary and reflex action, and sympathy, must be regarded as compound functions resulting from the association of the simpler ones, just as certain associations and connexions of vesicular and fibrous matter make up the complex organs termed brain, spinal cord, or ganglia. The elementary structures and functions concerned in animal life having thus had the first places in the classification, we might have expected next to meet with an enumeration of those by whose aggregation the vegetative life of the organism is made up; from these, however,—on what account we cannot divine—we find only the secreting structure and the constituents of the blood selected; the other tissues, their properties and functions, being wholly passed by.

In regard to the proximate elements of disease, enumerated in the second table, we have already remarked that the simple changes in the nutrition of elementary textures, which form its lower division, constitute a mere repetition of a part of the preceding group. The condition of the blood in circulation, on the other hand, we regard as a very fair specimen of the real "proximate elements of disease;" and it might not have been difficult to extend the list by introducing similar groups founded upon the disorders incident to other complex functions, such as those of the nervous system, which we would transfer from the first table, with those of respiration, secretion, &c., which are themselves complex as regards the number of elementary changes on which they depend, but are simple in their relations to the system as a whole. We fully admit that, in the present state of pathology, it would be impossible to establish a really definite and stable classification of ultimate and proximate elements of disease; but this is no reason for not making the attempt. If the chemist were to wait for his ultimate elements and proximate principles to evolve themselves, he might wait long enough; and the only true way of finding truth in any science is first to find out where she is to be looked for. We do not say that if we were to make a classification of our own, it would be, on the whole, more free from objections than Dr. Williams's; and we would again beg that our criticisms may be viewed simply in the light of contributions towards a possible arrangement that should be superior to either. We again repeat that his *method* appears to us to be thoroughly philosophical; and the more strictly it is carried out in accordance with sound physiology, the more perfect will be, in our apprehension, the basis thus laid for a sound pathology.

The treatment of the individual subjects embraced in the foregoing tables is on the whole very clear and satisfactory, considering the very narrow limits within which it was necessary to condense a great mass of details. In regard to muscular irritability, Dr. W. holds the doctrine which we have always advocated, that it is a property inherent in muscular

tissue; and in speaking of its excess, he clearly distinguishes between the increase of irritability as shown in a readiness to contract on a slight stimulus, and the increase manifested in the force or duration of the contractions. As we have shown on a former occasion (vol. i., p. 358), the use of the term irritability in two distinct senses has led to much confusion, and will continue so to do, so long as it shall be persisted in; and we could wish, therefore, that Dr. Williams had not applied the term in a manner which recognises both these senses, involving, as it does, the palpable inconsistency of setting down two contrary states as manifestations of the same power. For we are told (and correctly so) that "inordinate mobility of muscle often co-exists with want of power or completeness in the contractions; as is exemplified in the irritable heart, which, although acting very frequently, does not expel its contents so vigorously as in health." (p. 71.) But in the next page we learn that deficient force of contraction is a manifestation of deficient irritability; and as the proposition just quoted might be as correctly put in this form,—"Want of power or completeness in the contractions often co-exists with inordinate mobility of muscle,"—we might have the very same condition of the heart ascribed to either increased or diminished irritability, according as we intend specially to designate its mobility, or its contractile force.

The section on tonicity we regard as peculiarly valuable; since it gives a definite and local meaning to terms which practical men have employed vaguely and erroneously; and thus lays a foundation for the correct apposition of a considerable number of morbid phenomena, the relation of which is commonly lost sight of. We quote the following paragraph as a good illustration of Dr. Williams's method of treating the several ultimate elements of disease:

"Where tonicity is *defective*, the muscles are flabby, and incapable of continued exertion, but are sometimes too irritable with the tremulousness of debility. The heart likewise is irritable, and often exhausts its strength in palpitation; the pulse is soft and yielding; it may be full when slow, and sharp when frequent; but it is without firmness or endurance, and is easily accelerated. Another destructive character is its retardation, increasing the interval between the heart's beat and distant pulses; so that the radial pulse is often felt after the second sound of the heart is heard; the tubes being less tense, the pulse-wave is slower than usual. Sometimes the absence of that tightening of the walls of the arteries by which the tonic fibres control their movements, permits their mechanical elasticity to come into play, and this reacting after each stroke of the heart, gives that peculiar reduplication or rebounding of the pulse, which has been long described under the term *dichrotous* pulse. This is often observed in convalescence from fevers, and other diseases after the subsidence of vascular excitement. A loose, relaxed state of the vessels renders the circulation in distant parts weak, so that the extremities are cold, whilst the head and internal organs may be congested. Sudden exertions or change of posture may disturb the circulation, and cause faintness or giddiness. Want of tone also in the stomach and intestines causes indigestion and costiveness, and permits them to become distended with wind and accumulating fæces. The secreting organs, irregularly supplied with blood, are also liable to disorder, being either scanty, depraved, or profuse and watery.

"It is quite obvious that a person in such a condition must be prone to various diseases. He has no resisting power against malaria, infection, or other depressing agents. If he is exposed to cold, the blood is readily driven through the weak vessels into the interior, where it causes congestion or inflammation. The weak intestines have no power to expel offending matter from them. Thus

the system in a state of atony is open to the action of many exciting causes of disease; besides being itself, in many respects, on the verge of disease, especially congestion and its consequences, and other derangements of the circulation." (pp. 77, 78.)

The section on the functions of the nervous system presents little that calls for remark. We have already pointed out what we regard as a fault in this part of Dr. Williams's classification; and we need not here do more than express our opinion, that nearly the whole of this section might with propriety be transferred to the chapter on the *proximate* elements of disease. The functional derangements which constitute the *ultimate* elements of disorder of the nervous system, appear to us to be those which relate to its general power of receiving and responding to impressions; whether these be of a kind which produce sensations and thus arouse the higher mental operations, excite reflex motions, or modify the vegetative functions. There is sufficient reason, we think, for regarding all these disorders as dependent upon similar derangements of the property of excitability; one phase or another presenting itself, according to the part of the system more especially affected; and all not unfrequently manifesting themselves together or in succession in the same individual. There seems to us to be the same kind of fundamental relation between these disorders, as there is between the weak and iregular action of the heart and the torpor of the bowels, which, as just shown, are manifestations of the general deficiency of tonicity in the muscular system. The state to which we refer must be familiar to every experienced practitioner, and it is not passed over by Dr. Williams. After noticing the excessive sensibility manifested in determination of the blood to the encephalic nervous centres, and in the early stage of inflammation, as shown in intolerance of light, noise, or motion,—Dr. Williams refers to the phenomena of hydrophobia and tetanus, which he speaks of as proceeding from excitement without inflammation. Now, whatever may be the case with regard to the former of these diseases, we think it must be apparent that those of the latter are not dependent upon an increase of *sensibility;* since they present the most complete and typical illustration possible of increased excitability, manifesting itself in the production of reflex motions from the slightest impression, in which sensation does not necessarily participate. Let this increased excitability affect the spinal cord, and we then have tetanic spasms. Let it affect the ganglia of sensation, and we then have intolerance of light, sound, &c.; and if it also involve the motor apparatus connected with them, we have the paroxysms of hydrophobia. If the cerebral structure be the part specially affected, we shall have excessive mental excitability; which may be manifested either in undue but imperfect activity of the intellectual powers, or in violent emotional agitations. Now all these conditions, which are seen in their completest development when specially and singly exhibited, are presented in combination, though less obviously and strikingly manifested, in persons suffering under undue excitability of the nervous system in general, rather than (as represented by Dr. Williams) under excess of sensibility. His description of this state is a true and graphic picture.

"Such persons are commonly called nervous; they are worried with trifles; startled at shadows; distracted by noise or bustle; never free from some ache or pain; for almost every feeling is suffering; and what in others would be slight

pain, in these amounts to agony. Hence they are perpetual invalids, quite unfit for the rugged path of life, over which they, as it were, walk barefooted and thin-skinned. If real disease attack them, its nervous symptoms are so much exaggerated, that a medical attendant is apt to fall into the error of either ascribing all to the 'nerves,' or of measuring the disease by the severity of the symptoms. This over-sensibility is generally conjoined with excess of irritability and want of tone. Other nervous functions, such as sympathy and reflex actions, are also augmented or in disorder. The symptoms connected with sensation most frequently present are neuralgic pains of various parts, excessive sensibility of the surface, headache, pain in the back or left side, and spinal tenderness." (p. 80.)

This undue excitability of the nervous system is one of the most prominent features of the disorder which, for want of a better name, we term hysteria; and any one who will take the trouble to analyse its proteiform manifestations, will come, we feel assured, to the conclusion that one general state is common to all those in which there is an excess of action, whether of the sensory or motor system; the variations in the phenomena being due to differences in the part of the nervous system chiefly affected.

Considerable additions are made in this edition, under the head of "Diseases of reflected and sympathetic nervous influence;" and these show a full acquaintance with all that physiology can contribute towards their elucidation. As, however, they contain nothing of absolute novelty, we need not detain our readers with any further reference to them.

The next section, on Diseases of Secretion, undoubtedly stands in its proper place, as treating of one of the really ultimate elements of disease. There is no operation in the body whose nature is more elementary; and there is none which is more intimately connected with the maintenance of life and health, through its influence on the purity of the blood. The subject is treated by Dr. Williams with great clearness; considering, however, its vast importance, both in a scientific and practical point of view, we think that this section might have been a little more expanded with advantage. We quote the following passages as an illustration of the important aid derivable from the study of the function in its totality, according to the method pursued in this work.

After noticing the chief peculiarities of the biliary and urinary excretions, and pointing out the necessity of a balance or due proportion between them, the author continues:—

"Whether the materials from which these eliminating processes are supplied be the principles of the blood itself, or the decayed constituents of the tissues, or matters derived from the food, the co-operation of all these processes will be generally required to maintain a uniformity in the composition of the circulating fluids; so, too, if one of these processes is more active than the others, the blood must suffer by the excess of those matters which the less active processes allow to accumulate in it. A clinical illustration of this position may be found in cases of bilious diarrhœa or cholera. This flux of bile is either accompanied by a highly loaded state of the urine, or by fever; in the latter case, the fever does not subside until the urine becomes very copious, or deposits an abundant sediment. The most probable interpretation of this fact is, that the excessive secretion of bile disorders the composition of the blood; so long as the kidneys rectify this disorder, by separating in greater abundance the solid contents of the urine, no fever results; but if the kidneys fail in this task, fever ensues, and continues until they accomplish it; then a free secretion and copious deposit is symptomatic of the decline of the fever." (p. 103.)

"If an excessive secretion have already caused febrile disturbance, great advantage will be found to result from the use of means which increase other secretions, and thus restore the balance before explained. Thus, in bilious cholera, saline diuretics and diaphoretics are highly serviceable. In renal irritation with copious secretion of lithic acid, blue pill, which augments the secretion of bile, is often beneficial. These means may be supposed to operate partly as derivants; but the manner in which they remove the febrile irritation, after the reduction of the excessive secretion, renders it most probable that they act also by removing from the blood dregs left by the inordinate separation of the matter of the single secretion which has been in excess. No practical physician can doubt that we possess medicines which often augment the secretions of particular organs (mercury that of the liver and salivary glands, colchicum that of the kidneys, &c.), yet there is a limit to the operation of these agents; but this limit may be increased by simultaneously acting on other organs which maintain the balance. Thus, in any disturbance of the secretions, especially if it continue long, combinations of medicines are much more useful than those fulfilling one indication only; and thus experience has sanctioned the practice of conjoining mercurials with diuretics, and antimonials with salines, &c." (p. 105.)

The following remarks are equally philosophical and practically valuable:

"The positively noxious properties which excrementitious matter retained in the blood is known to possess, must be taken into account when we attempt to explain the states of constitutional irritation and depression, with perversion of functions, which fevers so generally present. The changes in the blood, manifest in some such cases by its fluidity and by petechial appearances, may also be in part referred to defective elimination of effete matter;* and it is when the secreting organs recover their power, and a diarrhœa occurs, or a copious discharge of highly-loaded urine, that these appearances cease. It is very probable that severe mechanical injuries or shocks, and animal and other poisons, operate by thus injuring the vital powers by which the blood is continually purified from its own noxious products; and that this is a part of their mode of action seems almost certain, from their effect in suppressing or impairing the natural excretions. Accordingly, in such cases urea has sometimes been detected in the blood." (p. 107.)

An extremely interesting case, fully bearing out this view, has recently been put on record by Dr. Shearman. (Edinb. Monthly Journal, March, 1848, p. 666.) A boy of eight years old was run over by a truck, which passed across his loins, evidently inflicting some considerable internal injury. From the collapse which at first supervened, he recovered, under the influence of warmth and stimulants; but he passed no urine for thirty-six hours after the accident, and that which was then discharged contained a large quantity of blood. Dr. Shearman examined this bloody urine most carefully; but failed to detect the least particle of urea or urates in it. About sixty hours after the accident, there was a considerable access of fever, with increased pain in the region of the kidneys; and these symptoms, in the course of two days, were succeeded by coma. The boy was then bled from the arm; and on making a chemical examination of the blood, urea was most distinctly detected in it, and in considerable quantity,—the urine, at the same time, not containing a particle of urea, urates, uric acid, or albumen, and its specific gravity being only 1·005. Dr. Shearman then got his little patient under the influence of mercury as quickly as possible; and as soon as the constitutional effect of this

* "Purpura I have found to be often connected with hepatic congestion and imperfect excretion of bile, and to be most effectually removed by remedies which promote the restoration of the proper secretion."

agent was produced, urea gradually reappeared in the urine, and its specific gravity increased. By degrees the comatose symptoms subsided, and in the course of five weeks his usual health was re-established. This case is headed "Suppression of the Secretion of Urea by the Kidneys, and Absorption of Urea into the Blood;" but we apprehend that it would be more correct to substitute for the latter half of the title, "Accumulation of Urea in the Blood;" since there could have been no *absorption*, unless the urea had been first *produced* by the action of the kidney, which would not seem to be the case. When the liberation of carbonic acid by respiration is checked, and it consequently accumulates in the blood, we do not speak of this gas as absorbed into the blood; since its presence there is the result of the changes which take place during the passage of the fluid through the systemic capillaries; and we apprehend that the same is the case with regard to the presence of urea in cases of suppressed secretion.

We must indulge our liking for this chapter by another quotation, in which a sound principle of treatment is well enforced.

"The specific stimuli of the secreting organs, if used in excess, or too long, may not only cause general weakness, but also exhaust the vital powers which they excite; and the result may be a diminution either of the secreted fluid, or of its most characteristic constituents. Hence the long and excessive use of mercury causes torpidity of the liver; that of purgatives, imperfect action of the bowels; that of diuretics, scanty urine, or albuminous or watery urine, defective in urea. These facts point out the expediency of intermixing the use of these agents, and of alternating or conjoining them with others calculated to improve the vital properties of the textures generally, which may often be effected by the medicines called tonic, and by regiminal means which improve and equalize the state of the circulation, and preserve the digestive and assimilative functions in the best order. In illustration of this position, I may refer to the acknowledged advantage of giving bitters with or after mercurial courses; chalybeates with or after saline aperients or diuretics, when these are long used; and these additions, which alone, or used at first, would check the secretion to be increased, now sustain it, and render it permanent. Some medicines which are inferior in efficacy to those already named, are yet, in some instances, more eligible for chronic cases of defective secretion, because they are less exhausting, and combine some measure of tonic influence with that of increasing the secretions. As examples of this kind may be named taraxacum, preparations of iodine, sarsaparilla, nitric and nitro-muriatic acids. Courses of these medicines are sometimes of great efficacy in keeping free the secretions, after they have been restored by more powerful means; and they likewise often improve the functions of digestion and nutrition." (pp. 108-9.)

We are rather surprised not to meet in this chapter with a distinct recognition of the importance of the large extent of glandular surface presented by the intestinal walls, especially in the large intestine, as a means of depurating the blood. It has been too much the custom to regard the fæcal evacuations as little else than the indigestible residue of the food, mingled with portions of the biliary and pancreatic secretions; whereas we think that a little consideration serves to show that the peculiarly *fæcal* matter is a real excretion, which must have been eliminated from the blood by the glandulæ of the intestinal walls. The undigested residue of the food may form a greater or smaller proportion of the *bulk* of the evacuation, according to the nature of the ingesta and the completeness of the digestive process. When the alimentary canal is in an irritable state, and the ailment is hurried through it without time being

allowed for the proper action of the gastric and other secretions, a considerable part of it may be recovered from the fæces in almost unchanged condition. It is well known that the coats of seeds and the skins of fruits resist the gastric solvent; so that, if they have not been mechanically ruptured, their contents will pass out unchanged, the seeds not being in the least the worse as regards germinating power for having passed through the intestinal tube. It has been found that even starch-vesicles, if not ruptured by the masticating process, or by the heat employed (by cooking-animals) in preparing the food, resist the digestive process so completely, as not to give up their contents, being readily detectible by the microscope in the fæces. Further, there is no evidence whatever, that the undigested residue of the food *could* acquire the fæcal character during the short period which suffices in the state of health for its transmission along the alimentary canal; and there is every reason to believe the contrary; since the substances which resist the action of the gastric solvent are precisely those which have least tendency to this kind of decomposition. Moreover, in purely carnivorous animals, and in man, when he adopts the same diet, the food is *completely* soluble, and there is consequently no indigestible residue; yet fæces are still voided, though in smaller quantity than in herbivora. The case is still stronger in regard to sucking animals. The continued evacuation of fæcal matter, when little or no food is taken in, the large quantity brought off by purgative medicines after the bowels have been completely emptied of their solid contents, and the colliquative diarrhœa which so frequently occurs at the close of exhausting diseases, are so many obvious confirmations of the same view. To many of our readers it will doubtless be familiar; yet we are disposed to think that the idea of the intestinal glandulæ as performing a most important part in the depuration of the blood, by eliminating from it the *putrescent* results of the decomposition of the solids and fluids of the body, is not generally entertained with sufficient definiteness. It would be doing injustice to Dr. Williams, however, if we were not to state that he has elsewhere recognised the importance of some of the phenomena in question, and has put what we believe to be the true interpretation upon them. We find the following note in reference to the follicular enteritis of typhus in a later part of the volume:

"I have observed an extraordinary development and inflammation of the isolated and grouped glands of the intestines in the bodies of persons poisoned with arsenic. Their enlargement in epidemic cholera, and in the severe form of sporadic cholera, diarrhœa, and dysentery, caused by putrid effluvia, is well known. Are these glands excretory organs for the elimination of poisonous or noxious matters from the system? And in typhus fever, do they become inflamed and ulcerated by the continued operation of the poison in the exercise of this function? The favorable influence of moderate diarrhœa in fever, the uncommon fetor of the stools, the general relation between the duration of the fever and the affection of these follicles, the salutary operation of mild mercurial remedies which promote their secretion, and other facts that might be adduced, give so much countenance to this question as to make it worthy of attention."

"The preceding surmise, put forth in the first edition of this work, has received corroboration from numerous facts which I have subsequently observed; and it seems to me to indicate the true cause of the intestinal complication in fevers and other diseases induced by a morbid poison in the system." (p. 248.)

The doctrine that the proper *fæcal* matter is not derived from the food,

but is an excretion from the blood, has been recently advanced as new by Professor Liebig (Animal Chemistry, 3d ed., pp. 143 et seq.); but, strangely enough, he found himself at a loss for a glandular apparatus which he could regard as the special instrument of the function. He has added, however, one new fact of much interest; which indicates that the substance to which the fæces owe their peculiar fetor is due to the imperfect oxidation of albuminous compounds.

"If," he says, "we heat in a retort one part of white of egg and three of hydrate of potass, so as to melt the mixture, and continue the heat till the disengagement of ammonia has nearly ceased, and if we then supersaturate slightly the contents of the retort, after cooling, with dilute sulphuric acid, and distil, we obtain, along with a disengagement of carbonic acid and sulphuretted hydrogen, a liquid which is slightly acid, from the presence of acetic and butyric acids, and which has the most horrible fæcal smell. The substance to which the smell belongs is soluble in water and alcohol; it combines with alkalies, without, however, neutralizing them. When exposed to the air, it is rapidly changed. By means of caseine, gelatine, and fibrine, when treated in the same way, we can procure all the different varieties of fæcal odour." (Op. cit., p. 154.)

We look upon this question as one of great importance; since on the view we take of it much of our practice will depend. If it be true that the intestinal surface contains an extensive glandular apparatus, whose special function is the elimination of certain products of decomposition from the blood, the facility with which we can stimulate this to increased action, by certain kinds of purgative medicine, gives us a most valuable means of augmenting the depurative operation. We are no friends to the indiscriminate use of purgatives, which is the vice of many practitioners who pride themselves on their active treatment; but, on the other hand, we cannot regard them with the horror which some entertain. Seeing, as we think that no observant practitioner can avoid doing, how frequently Nature herself employs this means of eliminating morbific matter from the system,—as is shown by the immense relief often given by a spontaneous attack of diarrhœa,—we look upon this apparatus as one which, like the liver, the kidney, or the skin, may frequently with propriety be stimulated by medicines which have a special action upon it, and one through which many morbific matters may be eliminated more certainly and speedily than through any other channel. But we would on no account encourage the system of violent purgatives, which is the facile resource of the routine practitioner who wishes to gain credit for the vigour of his remedial measures, but who does not know what else to be doing; and which is the favorite prescription of the amateur doctor or doctress, who thinks that no medicine can be good for anything, if it does not produce some such ostensible result.

We must not dwell long on the next section, which treats of the "Diseases of the Constituents of the Blood;" but have pleasure in expressing our opinion that it is the most complete summary anywhere to be met with, of what is definitely known on the subject. Independently of the condensed but discriminating account given by Dr. Williams of the labours of others, numerous original observations are introduced, which show that he has not been a slothful labourer in the same field. Of these we may select the following as an example:

"In several cases of Bright's disease of the kidney, I have observed the blood-

discs jagged or crenate at their margins, and otherwise imperfect; and the same remark has been made by Simon of Berlin, and others, and by Andral in a case of chlorosis. In one fearfully rapid example of albuminuria, which proved fatal in six days, with effusion of pus in the joints the day before death, I found the colouring matter dissolved in the blood-liquor after death, and scarcely any red discs remaining. There were numerous pus-globules in the blood. A similar total destruction of the blood-discs was observed in University College Hospital, in the blood of a person who died of malignant scarlet fever with purpura. I have met with similar proofs of breaking-up of the red particles, but to a much smaller extent, in acute purpura connected with jaundice, and in cases of disturbed function of the liver without jaundice: is this due to the remarkable solvent power exercised by small proportions of bile on the red particles, noticed by Simon and others?" (p. 115.)

To the crenate margin of the blood-corpuscles our own observations do not lead us to attach much importance; as we have found it so constantly presented by the blood-corpuscles of certain individuals of our acquaintance, of whose healthy condition we could feel assured, that we cannot regard it as indicating any considerable departure from the normal condition of these bodies. But the other facts recorded by Dr. Williams are valuable contributions towards a knowledge of the pathological conditions of the blood; as are also the following observations, which tend to correct an erroneous notion which has of late gained currency in regard to the increased amount of fibrin in the blood of scrofulous subjects.

"It is a fact of great importance, that the quantity of fibrin in the blood, and the facility with which it may be effused, are by no means in proportion to its plasticity, or capacity to become organized; thus it is abundant in the blood, and freely effused in the inflammations of scrofulous and tuberculous subjects, although in such the products of these inflammations and of nutrition are commonly cacoplastic or aplastic. The fibrin of the blood or coagulable lymph in these cases is more opaque and less elastic than in healthy subjects, and under the microscope presents a predominance of granular matter and fat-globules, and less of the finely-defined fibres and regular nuclei, as if it were imperfectly elaborated, and resembled coagulated albumen rather than the more animalized form of protein. Even the more perfect forms of fibrin, if in a position in which their vitality is not sustained by becoming organized, tend to degenerate and become disintegrated into an opaque aplastic matter (Gulliver), and this eventually may undergo a further chemical change into fatty and calcareous matter, like other aplastic deposits. It is interesting to observe that in these cases also the red particles are deficient in number; and this suggests a probable cause of the imperfection of the plasma." (p. 131.)

We might dwell much longer on this part of the work, and might point to many important practical suggestions which arise from our improved and improving acquaintance with the pathology of the blood. If no other good result had accrued from the investigations of Andral and his coadjutors, than the knowledge we have gained of the real condition of blood in chlorosis, the worse than absurdity of bleeding, and the rationale of the remedial action of iron, in that disease, the labour bestowed upon them would have been far from unproductive.

Under the head of Changes in the Blood by Respiration, we find (p. 143, note) a very just comment on the purely mechanical view of this function taken by Professor Liebig; who seems to consider the increased extrication of carbonic acid during exercise or exposure to cold, as a necessary consequence of the greater amount of air inhaled, in one case by accelerated movements of the chest, in the other, by greater density of the cold air. We

quite agree with Dr. Williams in the belief, that the proportion of oxygen absorbed and of carbonic acid expired depends chiefly on the condition of the blood brought to the lungs; and that the respiratory movements are regulated by this. For the hypothesis of Professor Liebig is totally inadequate to account for the great disproportion between the amount of carbonic acid liberated by warm-blooded animals at a high and at a low temperature. Thus it appears, from the experiments of Letellier, that a turtle-dove exhaled 0·974 grammes of carbonic acid per hour in an atmosphere of 32° (Fahr.), whilst it only exhaled 0·336 grains per hour in an atmosphere between 86° and 106°. Thus the quantity of carbonic acid exhaled was very nearly *three times* as great at the lower temperature as at the higher; and if Professor Liebig's explanation were correct, the density of the air ought to be three times as great at 32° as at 106°, so as to cause three times as much oxygen to be introduced at every inspiration; or conversely, the bulk of air at 106° should be three times that of the same weight at 32°. But how stands the fact? Air expands 1-480th of its bulk for every degree of Fahrenheit; consequently the whole expansion between 32° and 106° would be 72-480ths or 3-20ths; an amount too small to have any considerable share in the result. And further, if, as Dr. Williams justly observes, the increased liberation of carbonic acid and evolution of heat, produced by exercise, were due merely to the acceleration of the respiratory movements, the increase could be produced at any time by a voluntary acceleration of these movements, which is far from being conformable to fact. Everything appears to us to indicate that the rate and extent of the respiratory movements are governed by the *besoin de respirer,* which itself depends upon the various changes taking place within the system; the adjustment being made altogether involuntarily and even unconsciously, and being one of that series of marvellous adaptations which so forcibly indicates an all-wise and all-powerful design.

The section on Changes in the Blood by excretion does not seem very distinct in its subject from that on diseased secretion already noticed; and as many of the facts which it includes had been previously stated under the former head, and as all might have been with equal propriety, we think that it would have been preferable if the two had been united under one designation, unless some more definite distinction could have been drawn between them. Little or nothing material is added in this section to what had been previously stated as to the results of imperfect elimination of biliary and urinary matters from the blood; and we have to notice the same absence of any allusion to the intestinal excretion; but the results of disorder of the perspiratory secretion are more fully dwelt on in the following passage, from which we perceive that Dr. Williams has formally adopted views that have for some time been acquiring a sort of floating credence as to the *materies morbi* of rheumatism.

"The perspiratory secretion contains lactic acid and lactates of soda and ammonia, which probably proceed from the transformation or decay of the textures, particularly the muscular, which the recent researches of Liebig have shown to contain a preponderance of this acid. Hence these products abound during great muscular exertion; and when perspiration is checked by external cold they may be retained in the blood, causing rheumatism, urinary disorders, or various cutaneous diseases. The very serious effects sometimes resulting from sudden cold on the

perspiring body may be partly owing to the same cause, as well as to the disorder produced in the circulation. Rheumatism is especially liable to occur as an effect of cold, where the body is fatigued with much muscular exertion; and I have frequently observed that the rheumatism chiefly affects the limbs which have been most exercised. Where the skin fails to excrete, an increased task is thrown on the kidneys, whence may result various diseases of these organs; and if these organs fail in the task, the lactic acid accumulates in the blood, and, probably acting as a ferment, causes the formation of more, and of the kindred products, lithic acid and its compounds and products: these, in inflammatory subjects, excite rheumatic fever: in cachetic persons, miliary fever, erysipelas, or pemphigus; and in more torpid frames, various local rheumatic or gouty affections. All these cases are frequently remarkable for the acid character of the cutaneous and renal excretions;* and in a few instances the blood has been found to possess acid qualities, or to be deficient in its usual alkaline reaction. In low forms of rheumatism, especially the neuralgic, the *materies morbi* is probably oxalic acid, as originally suggested by Dr. Prout; for I have, in numerous instances, found an abundance of the octohedral crystals of oxalate of lime in the urine, especially when the patients began to convalesce.

"The remedy for rheumatism and other diseases arising from defective excretion, therefore, should not be merely antiphlogistic, but also of a kind calculated to eliminate the morbid matter from the blood. In slight cases of rheumatism, sudorifics may suffice; but in others, the kidneys and liver should also be excited to assist in the process of elimination, and various combinations of colchicum and alkalies with mercury, opium, and iodide of potassium will generally effect this purpose very satisfactorily, and both speedily and permanently remove the disease.† Where the disease is more decidedly asthenic, and the urine exhibits a deposition of oxalate of lime with or instead of lithates, or acid phosphates, great advantage may be often derived from the use of means calculated to raise the tone and vital energies of the circulating and secreting organs, such as bark, quinine, arsenic, and iron; and they are the more eligible in cases of neuralgic rheumatism, because the attacks are periodic, with intervals of depression highly favorable to the use of these remedies." (pp. 151, 152.)

If Dr. Williams really does not deceive himself in regard to his success in the treatment of rheumatism, we are almost driven to conclude that the cases which fall under his notice must be of unusual mildness; since we have long known the very same treatment put in practice, with results by no means as favorable as regards *time*. We cannot feel by any means sure that, in imputing the entire mischief to an undue development of lactic acid in the system, we have by any means the whole truth; or that the severity of the symptoms bears any proportion to the amount of this substance abnormally present in the system. And we think that the matter is at present rather one for inquiry and investigation, than for dogmatic assertion. Dr. Williams does not mention the hot-air bath as one of his remedies; we have seen it of much service, especially in those cases in which the skin was unusually dry and the perspiration deficient.

Under the next head, "Changes in the blood from the transformation of chyle and of the textures," we find included gout and other lithic-acid diseases, diabetes, both saccharine and ureal, and obesity. All these

* "In patients with acute rheumatism, I have frequently found the perspiration of the affected joints more strongly acid than on other parts."

† "The advantages of this due regard to the essential elements of disease in the treatment of rheumatism may be shown by the fact, that, in upwards of two hundred cases of various forms of rheumatism, under my care in University College Hospital, the convalescence was established in from three to six days on an average, and the patients were dismissed cured in from one to three weeks after."

states are dependent upon disordered conditions of the processes of primary and secondary assimilation; and though apparently so dissimilar in themselves, they are very rightly brought together. It would not have been difficult, we think, to have referred certain disorders of the biliary secretion, or rather of the general system, to the same class of causes. Certainly the "bilious" habit of body, the bilious diarrhœa of summer and autumn, and other functional derangements of the same kind, are more frequently referable, in the first instance, to dietetic causes, than to disorders of the liver itself, which would discharge its duty properly if other organs did theirs. Dr. Williams adopts the fashionable creed that lithic acid is the *materies morbi* of gout, as lactic acid of rheumatism; but he does not afford any explanation of the fact, that the lithic acid and the gouty diatheses present points of difference which appear sufficient to indicate a want of absolute identity between them,—a fact which led one of our predecessors to suggest that it is the *lithate of soda*, rather than free lithic acid, which really constitutes the offending matter in the latter disease.

"This appears to us to be indicated by the fact, that the substance named is that which is separated from the blood in gouty deposits;* and still more by the known connexion of gout with biliary as well as with urinary derangements, and by the beneficial result of treatment directed to *both* these excretions. Under the influence of particular circumstances, *lithic acid* has a tendency to accumulate in the blood; and it seems to us quite possible that, so long as it retains its uncombined form, gout may not result. But if, by deficiency in the secretion of bile, *soda* also be allowed to accumulate, the two will combine, and lithate of soda will be formed. This hypothesis accords well with the fact that peculiar advantage, in the prophylactic treatment of gout, is derived from the occasional administration of blue pill or other mild mercurials." (Brit. and For. Med. Rev. vol. xvi, p. 468.)

This view is corroborated by the following facts, which we quote from Dr. Williams's appendix:

"Dr. Garrod has, in several other cases of gout, detected lithate of soda in very appreciable quantity in the blood; whilst at the commencement of a fit of gout there is a marked diminution of it in the urine. On the abatement of the attack, the lithic acid, or its compounds, appears in increased quantity in the urine, and that in the blood is therefore diminished. This exactly accords with the view that I have always taken of the nature of acute gout in common with other febrile diseases excited by a morbid matter in the blood, that the febrile excitement is the result of a reaction which may succeed in eliminating the offending matter, and in relieving the system from its influence. It appears further, from Dr. Garrod's analyses, that slight traces of lithic acid may be detected in the blood of persons who are comparatively healthy, or who are affected by other diseases than gout; but in this malady the amount is much greater. My chemical assistant, Mr. Edward Palmer (on whose accuracy I can fully depend), has separated crystals of lithic acid also from the blood of two of my patients suffering from degeneration of the kidneys with albuminuria. In acute rheumatism, Dr. Garrod found no increase of lithic acid in the blood; but I have little doubt, from its abundant appearance in the urine in many cases during the decline of this disease, that something analogous and easily convertible into it does exist in the blood of rheumatic patients, and that further researches will establish the chemical relation between two diseases which

* Dr. Williams states,—"A case of chronic gout, at present (June, 1847) under my care, has afforded an opportunity of verifying this observation: the matter obtained by puncturing the white tumours of his fingers is of the consistence of thick cream, and consists of very fine acicular crystals of lithate of soda, with a trace of lime."

are known to border so closely on each other, in regard to their symptoms and treatment." (p. 514.)

The following observation is interesting as showing how frequently, by a careful scrutiny, a satisfactory *rationale* may be discovered for symptoms, which we have been accustomed to consider as ultimate facts in the history of disease.

"I have in several instances found in the cortical and tubular structure of the kidney, clustered crystals of lithic acid, which under the microscope exhibited such sharp angles and dagger-shaped projections, as would afford an easy explanation of the pain, inflammation, and hemorrhage, often attendant on an attack of renal gravel, even when none is obvious in the urine." (p. 154, *note.*)

The last section of this chapter, on the "Changed properties of the blood from the presence of foreign matters," is rather disappointing from its meagreness; this being one of the departments of pathology in which the greatest advance has been made within the last few years, and in which the greatest progress may be expected. It must be admitted, however, that much of what we are accustomed to regard as *proven* under this head of the inquiry, rests upon indirect rather than direct evidence; since neither chemistry nor microscopy have yet been able to detect in the blood of patients labouring under *zymotic* diseases the morbid matter, to whose introduction the obvious depravation of the fluid and the disturbance of the whole series of functions are attributed. But the evidence of the existence of such morbid matter, derived from the comparison of the phenomena in question with those resulting from the agency of undoubted poisons, is so strong as to leave little room for hesitation with respect to its right to be considered as a *vera causa.* It is to be remembered that even in organic chemistry, we are continually in the habit of resting satisfied as to the presence of particular bodies, from the result of certain *tests* applied to the mixture or combination supposed to contain it, when circumstances do not admit of the ready isolation of the body itself. And there is one case—we refer to fluorine—in which chemists were long unable to isolate the body at all, owing to its strong disposition to combine with all other substances, and yet allowed themselves to feel no doubt whatever with regard to its real existence, since its reactions with other bodies were so decided and constant. It seems to us that we have a right, in many instances, to draw similar conclusions, in regard to the existence and agency of morbific matters, from observed phenomena; and that no unreasonable doubts ought to be raised on the ground of deficiency of purely chemical or visual proof of the presence of a foreign substance. For we think that the actions of the living body should be regarded as affording a series of *tests* of the purity of the blood, from which evidence as valid and satisfactory may be drawn (when we know how to appreciate it aright) as that on which we are in the habit of resting in ordinary chemical investigations. And on this account we think that the attention of pathologists ought to be specially directed towards the phenomena attending the action of poisons, which undoubtedly act by absorption into the blood, and to the parallelism of various morbid processes with these. Dr. Williams is by no means insensible to the importance of these considerations, as we judge from many passages scattered through his treatise; but we think it would have been better for the interests of science, as well as more consistent

with his general plan, had the inquiry been carried out more fully under the present head. As we purpose touching upon it, however, in another part of our present Number (Art. X), we shall not dwell further upon it here.

We have dwelt long upon this portion of Dr. Williams's 'Principles,' because it constitutes the distinctive feature of the work; the "Proximate elements of disease," which are next considered, as well as the "Diseases of nutrition," afterwards treated of, forming, in one shape or other, the great bulk of most treatises on general pathology. We shall not follow him through the succeeding chapters, therefore, as systematically and minutely as we have done through that which we have now passed under review; but shall content ourselves with a notice of such points as appear to us to be worthy of special attention, or to require comment or correction. We have already placed before our readers Dr. Williams's tabular arrangement of the proximate elements of disease, and have offered some observations thereon. With regard to the sections on anæmia and plethora we have only to remark, that they display the author's usual ability, and are admirable summaries of what is known of these pathological conditions and of the indications for treatment which they present. We must break a lance with him, however, on an old subject of debate, the discussion of which commences under the head of congestion,—namely, whether there is any cause, originating in the reactions between the blood and the surrounding tissues, which can modify the flow of blood through the capillaries, acting (in the normal condition) as an auxiliary to the heart's action, but capable, when perverted, of retarding or even checking the motion of the fluid. We are among those who have maintained, upon general physiological grounds, the affirmative of this proposition; since a comprehensive view of the phenomena of the circulation appears to us to afford ample evidence that the variation in the rate of movement of the blood through different parts, or through the same part under different circumstances, is mainly dependent upon the relative activity of the changes which take place between the blood and the tissues; and that there is a force generated in the capillary circulation, which, to say the least, exerts a *distributive* power over the blood, such as the heart's action is obviously unfitted to generate. We never felt, with Dr. Alison, the necessity for assuming the existence of a set of *vital* attractions and repulsions as the hypothetical cause of these phenomena; and were always disposed to believe that their explanation would be found in the *physical* conditions under which they occur,—a belief which has been fully confirmed by the researches of Professor Draper, elsewhere alluded to. (p. 45.) The phenomena of asphyxia have always been urged by Dr. Alison, Dr. John Reid, and other physiologists who have adopted corresponding views, as affording conclusive evidence that the cessation of the changes normally taking place in the blood during its passage through the capillaries produces a stagnation of its movement; the venous blood transmitted by the pulmonary artery to the lungs being prevented by the stoppage of the oxygenating process from passing through the pulmonary capillaries; and the non-oxygenated blood transmitted through the systemic arteries being retarded in its flow through their capillaries, because its want of oxygen prevents the occurrence of the reactions to which it ought to be there subservient. In his former

edition Dr. Williams stated that it remained for future observers to determine whether these obstructions are connected with contraction of the vessels, increased spissitude or cohesion of the blood, or other simply physical cause; or whether they depend on peculiar (vital) attractions and repulsions exerted between the vessel and its blood. He now states (p. 186) that the researches of Mr. Erichsen on asphyxia have satisfactorily settled the question in favour of the first of these alternatives; having shown that the presence of venous blood in the minute arteries excites a contraction of their walls, which is a sufficient obstacle to the flow of blood through them. We must own ourselves at a loss to perceive how these observations prove anything of the kind. The following is the essential part of Mr. Erichsen's account of the phenomena from which Dr. Williams draws this inference:

"'On asphyxiating a young rabbit, a portion of whose mesentery had been conveniently fixed under a powerful microscope, the following phenomena will be observed to ensue. For about a minute after the struggles of the animal have ceased, the circulation appears to be going on with its usual rapidity; it then gradually becomes somewhat slower, the arteries contracting in size, containing less blood, and assuming a lighter and more tawny colour than before; whilst the veins become congested, and evidently fuller, assuming, when viewed by transmitted light, a very beautiful crimson hue. As the circulation becomes more languid, the arteries continue contracting, and acquire a lighter colour; the diminution in their size, and the difference in the quantity of blood contained in them and in the veins being most marked. The motion of the blood in the capillaries now becomes oscillatory, the whole mass of blood being at each impulse from the heart slowly propelled forward, and then moving backwards. This to and fro motion continues for some time, and then ceases entirely. On restoring the heart's action by setting up artificial respiration, an impulse was evidently transmitted from the blood in the arteries to that in the capillaries, in a pulsatory and jerking manner, which was soon communicated to the veins, driving forward the whole mass of globules accumulated in them, and gradually becoming more equable and powerful until the circulation was completely restored. . . . . . The diminution in the diameter of the smaller arteries, and the proportionate difference between them and the neighbouring veins, was most evident, and was such as could leave no doubt in my mind as to the important part that the contraction of these vessels plays in giving rise to an obstruction to the passage of blood through them in asphyxia; in which I have no doubt that it is the principal, if not the sole, agent.'

"These observations," continues Dr. Williams, "correspond perfectly with what I have many times observed in the frog's web; and we may fairly infer that the same obstruction which operates in the systemic circulation will suffice also in the pulmonary. The notion of vital attractions of blood for the vessels, or even of the aggregation of the red corpuscles (suggested by an imperfect observation of Mr. Wharton Jones) as causing the obstructed circulation of asphyxia, is not only superfluous but untenable; for, were the obstruction in the capillaries and veins, the arteries, for a time at least, should exhibit distension, which the above description proves not to be the case." (pp. 187-8.)

Now let us inquire how far the facts of this case warrant the deductions of Mr. Erichsen and Dr. Williams. If contraction in the arteries were really the cause of the obstructed capillary circulation, the first effect ought to be that the stream would be accelerated; just as a force-pump sends a more rapid stream through a small jet than through a large one, and as, in the ordinary circulation, the blood moves through the arteries more quickly than through the veins in proportion to their difference of calibre. But no such acceleration takes place; on the contrary, the first

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"These observations," continues Dr. Williams, "correspond perfectly with what I have many times observed in the frog's web; and we may fairly infer that the same obstruction which operates in the systemic circulation will suffice also in the pulmonary. The notion of vital attractions of blood for the vessels, or even of the aggregation of the red corpuscles (suggested by an imperfect observation of Mr. Wharton Jones) as causing the obstructed circulation of asphyxia, is not only superfluous but untenable; for, were the obstruction in the capillaries and veins, the arteries, for a time at least, should exhibit distension, which the above description proves not to be the case." (pp. 187-8.)

Now let us inquire how far the facts of this case warrant the deductions of Mr. Erichsen and Dr. Williams. If contraction in the arteries were really the cause of the obstructed capillary circulation, the first effect ought to be that the stream would be accelerated; just as a force-pump sends a more rapid stream through a small jet than through a large one, and as, in the ordinary circulation, the blood moves through the arteries more quickly than through the veins in proportion to their difference of calibre. But no such acceleration takes place; on the contrary, the first

change in the rate is one of retardation; and this retardation continues to increase with the diminution of the supply transmitted from the heart. This supply becomes smaller and smaller, as less and less blood is returned from the lungs to the heart; and at last ceases altogether. What else is to be expected from the known properties of the arterial walls, than that the arterial tubes should gradually contract as they become emptied, just as they do after death? The observations in question thus become, to our minds, clearly indicative of the incorrectness of Mr. Erichsen's inference; and the objection raised by Dr. Williams to our view of the case, turns out, when properly examined, to afford an additional argument in its favour. If obstruction to the flow of the blood through the systemic capillaries be, as we maintain, the first change which the state of asphyxia produces in the systemic circulation, there will be for a time increased *pressure* within the walls of the arteries; but there will not be *distension*, unless the walls yield to that pressure. The negative evidence of Mr. Erichsen, as to the absence of arterial distension, by no means proves that there is not an increase of pressure; whilst the evidence of Dr. Reid, obtained by careful measurement with the hæmadynamometer, is positive proof to the affirmative. For he found that, in the first stages of asphyxia, there *was* increased pressure within the systemic arteries; indicative of an obstruction to the flow of blood through the capillaries, whilst the supply from the heart was but little if at all diminished. Very soon, however, the quantity returned from the lungs became rapidly smaller and smaller, so that less and less was propelled by the heart through the aorta; and the pressure within the arteries, as indicated by the hæmadynamometer, diminished in like proportion,—exhibiting an immediate increase whenever air was admitted to the lungs. As Dr. Williams is evidently not unacquainted with these experiments of Dr. Reid's, having referred to them in a preceding page, we are at a loss to understand how he could overlook their bearing upon this question.

The same fundamental error, as it appears to us, pervades Dr. Williams's account of the state of "active hyperæmia" or "determination of blood;" and we are particularly struck by the inconsistency between his ideas of the nature of this state, and those which he has put forth as to the causes of passive congestion with diminished movement of the blood. The latter state he considers, and we believe very justly, to be frequently attributable to diminution of tone on the part of the walls of the veins, causing them to yield to distension instead of resisting it, and thereby producing a mechanical impediment to the onward movement of the blood; and he has devised an ingenious experiment which affords proof that such a state will really act in the manner supposed (p. 188). But he attributes the determination of blood to a part whose functional activity is exalted, as a result of a corresponding deficiency of tone in the *arterial* walls, producing an enlargement of their tubes, and a consequent increase in the amount of blood passing through them; and he thus endeavours to explain away the inconsistency of viewing the two opposite conditions as results of a common cause:

"It has been objected that I assume enlargement of vessels to be the cause both of increased motion (in determination) and diminished motion (in congestion), which seems contradictory; but if the objector had duly considered my explanation, he would have found no contradiction in it. In determination, the vessels enlarged

are the arteries, which being near to the source of motion, and highly charged with its propulsive power, give vent to the current as from a reservoir of high pressure; whereas in a tonic congestion, the vessels enlarged are the veins and capillaries, which are remote from the source of motion, and receive their impulse only through the arteries, which are not enlarged, or are even contracted; and thus the accumulated blood becomes comparatively stagnant. There is nothiug contradictory in this simple application of hydraulic principles." (p. 205.)

Dr. Williams seems to have forgotten, however, one very important element in the comparison: namely, the *rate* at which the blood moves onwards. We are very ready to admit that, on simple hydraulic principles, more blood will flow, in a given time, through a part whose arteries are large, than through one whose arteries are of smaller calibre; but on the very same principles the *rate* of movement will diminish with the increase of the calibre of the tube; and this the more as the pressure is greater. At any rate the increase of the calibre of the tube cannot, under any circumstances, produce an increase in the rate of movement of the contained fluid, such as, on Dr. Williams's own showing, exists in active determination of blood to a particular organ. We must, therefore, look for some other cause of the increase; and we do not see where it is to be found, save in the general principle to which we have already drawn the attention of our readers.

Our want of agreement with our author on the subject of the capillary circulation, occasions a fundamental difference between our respective interpretations of the phenomena of inflammation. It is not onr intention, however, to enter at the present time upon a discussion of this much vexed question; and we shall confine ourselves to a brief indication of the points of divergence, with the view of guiding our readers in their own investigations. This we can best do by quoting Dr. Williams's summary of the "local elements of inflammation."

"Determination of blood towards the affected part.

"Obstruction of the vessels most affected; by a tonic enlargement of the capillaries; by production and adhesion of white corpuscles in the vessels.

"Distension of arteries and capillaries *before* the obstruction, causing increased effusion of serum, lymph, pus, &c.

"Emptiness of veins *beyond* the obstruction, causing increased absorption; hence softening, &c.

"Impeded or arrested circulation *at* the obstruction, causing a reduction or abolition of vital properties; hence the death of the part, and its removal by ulceration and suppuration, or its decomposition by gangrene.

"Increased circulation of blood *around* the obstruction, causing exaltation of vital properties; hence spasm, pain, sympathetic irritations, increased secretion, &c." (p. 321.)

There is no considerable difference of opinion between Dr. Williams and ourselves in regard to the ostensible phenomena of the inflammatory process. The retardation or total stagnation of the circulation *in* the inflamed part, and the acceleration of the flow in the *surrounding* vessels, are admitted by all observers. The diminished vitality of the solids of the inflamed part, as indicated by the suspension of their normal actions of nutrition, secretion, &c., or their absolute death and disintegration, is a point on which we fully agree with him. And we further accord with him in considering the increased production of white corpuscles in the capillaries of the inflamed part, communicating to the blood circulating

through it (and thereby to the entire current) an increase of plasticity, as one of the essential phenomena of the process. But we are at issue with him in regard to the interpretation of these phenomena; and especially with respect to their relation of mutual dependence. Dr. Williams considers that the disordered condition of the vital functions of the part is due to the retardation of the flow of blood through it; and that the retardation is dependent upon an altered state of the walls of the blood-vessels. It is in these, therefore, that he looks for the essential or fundamental phenomenon (or proximate cause) of the inflammatory process. In our apprehension, on the contrary, a disturbance of the ordinary relation between the blood and the tissues, usually depending upon a state of depressed vitality in the latter, is the origin of the mischief; a disturbance of the capillary circulation will ensue as a necessary consequence; and this disturbance tends to increase the mischief. In the mean time, a counteracting or conservative change takes place in the blood; the increase in the plasticity of which is destined to repair the mischief occasioned by the disintegration of the solid tissues. We thus should designate inflammation as a local perversion of the ordinary nutritive process; the circulation being affected only as it is influenced by the reactions taking place in the capillaries: whilst Dr. Williams would consider it as a local alteration of the capillary circulation, which induces consequential changes in the nutritive functions to which that circulation ministers. Those of our readers who may wish to see what is to be advanced on either side, should carefully study this chapter of Dr. Williams's treatise, and then peruse the review of the corresponding part of the former edition in the 'British and Foreign Medical Review,' vol. xviii, p. 103 *et seq*. We should be doing injustice to our author, were we to allow our difference of opinion with regard to the theory of inflammation to prevent us from recommending this portion of his treatise, as containing a masterly summary of the causes, symptoms, and results of the process, with an admirably condensed view of the principles to be followed out in its treatment.

In the fourth chapter we enter upon the "Structural diseases or diseases of nutrition;" which, it is admitted by Dr. Williams, might be regarded, like those of secretion, as belonging to the primary elements of disease; but which he deems it better to consider under a separate head, "both because the previous consideration of disorders of the blood and its vessels gives the best introduction to them, and because we cannot strictly distinguish structural disease into ultimate and proximate elements. By analogy, indeed," he continues, "we might infer that ultimate structural disease is that which affects elementary structures singly, such as muscular fibre, nervous matter, cellular texture, &c.; but we find structural disease to be rarely thus confined to one anatomical element, but rather to affect structures as they exist combined in more or less complexity." (p. 353.) We regarded this chapter, in the former edition of the work, as constituting one of its most distinctive and meritorious features; and we consider that Dr. Williams is fully entitled to speak in the following terms of the essential truthfulness of the views which he there put forward.

"Although considerable advances have been made in pathological anatomy, and several new works devoted to the subject have appeared since the publication of the first edition of this treatise in 1843, it is satisfactory to me to find that in no

material points has the advance of knowledge superseded the views there given of the elements of structural disease, but in many instances the views have been signally confirmed and extended. Under these circumstances, I have added to, rather than modified the text; and in preference to new classifications and uncouth nomenclature, which it has been attempted to introduce from various foreign writers, I have retained my former division of the elements of structural disease, designated by terms in common use among British pathologists." (p. 354, *note.*)

In his classification of the elements of structural disease, Dr. Williams includes under that term the morbid changes, "which take place independently of distinct inflammation, and which are mere modifications of the process of nutrition or reparation, which is continually going on in the textures of the living body." (p. 355.) In isolating these changes from the alterations of structure produced by inflammatory action, Dr. Williams is evidently influenced by his ideas with regard to the essential nature of the latter: for this, primarily consisting (as he thinks) in a disorder of the circulation, cannot be associated with processes whose essential character is a perversion of nutrition, such as no alteration in the simple rate of the blood's movement, or in the contractile state of the walls of the vessels, could of itself produce. But we cannot but think that it would be much more accordant with the mutual relations of these phenomena, as seen in the progress of disease, if they were all to be considered under one category; for it seems to us impossible to draw a distinct line between several of the elementary changes included in the foregoing group, and those which are always ranked, and which have been already described by Dr. Williams, under the head of inflammatory products. This indeed is virtually admitted by Dr. Williams himself; for when treating in detail of "Induration and softening, Deposits," &c., he refers to the fact that these have been already considered in part under the head of inflammation; and he only ranks them as a distinct group because they occur sometimes independently of inflammation;—although he remarks, in regard to the former processes, that "they probably, in most cases, depend on some of its elements;" and with respect to the latter, that they "result from an overflow of the nutritive material beyond what is necessary to nourish the textures themselves," which must be accounted as one of the conditions of inflammatory deposits. We cannot but believe that further consideration of the subject will lead him to see, that *all* structural diseases depending upon a perversion of the nutritive process must be arranged in one class, whether or not inflammation participate in the production of them. Thus in regard to the deposition of tubercular matter, we think it can scarcely now be doubted that it may take the place either of the ordinary material of textural nutrition, or of a fibrinous inflammatory effusion; and that whilst its production may be quite independent of inflammation, it is often the immediate effect of that process, and may be considered as one of its results. Our limits do not allow of our taking any lengthened notice of this excellent chapter; and we shall content ourselves with an extract relating to the gradual contraction of cacoplastic deposits, which frequently leads to the most serious consequences, without any ostensible indication for treatment.

"The microscopic examination of cacoplastic deposits at different periods gives much explanation of these contractile changes. When recent, it consists of fibres with a great amount of granular matter with and without cells, and more or less

amorphous material. When examined older, and after having contracted, it is much more tough, and is found to be more distinctly fibrous, much of the granular and amorphous matter having disappeared. Old contractile adhesions and cicatrices are still more dense, and are found to consist almost entirely of closely interwoven fibres, differing from those of fibro-cartilaginous tissue only in being less regular and distinct. It appears therefore that it is by the partial absorption of the granular amorphous portion of the deposit, and by the closer approximation of the fibres or more organized constituent, that the condensation takes place. Hence it is that the structure so contracted is less liable to the further degeneration to which cacoplastic deposits commonly tend. Certain it is, that of different portions of cacoplastic deposit in the lungs and elsewhere, those that are contracted remain unchanged, whilst others pass into the aplastic state of opaque and softened tubercle. This contractile process seems, therefore, to raise the deposit to a higher standard, in which, although below them, it is tolerated by the adjoining textures. But this very change may seriously injure the textures of organs, by contracting and compressing their vessels, and interfering with their nutrition and other functions; and in this respect it may be classed with the highest or most animalized variety of degeneration, the fibrous which has already been described. This is the chief mode in which the contractile diseases of the liver and kidneys gradually infringe on the circulation and secretion of these organs, and thus may eventually prove fatal. The contraction which takes place near the summits and roots of the lungs, in the more limited and therefore chronic forms of pulmonary tuberculous disease, often lays the foundation of emphysema of the lungs and habitual asthma. Similar contraction occurring in the deposit under mucous membranes of the alimentary and urinary passages constitute strictures, which occasion much distress and serious disorder." (pp. 389-90.)

Under the head of "Treatment of cacoplastic and aplastic deposits," Dr. Williams has introduced a statement of his opinion with regard to the remedial value of cod-liver oil; which is highly favorable. He considers that fixed oils in general have a solvent power on the fat of tubercle, which may enable them to remove the deposit, if carried into the circulation in sufficient amount; and that the superiority of fish-oils especially depends upon their being more permanently fluid, and less liable to become rancid. He lays great stress on the purity of the oil, as allowing a sufficient quantity to be ingested; and altogether dissents from the opinion of those, who think that the less pure forms of it are more efficacious. Where it can be borne in sufficient doses,—

"It increases rather than impairs the appetite; and, if continued for some weeks or even months, promotes in a marked degree the function of nutrition, increasing the strength as well as the flesh, and giving increment to all the textures. Nor is this surprising when we consider that the nuclei or rudimental molecules of all structures appear to consist of fat, which the oil in its highly divisible state supplies and renews in the manner most conducive to active and healthy nutrition. Its peculiar fluidity and little proneness to change also enable it to pervade all structures, and to penetrate even into imperfectly organized deposits, and by softening their concrete fatty molecules, and rendering more permeable and supple their whole mass, brings them more under the influence of the adjoining living parts, through the circulation in which either their vitality and nutrition are improved and maintained, or if incapable of improvement they are gradually dissolved and absorbed away.

"Such appears to me the mode of operation of the cod-liver oil, assuredly the most efficacious of all medicinal agents in the treatment of cacoplastic and aplastic deposits, and one which, after two years' constant experience in its use, is still frequently surprising me by the wonders that it occasionally works even in aggravated

and advanced cases of scrofula, mesenteric disease, pulmonary consumption, chronic pneumonia and pleurisy, and chronic rheumatism.

"But no remedy, however beneficial, should lead us to neglect attention to those general and hygienic measures by which the constitutional health is promoted and sustained; or, in other words, the great functions of circulation, respiration, digestion, absorption, and excretion are promoted." (p. 405.)

Dr. Williams has acted very judiciously, we think, in not devoting any large portion of his treatise to mere Nosology. That some kind of classification is needful, is obvious enough. That a *natural* classification, in which diseases, or collections of symptoms should be arranged according to the morbid states on which they are dependent, is the one most to be desiderated, will be admitted, we think, by every one who has any definite idea of the sciential part of medicine. But that we are still in the dark with regard to the essential nature or proximate cause of a large proportion of the morbid phenomena which we witness, is a truth which there is no gainsaying; and to attempt to establish a natural classification of diseases in the present state of pathological science, would be just as absurd as to propound a natural classification of a set of plants or animals, of whose internal structure we are entirely ignorant. The great use of a natural arrangement is the large amount of information which we gain respecting any object, when once we are enabled to refer it to its proper place; thus when by the study of its characters we have determined the natural family of a plant or animal, we thereby come to know much of its general structure, habits, properties, &c.; and in like manner, when the observation of symptoms or characters shall enable us to refer a disease to its place in the natural arrangement, we shall be immediately put in possession of the general laws of its progress and consequences, and of the principles which should guide our treatment. This is the case already to some extent with regard to certain well-marked groups, such as that now designated by the term zymotic diseases, in which, as is generally admitted, the disorder of the system is due to the introduction of a poison into the blood; the phlegmasiæ, in which a local inflammation is the essential change; and those which depend upon particular diatheses, such as the tubercular, the gouty, &c. The study of these truly natural groups is laying a good foundation for a really scientific nosology. But in the mean time, if we adopt any system at all, it must be one of a more or less artificial character; and we are inclined to think that Dr. Williams is correct in saying that a classification primarily founded on *locality*, although less pure and methodical than some others, is the most useful in practice.

The chapter on "Semeiology and Diagnosis" might, we think, have been extended with advantage. These are topics on which the student and young practitioner peculiarly need instruction, and which, not being systematically treated in any works save those on general pathology, should not be dismissed too summarily in a treatise like the present. It is always to be remembered that the mental operation by which medical knowledge is acquired from books or lectures, is entirely the reverse, in point of order, of that by which medical practice is to be guided. In the former case the student has before his mind a systematic array of diseases, and endeavours to store his memory with the series of symptoms which are described as respectively characteristic of each. In the latter, the symptoms are first presented to him; and it is his primary object to deter-

mine from them the nature of the disease. How far the most complete systematic knowledge is from sufficing for this purpose, and how much the sagacity derived from long observation of disease is superior to the most comprehensive erudition without the power of applying it to the case in point, must be obvious to every practitioner. Indeed it is the universal perception of this fact, which has given rise to the low estimate commonly entertained of the scientific character of medicine; for it is tacitly believed, if not openly avowed, by a large part of the profession, that although it is all very right for young men to read and study and attend lectures, yet that they obtain thereby little beyond a general preparation for learning that which alone can be practically useful to them. Now we go a great deal further than this; and are fully prepared to maintain that if a student have fully mastered the contents of such a work as that now before us, and have learned *pari passu* to apply this knowledge at the bedside, he will be far better qualified for the treatment of disease, than any one can be, who has devoted the same amount of time to clinical observation only. But it is only by knowing how to apply his knowledge, that the studies of the former can be of any avail to him; and for imparting this power, we deem a sound system of clinical instruction to be the most effectual method. This system should have for its especial object to conduct the student to the right interpretation of the phenomena presented to him, and to bring them into connexion with the scientific principles in which he has been instructed; in other words, to teach him the art of medicine on the basis of the sciences of pathology and therapeutics. Practical instruction in semeiology and diagnosis forms one very important part of this process; and we should have been glad, therefore, to find a more ample exposition of their principles in such a work as the present. The semeiological states of the pulse, the skin, the tongue, and of the alvine and urinary excretions, are all discussed within the space of two pages; and a large number of important indications are not noticed at all. "How to observe," and "what to observe," should be the mottoes of such a chapter; and a physician of Dr. Williams's large experience in practice and in teaching may do immense service to the rising generation of students by working upon these hints. One of the most essential questions of diagnosis, which seems to us to be progressively increasing in importance, either from a gradual change in the type of disease, or from an increase of discrimination on the part of the profession, or from both causes combined, arises from the very frequent complication, especially in the female sex, of real disorders of structure or function, by pain and other symptoms of great violence, which are immediately referable to the nervous system. We have known the time when all such symptoms were set down as inflammatory, and were met by bleeding, blistering, and the whole armamentum of antiphlogistic remedies. And we have often seen them treated, on the other hand, as if they were entirely "hysterical," with little better success. We believe that, in the class of cases to which we refer, and which the increased and increasing prevalence of an asthenic constitution seems to have rendered of more frequent occurrence than formerly among the male sex; the only true way is to put aside the nervous symptoms altogether in forming a diagnosis, and to endeavour to determine what kind and amount of disorder of structure or function may be diagnosed from other indications.

To the young practitioner such cases are often extremely perplexing; and the more so because he usually sees less of them in hospital practice, than he will do when his attention is directed towards the middling and higher classes.

We shall not dwell upon the portion of the next chapter which relates to Prognosis; and shall limit our remarks upon Dr. Williams's method of treating the *Modes of death* to a few particulars. We do not find any such modification in this edition, as sets it free from the objection raised against the arrangement adopted in the former one,—namely, that death from Coma and death from Paralysis are both to be attributed to Asphyxia; that state being engendered by want of power in the nervous centres to keep up the respiratory movements. Nor do we find any allusion to the proofs afforded by the experiments of Chossât,—that, in a great variety of exhausting diseases, death takes place from the *cooling* of the body as a consequence of the deficiency of combustible materials. We consider this to be a point of great practical importance; since it affords an obvious indication for treatment. Where there is reason to believe that there is no lesion inconsistent with the recovery of health, and where the prolongation of life for a certain time (until, for example, the poison of fever shall have been thrown off) is the great object, we can understand the benefit derivable from alcoholic liquors, which act not so much as stimulants, as by supplying material for the combustive process; and as an aid to these or as a substitute for them, we believe that artificial heat may be applied with great advantage. Dr. Williams has the merit of having formally directed attention to a cause of death, which had not been previously sufficiently distinguished from others; namely, *Necræmia*, or death beginning in the blood, such as is seen in its extreme form in the operation of the poison of serpents, or of the most malignant cholera or typhus poison, the direct influence of which, when introduced into the blood, is to produce a speedy alteration in its vital and physical properties, and thence to act through the whole system, producing a state of adynamia or prostration of all the powers alike, which soon ends in death.

"But if the noxious influence is in smaller quantity or more diluted, the vital powers react against it in various ways, the object of which can often be plainly discerned to be its expulsion from the system. The shivering, hot stage, and sweating termination, of paroxysms of intermittent fever; the similar but less marked series of febrile movements which occur in slight forms of remittent and continued fevers; the profuse and violent fluxes from the stomach and intestines in cholera, dysentery, and epidemic diarrhœa, and the similar discharges induced by poisonous ingesta, are instances of the operation of the vital reaction attempting the expulsion of the noxious matter and of that part of the animal fluids that had been corrupted by it. But these struggles in many instances constitute serious diseases, in which life may be compromised by the violent and exhausting effect of the reaction, as much as by the prostrating influence of the cause of the disease: in these more complex affections, individual organs may especially suffer in different cases, and the danger and the cause of death may be less in the changed condition of the blood, than in the affections of particular organs, or the exhaustion consequent upon them, which destroy, not by necræmia, but by coma, asphyxia, or asthenia, modes of death already considered.

"The injurious effect of these poisons may be still more completely prevented when their quantity is small and the living powers are vigorous. A diarrhœa, a profuse sweat, or a free flow of urine, sometimes carries off the commencing disease.

The intestines, the skin, and the kidneys, appear to be the proper emunctories, through which the morbid matter is expelled. The peculiar fætor of the secretions from the bowels in typhoid fever, the beneficial influence of moderate diarrhœa, which removes them in the early stage of fever, and appearance of a foul fibrinous matter (typhus-material of German writers) in the intestinal glands, seem to be examples of the elimination of a morbific matter; and I have before suggested that the follicular inflammation, ulceration, and sloughing of the intestines in fever may arise from the excessive irritation of the follicles in the exercise of this eliminating function. Again, with regard to the kidneys, it has been before mentioned that granular degeneration, which impairs their function and power of elimination, renders the body peculiarly liable to contract epidemic and infectious diseases, and to succumb under them. This renders the prognosis unusually unfavorable in these cases. The same remark extends, and for the same reasons, to persons who have been habitually intemperate. On the other hand, those whose kidneys are naturally active, more effectually resist disease, and more readily throw off its effects. In the like manner, it is well known that persons with a naturally dry skin do not so readily get rid of a fever as those in whom perspiration is readily excited." (p. 467.)

These views we regard as of great importance. Like others already quoted, they show how much may be done, by a sagacious collocation of phenomena, towards establishing the *rationale* of their mutual dependence; and how beneficially that knowledge must act in practice, if only in giving warning as to what ought *not* to be done, it is surely unnecessary for us to point out.

To the present edition has been added a chapter on Prophylaxis and Hygienics; into which Dr. Williams has managed to compress as much sound and concise information on these topics, as a limited space would allow. As we do not find in it, however, anything specially worthy of comment or criticism, we shall not further occupy the attention of our readers; but shall content ourselves with again recommending this treatise as the best exposition in our language, or we believe in any language, of *Rational Medicine*, in its present improved and rapidly improving state. We remember hearing Professor Retzius remark, during his late visit to this country, that he considered the best English medical writers to stand pre-eminent in the art of teaching; combining simplicity and definiteness of expression, and condensation of material, with sound philosophical habits of thought; and being free from the grandiloquent wordiness of the French school, and from the vague mystical abstractions and the overloading of details which characterise the German. And we do not know that we could anywhere point to a specimen more deserving of this commendation, than the treatise before us.

We must say a few words in conclusion respecting the other work before us,—the General Pathology of Professor Chomel; which is, as we need scarcely observe, one that well deserves the attention of the student of Pathology, although by no means so well adapted to give a general view of the present aspect of the science, as is the treatise on which we have been so fully dwelling. Its especial excellence consists in the fulness and clearness of its chapters on Semeiology and Diagnosis, the subjects which we regard as least perfectly treated by Dr. Williams. And the well known practical skill of its distinguished author in the detection of obscure diseases, gives to these chapters a far higher value than they could have

possessed as the productions of a mere theorist, however ingenious. Hence they may be advantageously perused as a supplement to Dr. Williams's 'Principles,' and as an introduction to clinical study. It is to be regretted that the work should have fallen in many parts so much behind the present state of science. The last French edition, from which this translation is made, bears the date of 1840; and those who can look back to the uncertain state of opinion prevalent at that period on several of the most important questions of pathology which may now be regarded as satisfactorily settled, will know that considerable modification would be necessary to place it *au courant* with the present state of the science. This the translators have not attempted; and we consider that they have acted wisely in their forbearance. Their translation reads well, and appears to have been carefully executed. A few notes have been appended: but we think that these might have been almost as well omitted, as they cannot be said to do much towards giving consistency and completeness to the work.

ART. VIII.

An Account of the Cultivation and Manufacture of Tea in China; derived from Personal Observation during an Official Residence in that Country, from 1804 *to* 1826, *&c.* By SAMUEL BALL, Esq., late Inspector of Teas to the Hon. United East India Company in China. With Three Plates, and numerous Wood-Engravings.—*London*, 1848. 8vo, pp. 382.

OUR readers may at first, perhaps, be surprised at our reviewing a work written expressly on the Cultivation and Manufacture of Tea in China; notwithstanding that it forms an article of diet of a large proportion of the British population, and is esteemed as a medicine in many parts of the Continent, as well as in other parts of the world. But there are many points connected with the manufacture of tea, in which all are interested; and there is no question of the kind which is more frequently asked of medical men, than the cause or causes of the difference in colour and properties between black and green tea. All are familiar with some of the statements made on this subject, as for instance, that the green colour was due to the tea having been manufactured in copper vessels; also, that the colour was due entirely to the employment of colouring ingredients, such as turmeric, indigo, prussian blue, with gypsum, kaolin, &c. Mr. Warington has shown that what is called the fine bloom of many green teas is ascribable to the employment of such materials; though it does not follow that the colour of the best green tea is entirely due to this cause. The most reasonable explanation seemed to be that, which ascribed the difference to green and black teas having been produced by two different plants. This explanation also, has been proved to be incorrect, first by Mr. Fortune, and now by the author of the present work.

It must not, however, be inferred from this statement, that the real facts of the case had never previously been made known; but in this, as in many other cases, truth had been so mixed up with fable, or rather the accounts of the manufacture of the adulterated had been so confused with those of the genuine article, that it became extremely difficult to

ascertain the true state of the case; especially as no Europeans, for many years, have had access to the tea districts of China, which are distant about 800 miles from Canton. Mr. Ball, from his official duties for a long period at this threshold of China, necessarily became well acquainted with the several varieties of tea, which are, or then were, known in commerce; but he was not satisfied with the single performance of his duty, but employed his leisure in making careful inquiries of the different tea-makers who visited Canton, respecting the processes of manufacture, and the causes of the difference in appearance of their several teas. He also induced them to make their teas in his presence, with the leaves of the tea plants procurable in Honan, and himself went through the several processes. The results he has detailed to us in the present work, which we can confidently state is written with great care, and a cautious spirit of induction. With the details of culture, gathering, and manufacture, we have an account of the districts where tea is chiefly cultivated; with their climate, a notice of the botanical characters of the plant, its chemical analysis, and the causes of the change of colour in teas during the process of manufacture; and he concludes with a notice of the experiments now going on in the Himalayan Mountains for the cultivation of tea.

Though Mr. Ball does not treat of the plant or plants with which the different kinds of tea are manufactured, until his twelfth chapter, we think it desirable to have the question regarding the particular plants settled, before we proceed to the processes of manufacture. This, however, is not the best part of the work under notice; for the author mixes up the question of the identity or difference, as species, of the plants known as tea plants, with that of whether the different kinds of tea are made from one or more species. He is no doubt right in his account of the latter point; but appears as much wrong, according to the views of more competent judges, in considering the tea plants known here to be mere varieties, and not distinct species, of the genus Thea.

There are two well-known plants in our botanic gardens and nurseries; the one called *Thea viridis*, having been supposed to be the plant employed in making green tea, the other called *Thea bohea*, having in like manner been considered the black-tea plant. These two plants are well figured by Dr. Lettsom, in his 'Natural History of the Tea Tree,' 1799; *Thea viridis* forming the frontispiece, and *Thea bohea* being figured at p. 41. The latter, being a plant of the south of China, is naturally found to be more tender; while the *Thea viridis*, from the northern districts of China, is sufficiently hardy to live in the open air of this country, with very little protection. As the green teas of commerce are obtained from the northern tea districts, it was naturally inferred that they must be yielded by the green-tea plant, and the black teas of the Fokien district by the black-tea plant; more especially as some persons, well qualified from their position, and from apparently careful inquiries, had stated that the genuine black and green teas of commerce were not, and could not be, prepared from the same plant. Others, however, equally well acquainted with China, have, from the time of the Jesuits until the present day, as positively stated, that both kinds of tea could be and were prepared from the same plant, and that the difference depended entirely on the process of manufacture. Though no competent observer has penetrated to the best tea districts,

and perhaps no genuine specimens of the tea plants from thence have been examined by any qualified botanist, there is now very little doubt of this being the correct state of the case; though it is quite possible, nay probable, that the Chinese may prefer varieties of the same plant, in particular soils and situations, for the preparation of particular varieties of both black and green teas.

Mr. Fortune, who was deputed by the Floricultural Society of London to investigate the flora of the accessible parts of China, for the purpose of introducing as many of its ornamental treasures as were suited to the climate of this country, has, in his entertaining 'Wanderings in China,' given us an account of what he saw of the manufacture of both black and green tea at the different parts of the coast which he visited, that is, from Canton to Shanghae; and he concludes "that the black and green teas of the northern districts of China (those districts in which the greater part of the teas for the foreign markets are made) are both produced from the same variety, and that that variety is the *Thea viridis*, or what is commonly called the green-tea plant. On the other hand, those black and green teas which are manufactured in considerable quantities in the vicinity of Canton are obtained from the *Thea bohea*, or black tea." (p. 198.)

We shall now take a glance at the manufacture of black tea, as described by Mr. Ball; though we can hardly hope to do justice to his detailed descriptions, which, however, will no doubt prove extremely valuable to those engaged in the manufacture of tea, in different parts of the world. The author, after giving an account of the gathering of the leaves, details the processes of manipulation previous to roasting. The leaves are first exposed to the air; sometimes to the sun, though this is a practice not always approved of. During this they "wither and give," or "become soft and flaccid." Leaves gathered after rains, or in cloudy weather, require exposure to the sun, or to be dried before or over a fire, previous to their being roasted.

The processes previously to roasting, consist of Leang Ching, To Ching, and Oe Ching. 1. Leang Ching is literally that of cooling the leaves, or keeping the leaves cool to prevent or check fermentation. They are placed either in shady situations exposed to the wind, or in open buildings which admit a draught through them. They are kept in this state until they begin to emit a slight degree of fragrance, when they are sifted, preparatory to the next process (2), To Ching, which signifies the tossing about the leaves with the hands in large trays. 3. Oe Ching. This consists in collecting the leaves of each sieve into a heap, and covering them with a cloth. They are then watched with the utmost care, until they "become spotted and tinged with red," when "they also increase in fragrance, and must be instantly roasted, or the tea would be injured."

"Thus," Mr. Ball observes, "the manipulation previously to roasting seems to be for the purpose of evaporating as much of the fluids as possible, without injury to the odorous principle or aroma; or rather, perhaps, to induce a slight degree of incipient fermentation, or analogous change, which partakes of the saccharine fermentation of hay, during which the requisite degree of fragrance is evolved." (p. 115.)

"The roasting and drying of the teas may be divided into the two processes of Chao or Tsao and Poey. The former takes place in a shallow iron vessel called a Kuo; and the latter in sieves over a charcoal fire.

"In the first roasting of all black tea, the fire is prepared with dry wood, and kept exceedingly brisk. The vessel is heated to a high temperature, much above the boiling point; but any heat may suffice which produces the crackling of the leaves described by Kæmpfer." (p. 122.)

"The roaster stands on the side of the stove opposite the fire-place, and taking about half a pound of leaves between his hands, he throws them into the Kuo. He then places his hands upon the leaves, and with a slight degree of pressure, draws them from the opposite side of the vessel across the bottom, to the side nearest himself. He then turns them over, and throws them back again, repeating this action until the leaves are sufficiently roasted.

"When the heat becomes excessive, and difficult to bear, the roaster then raises the leaves some height above the Kuo, and shaking them in his hands, he lets them gradually fall, which serves to dissipate the steam, and to cool them." (p. 122.)

With respect to the degree of roasting, Mr. Ball states, that the roasting must be continued until the leaves give out a fragrant smell, and become quite soft and flaccid, when they are in a fit state to be rolled. This rolling is described as follows:

"After the first roasting (Chao), the leaves are immediately rolled. Each roller is provided with a circular tray of bamboo-work, upon which he places as many leaves as the two hands held together in a concave position can cover. They then all fall to work, immediately rolling the leaves round from left to right, using a slight degree of pressure, and attentively keeping them in the form of a ball—when sufficiently rolled, the ball is shaken to pieces. The leaves are then found twisted; and the viscous juices expressed in the process of rolling are sufficient to keep the leaves in the twisted form. They are now spread out on clean trays, and placed on stands several tiers in height, until the whole of the fresh leaves have been roasted, when they again undergo the process of Chao. In the second roasting of all tea, the heat of the fire is considerably diminished, and charcoal used instead of wood. The leaves having now been deprived of a considerable quantity of moisture, their bulk is consequently much diminished." (pp. 125-27.)

The roasting and rolling are often repeated a third, and sometimes, with large and fleshy leaves, even a fourth time; and it is only when juices can no longer be freely expressed in the process of rolling, that the leaves are considered to be in a fit state to undergo the final desiccation, which is denominated Poey.

This process is considered by the Chinese a very important part of the manipulation of black tea, in consequence of the care required to prevent any of the tea leaves falling into the fire and producing smoke, which would injure the tea. It is performed in a tubular piece of basket-work, open at both ends, covered with paper, about two and a half feet in height, and one and a half in diameter, but narrowest in the centre; a little above which are placed two cross wires, for the purpose of receiving the sieve which contains the tea, and which is placed about fourteen inches above the fire. This process is employed for every kind of black tea, manufactured with care, whether Pao Chong, Souchong, Sonchy (Caper), Pekoe, or Congou.

A bright charcoal fire is prepared in a common chafing-dish; the drying tube is then placed on the fire, one end resting on the ground. The roaster then spreads some properly-sifted leaves equally on a sieve, which is then placed on the cross wires within the drying tube, and this is nearly covered at top with a flat bamboo tray. This is left about one third open, to admit of a free evaporation of the steam which arises from the leaves; for a considerable degree of moisture still remains, though no more can be expressed by the process of rolling. The leaves still retain

their green and vegetable colour, and are suffered to remain about half an hour, when the sieve being taken out, the leaves are turned, and again placed over the fire as before; at the expiration of about half an hour, the leaves are again taken out of the drying tube, and rubbed and twisted between the hands.

"A great change has now taken place in the colour of the leaves. They have already begun to assume their black appearance. A considerable quantity of moisture having also been dissipated by this mode of drying, the fire is now covered with the ash of charcoal or burnt paddy husk, which not only serves to moderate its heat, but prevents smoke, in the event of any leaves falling accidentally through the sieves.

"The leaves are then twisted, and again undergo the process of drying, twisting, and turning as before; which is repeated once or twice more, until they become quite black, well twisted, and perfectly dry and crisp." (p. 136.)

"When the leaves appear sufficiently dry, which is ascertained by their crispness, they are then taken from the fire, and sifted; and the old, the yellow, and the chaffy leaves are winnowed off by means of a large circular bamboo tray." (p. 137.)

Equal care is bestowed on the packing of the tea, which has thus been so carefully prepared. Mr. Ball then gives an account of the varieties in manufacture required to produce the different black teas.

We shall now proceed to give Mr. Ball's description of the mode of preparing the green teas of commerce, which are produced in the southern parts of the province of Kiang Nan, in the district of Whey-chew-fu. These teas Mr. Ball classes under two heads, "Hyson and Singlo; all other kinds are made from those shrubs, and there is much reason to think that even the Hyson is merely the Singlo shrub improved by soil and cultivation." (p. 207.) After giving his reasons for this opinion, he proceeds to treat of the manipulation of green tea.

"There are only two gatherings of the leaves of green tea; the first begins between the 20th of April and the 5th of May, and lasts for about ten or fifteen days; and the second at the summer solstice. The green-tea factors universally agree that the sooner the leaves of green tea are roasted after gathering the better; and that all exposure to the air is unnecessary, and to the sun injurious. But as all the leaves cannot be roasted at the same moment, means are necessarily resorted to in order to prevent their heating or turning yellow. They are, therefore, thinly strewed on tiled floors, or placed in flat bamboo trays on stands, in shady places exposed to the air." (p. 213.)

The iron vessel in which the green tea is roasted is called a Kuo; this is thin, about sixteen inches in diameter, and about ten inches in depth, set horizontally (that for Twankay obliquely), in a stove of brick-work, about five inches below the surface, making the depth altogether about fifteen inches. Mr. Ball then describes the manufacture as he saw it performed at Canton, with leaves of the Honan plant (*Thea bohea*), by men from the green-tea districts.

"The fire was prepared with dry wood, and kept so very brisk that the flames rose above the surface of the stove. The bottom of the Kuo soon became red-hot, and the heat intolerable. This excessive heat, however, was not approved by the conductor of the process. It must, therefore, be considered as an example where the roaster wished to display his skill, rather than as one to be imitated. The steam also arising from the leaves was so considerable, that I could scarcely bear my hand over it for an instant."

About half a pound of leaves were put into the Kuo at one time, when they produced the crackling noise described by Kæmpfer, and were quickly stirred about, first with one hand, and then with the other, the roaster being frequently obliged to change the hand, on account of the excessive heat. For the same reason, almost at the end of every turn, he raised the leaves about six inches above the surface of the stove, shaking them on the palm of the hand to separate them, and to disperse the steam. He then gave them two or three brisk turns round the Kuo, when suddenly collecting them together in a heap, he passed them to another man, who stood in readiness with a basket to receive them.

"The process of rolling is much the same as that employed in the rolling of black tea, the leaves taking the form of a ball. After the ball was shaken to pieces, these people twisted the leaves again between the palms of the hands, so that the leaves might be twisted regularly, and in the same direction. They were then spread out in sieves, and placed on stands in a cool room." (p. 217.)

"Previously to the leaves being carried a second time to the Kuo, the fire was considerably diminished, and charcoal used instead of wood. The leaves were also constantly fanned by a boy who stood near." (p. 219.)

"In this second roasting, the roaster began by slightly pressing the leaves with the palms of both hands against the bottom of the Kuo, then drawing them towards himself to the top of the stove, he shook them in his hands to cool them. When the leaves have lost so much of their aqueous and viscous qualities as to produce no sensible steam, they no longer retain any disposition to adhere together; but, on the contrary, having acquired a twisted form in the process of rolling, the simple action of the fire naturally disposes them to separate, twist, and curl of themselves. When taken from the Kuo, they appeared of a dark olive colour, almost black. After being sifted, they were placed on stands as before, until the whole of the leaves had undergone the second roasting, when they were roasted a third time." (p. 220.)

"In the third roasting, which, in fact, is the final drying, the heat of the fire was again diminished, and reduced to that degree which the hand can bear for some seconds without much inconvenience. The fanning and the mode of roasting were the same as in the final part of the second roasting. It was now curious to observe the change of colour which gradually took place in the leaves, for it was in this roasting that they began to assume that bluish tint, resembling the bloom on fruit, which distinguishes this tea, and renders its appearance so agreeable." (p. 220.)

On this Mr. Ball remarks, "Thus it is obvious, that the peculiar colour of green tea does not properly arise from the admixture of colouring matter with the leaves, but naturally out of the process of manipulation." He further observes, that "as the close twist and curl of the leaf is one test of superior quality, so also is brightness of colour," and that "the factitious colouring of green teas has originated with the Chinese from the desire to give a spurious superiority to inferior teas, while foreigners have been easily duped by these deceptions."

The green tea which has been thus prepared, is usually packed in chests in the tea country, and called Mao Cha, because it has not been sorted into the different kinds. Ching Cha is Hyson tea, that is, after the Hyson-skin, young Hyson, and gunpowder have been separated from it. We are told by a Chinese authority, that "when the factors have concluded their purchases, they carry their tea home, where it is sifted, winnowed, and assorted into different kinds suited to the foreign markets." (p. 223.) It is thus by sifting, winnowing, and fanning, with further roasting, that the several varieties are prepared. The author then details the manufacture of Twankay teas.

With regard to the production of colour, Mr. Ball made some interesting experiments, by which it appears that leaves, while undergoing the third roasting in the same vessel, but kept separate by a thin partition of wood, became of a black or of a green colour, according as they were kept in a quiescent state or in constant motion. "The leaves kept in constant motion dried rapidly, and soon assumed the colour and appearance of green tea. The other parcel (kept in as quiescent a state as possible) required a much longer drying, and, when completed, assumed the colour of black tea." (p. 242.)

We cannot afford space for the author's interesting chapter on the degree of heat which is required for the proper manufacture of tea. But we may observe, that this seems to depend a good deal upon the succulency of the leaves. We will now proceed to his chapter on the analysis of tea, and to his explanation of the causes which produce the great difference between black and green tea. Mr. Ball takes the analysis by Mulder as the basis of his observations, objecting, in the first instance, to the statement of that able chemist, that black tea is roasted and dried at a higher temperature than green tea, and that this is the cause of its darkness and redness of colour in leaf and infusion. The author shows, that "so far as the teas of commerce are concerned, there can be no doubt that green tea is manufactured at a higher temperature than black." (p. 276.)

Mulder, in his analysis, states that the principal constituents of tea consist of volatile oil, tannin, gum, and extractive matter, all which are found to exist in a larger amount in green tea than in black; but that in black tea, apothem obtained from extractive by manipulation and high temperature, gives the colouring matter, and red or brown infusion to black tea. Mr. Ball observes that "the red or brown colouring matter of black tea, and the red infusion, have been shown to arise from a particular treatment of the leaves by exposure to sun and air, like hay, previously to roasting; and that corresponding results have been produced by exposure to sun, without the action of fire."

With respect to the essential oil of tea, Mr. Ball observes, that the fresh unmanipulated tea has no fragrance, and adduces some experiments made by Nees von Esenbeck on newly-gathered leaves, in the Botanic Garden at Bonn, and his opinion that the aroma of tea is analogous to that of coffee, requiring a high temperature to develope it. The author himself states, that there is no doubt that the aroma of green tea is developed entirely by artificial heat, and inquires whether the odour of black tea be not generated, "or, at all events modified, first, by the action of sun and air, and finally, by artificial heat." (p. 280.) He quotes the opinion of Liebig, that "the odorous principle of many vegetable substances is newly formed during fermentation of the saccharine juices of the plant," and adduces the preparation of tobacco, and the formation of nicotine, as well as the development of odour in roasting coffee, as analogous cases.

Mulder ascribes more importance to the astringent principle than even to the volatile oil; and considers that the gum plays an important part in softening and cloaking this principle, so that those teas have the best flavour which have a tolerably large quantity of tannin, with enough of gum to moderate the astringency on the tongue.

Green tea is well known to be more astringent than black. Mulder's analysis shows, that while Hyson yielded 17·80, Congou contained only

12·88 per cent. of tannin; and he considers that part is converted into apothem, hence black tea is less astringent than green. But Mr. Ball observes that this hardly accounts satisfactorily for the great deficiency of tannin in black tea. "The quantity of apothem is only 1·48, whereas the loss of tannin is about 30 per cent. of the whole quantity." He ascribes it, with great show of reason, to the process which black tea undergoes previous to roasting, during which the leaves are treated as hay. This process, which has been aptly termed "withering, is, in fact, incipient change, fermentation, or *eremacausis*," during which oxygen gas is absorbed, as during the decay of other organic substances, when the elements enter into new combinations. He adduces the experiments of Liebig, in which the moistened leaves of different trees placed in oxygen, were found to absorb that gas, as they change colour. "The diminution of the gas which occurs can only be owing to the union of a larger proportion of oxygen with those substances which are already in the state of oxides, or to the oxidation of the hydrogen in those vegetable compounds which contain it in excess. The fallen brown or yellow leaves of the oak contain no longer tannin, and those of the poplar no balsamic constituents." (Liebig, Chem. of Agric.)

Mr. Ball applies this to the changes which take place during the manufacture of black tea. Newly-gathered leaves, exposed to sun and air, soon begin to suffer change; and all organic substances, during this state of change, absorb oxygen from the atmosphere. "The green resinous principle of the leaf disappears;" and, in leaves containing tannin, as tea leaves, "red or brown colouring matters are formed; the tannic acid disappears, and is replaced by sugar." Thus is the loss of tannin accounted for; and the red colouring matter of the leaf, as well as the red infusion, explained.

We cannot proceed further with the analysis of tea, but we recommend the subject to chemists as an interesting one for investigation; especially the production of aroma, dependent on volatile oil, in the process of roasting, as chiefly seen in the green tea; and the disappearance of tannin in the process of "withering," and the consequent change of properties in black tea.

Mr. Ball concludes his able and valuable work with a notice of the experiments now going on in producing tea in the Himalayan Mountains. These have been chiefly, if not entirely, proposed and carried on by medical officers of the East India Company's service. Dr. Royle first proposed the culture in the year 1827, and again in 1834; Dr. Wallich in 1832; Dr. Falconer in 1834, and began the culture in 1836; Dr. Jameson, who succeeded the latter in 1842 in the charge of the Botanic Gardens at Saharunpore, has been carrying on the culture and manufacture with an energy which promises to make the undertaking one of national importance. Excellent specimens of both green and black tea have been received from Kamaon, and the cultivation is being greatly extended at the recommendation of Lord Hardinge.

ART. IX.

1. *Copy of Dr. King's Report on the Fever of Boà Vista.* Ordered by the House of Commons to be printed, 10th March, 1848.—pp. 16.

2. *Dr. M'William's Remarks on Dr. King's Report on the Fever at Boà Vista.—London,* 1848. pp. 15.

SINCE the publication of our last Number, two additional documents on the fever of Boà Vista have reached us; and in order to carry out the history of this important controversy, we shall place their contents as briefly as we can before our readers.

We may, in the first place, remark that in the analysis of Dr. M'William's Report, contained in our first Number, and in the discussion on the contagion of yellow fever which we founded upon it, we were actuated, solely and entirely, by a desire to elicit and to enunciate the truth, on whichever side we might have discovered it. We believe that we entered upon the discussion in the true spirit of criticism, and with a firm determination to be actuated by no unfair feeling of partisanship. And in the same spirit we do not hesitate now to affirm, that had we found anything in Dr. King's Report calculated to shake our faith in the conclusions we had arrived at in the absence of this Report, then we would at once, and unhesitatingly, have acknowledged our errors and have retracted our previous erroneous belief. But we owe it to ourselves to declare that Dr. King's Report has strengthened, not weakened, our arguments; that it has thrown light on some points that yet needed it, and has enabled us to fill up in stronger colours some portions of the picture which yet remained obscure.

It appears that after Dr. M'William's return to England, in September, 1846, the yellow fever reappeared at Boà Vista. Dr. M'William immediately offered his services to resume the investigation; but Sir William Burnett resolved, and it appears to us with great propriety, to select another officer, in order that the ground might be retrodden, and the previous investigations confirmed or refuted by additional examination. Dr. King, inspector of hospitals, a gentleman known for his ability, and his previous acquaintance with yellow fever, was the individual chosen; he landed at Boà Vista on the 23d of December, 1846, and pursued the same course of investigation which had been adopted by Dr. M'William ten months previously.

It is necessary to state here, that Dr. King's opinions respecting the contagion of yellow fever, seem to have been fully formed before he landed at Boà Vista. He was, and is, a decided non-contagionist,—and believes that he has succeeded in throwing doubts upon the fact of the importation of yellow fever into Boà Vista by the crew of the "Eclair." In this belief we do not agree, and we shall now proceed to examine and answer his arguments, in so far as these arguments have not already been anticipated, and answered by anticipation, in our previous analysis of Dr. M'William's Report.

On arriving at Boà Vista, Dr. King's first care was to ascertain the nature of the fever which had prevailed in the island, and of which a slight renewed outbreak had just occurred, and the identity, or otherwise,

with the fever of the Eclair. The cases of fever were not very numerous, but Dr. King saw enough to convince him they were genuine cases of yellow fever.

"From the general symptoms," he writes, "in the cases I did see, from the admission of Senor Laoa, that the patients I visited at Joao Gallego were the subjects of yellow fever, and from the testimony of all I conversed with, or examined respecting the symptoms which characterised the disease throughout its course, I am decidedly of opinion that the diseases were identical."*

Secondly, Dr. King states that, in both the upper and lower classes of society, it was universally affirmed that fever did not prevail at Boà Vista, previous to the arrival of the Eclair in August 1845, and that it did not prevail, at this or at any subsequent time, in the other islands of the Cape de Verde.† The previous healthiness of Boà Vista is a very necessary link in the argument. In our first Number, we brought evidence to prove that, for certainly 35 or 50 years, no disease like yellow fever had been prevalent there. It has been thought very extraordinary that Boà Vista, situated not only in the geographical limits of the yellow fever, but in close neighbourhood with islands such as St. Jago, where yellow fever is known to be common, should not at times have been subject to the disease. But this exemption is a well-known fact in the case of other islands and districts equally in the yellow-fever district, and adjacent to other countries which suffer more frequently and more severely than they do; and even if this were not the case, still we have it in evidence, and however it may disagree with our previous notions, we must accept the evidence, that Boà Vista had been a remarkably healthy place before the arrival of the Eclair.

The following letter from Dr. Almeida is published by Dr. M'William.‡ It is important, as being written by the man best qualified by the nature of his profession, and by his long residence on the island, to give an opinion as to its previous healthiness. It is also an answer to an erroneous statement of Dr. King's, that Dr. Almeida did not believe the fever to have been imported by the Eclair. The letter is addressed to Mr. George Miller, a merchant at San Nicolao; a gentleman who, we are informed by Dr. M'William, has taken great trouble to investigate the early steps of the disease.

"Boà Vista, June 30, 1847.

"MR. GEORGE MILLER—I have before me your esteemed letter, in which you request from me my professional information, as a resident in this island since the year 1809, upon the sanitary state of it, from that time until the appearance of the yellow fever in 1845. In reply to which, I have to inform you, that I have resided in this island thirty-eight years, as the only medical man of the place. I have treated the inhabitants of this island of all and whatever sicknesses with which they have been attacked, and it has never come to my knowledge that contagious fevers have been introduced into this island, nor have originated in it; but only fevers 'miasmaticas,'§ exhibited with intermittent and remittent symptoms, which

* Report, p. 2. † Ibid., p. 3. ‡ Remarks, &c., p. 14.

§ With regard to the nature of these "miasmatic fevers," the Portuguese Governor-General, in a letter to Dr. King, inserted in Dr. M'William's Remarks (p. 10), says: "The fevers that are indigenous or have a local origin, are called by the doctors at Villa da Praga (San Jago), 'miasmaticas;' and, in the other islands, the gastro-enteritis; and these are of a very mild character, but unfortunately they make their appearance every year in the rainy season." The Governor, in the same letter, says: "Never a fever with equal symptoms visited these islands, before the arrival of the unhappy Eclair at Boà Vista."

only took place some years in the rainy season, from the effects of stagnant water in the ravine of Rabil,* and attacked the people living in the neighbourhood thereof, while those in the other villages were exempt. It was only in the year 1845 that I have known in the island the contagious or yellow fever, which unhappily was introduced by H.B.M. steam-vessel Eclair, and to such an extent devastated this unfortunate people.

(Signed) "H. J. Xavier de Almeida, *Licensiado.*"

These two points are then admitted by Dr. King:—1. The fever prevalent at Boà Vista, after the visit of the Eclair, was identical with that raging on board the vessel. 2. The island had not been subject, as well as we can judge from tolerably good evidence, to attacks of yellow fever for a long series of years.

Was, then, the outbreak of this fever and the arrival of the Eclair a mere coincidence—one of those remarkable accidents, which occasionally baffle our reason and cheat our senses, by an almost incredible concurrence of events?

We must here refer our readers to our first Number, in order to avoid repetition, and request them to reperuse the successive steps of evidence by which we traced the extension of the disease from the people of the Eclair to Porto Sal Rey and Rabil. Now on most of these points Dr. King is completely in accordance with us, as far as matters of fact go—he differs only as to the inferences. Thus he states—"Of the three soldiers who constituted the guard at the fort when the Eclair's people left the island, two, a corporal and a private, were attacked with a fever resembling that which prevailed on board the ship, and these men died respectively on the 21st and 22d of September, after an illness of five or six days." (Op. cit., p. 6.) We gave the dates as the 20th and 21st, an immaterial difference. Dr. King also states, that one of the surviving privates of this guard being attacked with slight fever some days after this, was removed with his comrade to a small hut in Pao de Varella. Mr. Consul Rendall speaks of these two men as being both ill when taken to the house at Pao de Varella; but if it be the case that only one was ill, the argument is still sufficiently supported. Dr. King also admits that the first person who died in Porto Sal Rey was Anna Gallinha, and he states that she was attacked on the 12th, and died on the 16th of October, the dates we formerly gave on Dr. M'William's authority. The illness of this woman we attributed to intercourse with the sick soldiers. Now, it is at this point that Dr. King wishes to break the chain of evidence. He apparently wishes to deny that Anna Gallinha held any intercourse with the sick soldiers or soldier. "If," he says, "the soldiers were visited by Anna Gallinha, and others, what was the object of their seclusion, and how is the reckless conduct of their visitors to be reconciled with that dread of the fever which it is said pervaded all classes? Their washing and cooking are mentioned as an excuse, but I fear there was but little, if any occasion, for either." In opposition to this rather lax mode of argument we have the positive evidence of the soldiers themselves,† the evidence of Joanna Texeira,‡ of John Jamieson, and many others, that Anna Gallinha,

* The neighbourhood of this ravine at Rabil was, as we formerly remarked, the place where the epidemic fever prevailed with least severity.

† In Dr. M'William's Report, p. 23, Barbosa is asked: "Were you much visited in this house? Yes; Anna Gallinha and Joanna Texeira were usually in the house." At p. 24, the other soldier, Manoel, is asked the same question; he replies, "Yes; by a great many, particularly by Anna Gallinha, who cooked for us.

‡ Ibid., p. 28.

whose own room was only separated by a thin partition from the soldiers, was frequently in the same room with them, nursed them, and cooked for them. Her services at Beira too, Dr. M'William states, were not confined to these innocent duties.*

But supposing we surrender this point, and also all the corroborative circumstances which occurred subsequently to this woman's death, and which prove that the people who had been chiefly in contact with her were the next sufferers, allowing Dr. King, for a moment, the benefit of this alleged break in the evidence, he has thereby got into a difficulty, of which he has offered no explanation. Dr. King has already admitted that two soldiers at the fort had died of a fever identical with the Eclair fever. To account for this, he has pleaded that these two men were Europeans, and were but lately arrived in the colony,† and were "therefore pre-eminently exposed to attacks of endemic fever."‡ But this explanation is not admissible. 1. Because it has been already proved by very respectable evidence, that yellow fever, during half a century previously, had not been seen as an endemic, even at Boà Vista; and it may be concluded that if new-comers had, in previous years, been subject to an endemic fever of such severity as to kill in three or four days, this could hardly have escaped the notice of Dr. Almeida and the other European residents on the island, who all testified that such a thing was unknown to them. 2. Because the third case, that of Barbosa, occurred in a negro, and therefore in a non-susceptible person, according to ordinary doctrines. Now Dr. King, although he admits fully,§ that this man laboured under the same fever as the Europeans, only in a less intense degree, gives no explanation of the origin of his illness. To explain the illness of Anna Gallinha, Dr. King, after rejecting the agency of the sick soldier, calls in the aid of epidemic in addition to endemic causes. He states that||—

> "There is one large pool of stagnant salt and fresh water immediately behind but to windward of this part of the town (Pao de Varella), and still nearer to the houses there is a locality which is resorted to by many of the people when obeying the calls of nature; and the exhalations from the one, and the effluvia from the other are blown by the usual winds in the direction of Beira. Should it be objected that the operation of the same causes in former years did not produce the same deplorable results, I need only reply that the season of 1845 at Boà Vista was most unusual, and in many respects very remarkable."¶

Dr. King then alludes to the statements of Mr. Macaulay and of Mr. Consul Rendall,** as to the heavy rain in the early part of October;†† and concludes that "at the end of September, or beginning of October, the atmosphere had become vitiated and malarious."

Therefore, to account for the death of the two soldiers who died before this vitiated condition of the atmosphere can be supposed to have occurred, Dr. King calls in the peculiar susceptibility of the European constitution to yellow fever; to account for Anna Gallinha's death, he alleges the state

* Remarks, p. 8.
† In August, 1844, Remarks, p. 6.
‡ Op. cit., p. 6.
§ Ibid., p. 6.
|| Ibid., p. 6.
¶ It does not appear, however, that the season was remarkable, except in the fact of the annual rains being scanty and rather delayed. In October, November, and December, the winds were light and variable, with frequent calms; in consequence of the drought, the grass-crops failed, and next year there was a great mortality among the cattle, attributed by the inhabitants to starvation, from the failure of the crops.
** See our first Number, p. 59.
†† About the 5th October.

of the atmosphere; but how will he account for the illness of the soldier Barbosa, who was a negro, and not susceptible to endemic causes, and who was ill on the 24th or 25th of September, before any fall of rain at all? And yet it is absolutely incumbent upon him to find some cause for this attack as well as for the others.

But, in addition to this deficiency in the argument, it appears to us that Dr. King has by no means made out his assertion that the two soldiers in the fort died from the operation of endemic, that is, local causes. For it has been most clearly proved by Dr. M'William in his Report, and more lately in his remarks on Dr. King's Report, that no endemic sources of yellow fever could have existed in the small island to which the crew of the Eclair were confined. Dr. King has not been able to make his case good by pointing out anything more resembling a malarious "foyer" than "a dusthole similar to the receptacle for manure near the stables and mews of London, which was two thirds full of rubbish, and in the opposite corner of the court there were two common close privies."* And even on the island of Boà Vista, he has not been able to indicate any local source of fever beyond the salt-pan at the back of the town, the exhalations even from which, he allows to have been inoperative in former years, but to have become developed only during the unusual wet season of October, 1845. And therefore, according to Dr. King's own showing, and conceding that the exhalations from the salt-pan at the back of Porto Sal Rey became so malignant seven days after the commencement of the rains as to cause the death of Anna Gallinha in four days—a circumstance on all accounts unlikely—the illness of the two Portuguese and the negro remains unaccounted for, except on the supposition of the susceptibility of the former individuals. It is true that Dr. King points out, as Dr. M'William had already done, that the fort in which the sick of the Eclair were quartered was old, small, ill ventilated, and in every way badly adapted to accommodate nearly 100 men;† and in these circumstances, and in the utter neglect of any proper hygienic treatment of the sick, we cannot fail to recognise the cause of the great increase of disease and mortality which took place when the crew were landed. With regard to the future spread of the disease in Porto Sal Rey, Dr. King is silent; he does not allude to any cases subsequent to that of Anna Gallinha.‡

In the account which Dr. King gives of the illness of Luis Pathi, the labourer of Rabil, he does not accord so closely with Dr. M'William. Thus, Dr. King says that Luis Pathi, after being employed two days upon the Eclair, went on the 14th of September to a festa at Moradinha, and was there taken ill of fever; so far the accounts agree, but Dr. King says this fever was very mild, and lasted only a few days, and that it was not

* Op. cit., p. 11.

† The courtyard of the fort was only 93 feet long and 50 broad. The healthy part of the crew were lodged in sheds at the side of the yard; the sick in the fort itself, which is composed of two stories, in rooms, small, low, and miserably ventilated. (Dr. M'William's Report, p. 70.)

‡ In the rapid spread of the fever over Boà Vista, and in the great mortality (1 to 15·4 of native population), we find an argument that yellow fever had not during the present generation been prevalent on the island; for it is evident that the whole population were in a susceptible state. Dr. King states, that "the lower classes are extremely poor, and often badly fed; they breathe a polluted atmosphere in their crowded and ill-ventilated abodes, and there is a general disregard of cleanliness in the streets and about their houses." (p. 8.) Perhaps it was owing to this bad sanitary condition that the yellow-fever poison retained the contagious property it had acquired, instead of losing it again, as it seems so readily to do when acted upon by currents of pure and tolerably cool air.

for a month afterwards that his three children caught the fever, and all died. Now in this statement we must ask, first of all, does Dr. King admit that Luis Pathi suffered from the same fever which afterwards desolated Boà Vista? We presume from the context that he does, and also from his expression at page 4, where he says that only one labourer contracted fever on board the ship: moreover, if he does not, we infer that he would have broadly said so. But if he does admit it, how does he account for this case in the middle of September, long before the occurrence of the heavy rains, and the supervention of that malarious atmosphere consequent thereupon, to which he attributes the illness of Anna Gallinha? It appears also, from the evidence given by Luis Pathi himself to Dr. M'William,* that he was employed eight days, not two, on board the Eclair, and that he was ill eight days at Moradinha, and nearly three weeks at his own house. His daughter also, he states, was taken ill in the beginning of October, consequently during the time he was lying ill, about a fortnight after the commencement of his illness, and not a month after its cessation, as Dr. King's expression would lead us to suppose. Dr. M'William repeats in his Remarks the dates of the successive attacks of Pathi and his family, which accord with those we extracted from his Report.† Now we feel it right to adopt this evidence, in preference to Dr. King's rather loose assertion, because Dr. M'William states that, seeing the importance of Luis Pathi's evidence, he had taken ample care to sift it. "I considered," he says, "the real facts of this case of so much importance, that the evidence was not finally recorded in my note-book for several weeks, in order that it should be verified by ample corroboration."‡ We think, therefore, that the dates must stand exactly as we have already placed them.§ Dr. King does not allude to the fact of the earliest cases in Rabil occurring in the inhabitants of the houses immediately adjacent to Luis Pathi, who, as proved by evidence, had been in frequent communication with him.

The following summary gives, in a few words, the points of agreement and difference in the statements and inferences of Drs. King and M'William.

1. Both admit the previous healthiness of Boà Vista, and the coincidence of the arrival of the Eclair with malignant fever on board, and the appearance of the same fever shortly afterwards at Boà Vista.

* Report, pp. 42-3. † See our first Number, p. 58. ‡ Remarks, p. 11.

§ The following is part of Luis Pathi's evidence (Dr. M'William's Report, pp. 42-3):

642. How long were you employed on board the Eclair?—About eight days.

650. What family have you?—I have none left.

651. What family had you?—I had a wife and three children.

652. Did they all die of fever?—Yes; all of them.

653. Were you attacked?—Yes; I was first attacked.

654. When were you attacked?—Three days after I went to Rabil from the ship.

656. You were with your family when you were taken sick?—No; I was at Moradinha, &c. &c.

657. How long did you remain at Moradinha?—I was there eight days sick.

658. What did you complain of?—I had general fever, headache, pain of back and limbs; very sick.

660. How long were you sick after return to your own house?—Nearly three weeks.

663. Who, after yourself, was first taken ill?—My daughter, 12 years of age.

664. How long after your return from the Eclair?—It was in the beginning of October.

668. Who was next attacked?—Another girl, 7 years of age, four days after the first died.

669. Who was the next?—My boy, 11 years of age; he was taken ill eight days after the second girl died.

670. And your wife last?—Yes; my wife was taken ill the same day as the last of the children died.

2. Dr. King states that two soldiers died in the fort where the sick of the Eclair were; but attributes these deaths to yellow fever proceeding from endemic sources, and permitted to act by the constitutional predisposition of the subjects.

Dr. M'William gives the same deaths and the same period of their occurrence. But he denies altogether the existence of any local endemic cause of yellow fever.

3. Dr. King states that a sick soldier, accompanied by a comrade, who afterwards became sick, was removed from the fort in consequence of being attacked with the same fever, and was lodged in a house in Pao de Varella. But he does not attempt to account for this man's illness.

Dr. M'William gives the same account of the removal, and attributes the illness to intercourse with the two soldiers above named.

4. Dr. King states that Anna Gallinha was the first person who died in Porto Sal Rey; he attributes this death to a vitiated state of the atmosphere consequent on heavy rains.

Dr. M'William gives exactly the same dates for the death of Anna Gallinha; he also brings evidence to prove that there was the closest and most constant intercourse between Anna Gallinha and the sick soldier.

5. Dr. King does not proceed further with the analysis. Dr. M'William follows it up for five or six successive steps, proving that all those in contact with Anna Gallinha were the next sufferers.

6. Dr. King states that Luis Pathi, the labourer of Rabil, was taken ill on the 14th or 15th of September, of the same fever which prevailed on board the Eclair; but he does not attempt to account for this attack occurring in an insusceptible subject, a month previous to the vitiated atmospheric condition which he has presumed to accompany the rains of October, except by a single expression that Pathi contracted fever on board the steamer.

Dr. M'William gives the commencement of the illness as occurring on the 17th of September.

7. Dr. King states that Luis Pathi's children were not taken ill for a month after this.

Dr. M'William states that these were taken ill in the early part of October.

8. Dr. King takes no notice of the cases of the wife and child of Manoel Fachina, who lived next to Luis Pathi, and had been a good deal in his house while he was sick, and who were both ill either at the end of September or the beginning of October; the child died in three days with black vomit.

It will be seen that, admitting Dr. M'William's evidence in preference to Dr. King's, respecting the time at which the children of Luis Pathi were taken ill, the investigations of Dr. King corroborate in every particular the account we formerly derived from Dr. M'William's Report.

If it were possible to suppose that Barbosa and Luis Pathi were neither cases of yellow fever, and if we were to attribute to mere coincidence the fact that the nurse of one and the children of the other were the first to suffer from the fever originating in unusual atmospheric conditions, then we might perhaps feel some doubt about the fatal cases of the soldiers at the fort. Yet even then how could we explain these, except by admitting

endemic causes, of whose existence we have no evidence, and calling into requisition the presumed power of a constitutional susceptibility, which had been dormant for more than a year in these two sufferers? But even Dr. King does not deny that the cases of Barbosa and Pathi were genuine yellow-fever cases, so that we need not spend time in combating such a supposition.

Or, supposing that we are resolutely determined to deny contagion altogether; supposing that we contend that the death of the two soldiers in the fort, the illness of Barbosa and of Pathi, the deaths of Gallinha, of the children of Pathi, &c, were certainly attributable to yellow fever, but argue that this was independent altogether of the Eclair or of its crew; supposing that we insist on regarding this Boà Vista fever as caused entirely by an epidemic constitution, disregarding the fact that five and probably seven cases had occurred during beautiful weather, and before such alleged constitution had set in with the annual rains* (which nevertheless in 50 previous years had never produced such a fever); supposing we overlook the facts of the way in which the disease confessedly began in two localities to which men had been conveyed, and radiated from these, and the circumstance that the neighbouring islands of the Cape de Verde in sight of Boà Vista were visited by no epidemic constitution, then we say if we make all these and other assumptions, which we need not stop to enumerate, then we have the following extraordinary coincidences, which would in such a case form the strongest series of events which ever were recorded in a case of this kind.

1. The coincidence between the arrival of the Eclair with yellow fever, and the appearance of the same fever immediately afterwards at Boà Vista.

2. The coincidence that, of all the people on the island, the first two deaths should have occurred in the men who were brought most into contact with the Eclair's sick.

3. The coincidence that the next cases should be those of a soldier, the comrade of the two men who were first affected, and of a labourer on board the Eclair.

4. The coincidence that the first undoubted case in Porto Sal Rey should have occurred in the person of the nurse of that sick soldier.

5. The coincidence that the next person ill in Porto Sal Rey should be the fellow-lodger of this last-named case.

6. The coincidence that all the next cases in Porto Sal Rey should be in people having intercourse with these two cases.

7. The coincidence that the first person ill in Rabil should be the familiar friend of the labourer above named.

8. The coincidence that the next, or the simultaneous, cases should be this labourer's own children.

9. The coincidence that this labourer's neighbours and friends should all have suffered before the other inhabitants of Rabil.

* Much confusion has arisen from a statement, that the fever did not appear in Boà Vista till a month after the Eclair left. Even Sir Wm. Burnett repeated this, after receiving Dr. M'William's Report. Sir Wm. Burnett based his statement on a letter of the Portuguese Governor-General to the Consul at Gibraltar, which appeared in the 'Gibraltar Chronicle.' But it appears that even the Governor has now changed his opinion. In a letter to Dr. M'William, dated January, 1848, his excellency says, "I have changed my first opinion. I am quite convinced that the fever was contagious, and that it was introduced into Boà Vista by H.B.M. ship Eclair." (Remarks, p. 10.)

We do not think the case needs additional discussion.* We will merely allude to some other arguments brought forward by Dr. King. These are chiefly founded on negative evidence. Dr King seems to attach great weight to the fact, that the labourers on board the Eclair all escaped at that particular time, with the exception of Luis Pathi. This we do not deem in the least important; and we never could think of arguing, as Dr. King seems to suppose the contagionists must, that the subsequent illness of the labourers, washerwomen, &c., was owing to exposure on board the Eclair, 40, 50, or 70 days previously.

Dr. King enters into a history of the fever on board the Eclair, contending that it was evidently derived from the African coast; he also insists on the fact that exhalations from the unclean holds of vessels employed in the tropics, will produce fevers which, as yet, we cannot distinguish from the most malignant yellow fever. Now on these points we agree with him in toto, but we do not see how this affects the independent evidence derived from Boà Vista.

It appears to us that, in contending so strenuously against contagion, Dr. King has continually been waging mental war with the phantom "Bulam fever," or contagious specific yellow fever. But we are no more followers of Sir Wm. Pym than he is, and yet we believe that, under certain circumstances, severe remittent yellow fever will become contagious. If Dr. King will reconsider this point, we should not be surprised to hear of his conversion into a modified contagionist.

Dr. King concludes his Report with a few very interesting notes on the epidemic at Bermuda in 1843. He was at that time deputy-inspector of hospitals, and of course fully cognisant of all the facts of the case. The epidemic terminated, and left him a more decided non-contagionist than ever. The ultra-contagionists have referred this epidemic to the importation of yellow fever by a certain passenger who was landed from one of the mail-steamers: Dr. King now informs us that this person died from phthisis pulmonalis.

We must now take leave of our two rival Reporters. If we have ventured to differ from Dr. King, we trust we have done so with all the courtesy which we owe and wish to pay to an eminent, most intelligent, and conscientious physician, and with all the caution which we feel bound to display on so difficult a question. With regard to Dr. M'William, we need only say, that the services he has rendered to medical science by his most admirable Reports, do not require our testimony to make them known. Commencing the inquiry into the fever at Boà Vista, with all the predilection in favour of its simple malarious origin which he had derived from his terrible experience in the Niger expedition, Dr. M'William did not allow his prepossessions to bias his inquiry; but, confident that Truth was the goal, no matter in what region she might dwell, he has given the undistorted results of a most laborious and difficult inquiry, and in so doing, has done more for the elucidation of the great subject of the contagion of yellow fever, than any writer since the time of Rush. We can give him no greater praise.

* Dr. M'William pushes the argument much farther than our space will permit, and traces the disease into the other villages. He also adduces evidence, derived from the use of quarantines. But, for fuller details, we must refer to his very able and convincing reply to Dr. King.

ART. X.

1. *On Poisons, in relation to Medical Jurisprudence and Medicine.* By ALFRED S. TAYLOR, F.R.S., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital.—*London*, 1848. Fcap. 8vo, pp. 855.
2. *A Dictionary of Practical Medicine.* By JAMES COPLAND, M.D., F.R.S., &c. Art. *Poisons.*—*London*, 1848.

THE inquiry into the *modus operandi* of Poisons on the animal body, the natural course of the symptoms which they produce, the manner in which these may be modified or kept in check by remedial treatment, the evidences of their action presented by the post-mortem appearances observable in fatal cases, and the proofs of their existence in the system derived from chemical analysis, has been followed up of late years—as we need scarcely tell our readers—with great zeal and ability, and with a considerable measure of success. The progress of chemical science has enabled tests to be devised for many substances, which were previously regarded as undetectible by reagents; and it has suggested improvements of no less importance in the analytical processes employed for the discovery of others, whose presence could only have been certified in former times when so large a quantity was operated on as is but seldom to be met with. In this manner the ends of justice have undoubtedly been promoted; but at the same time new questions have arisen, which, when ingeniously shaped and doubtingly answered, have led to the acquittal of many individuals of whose guilt there could hardly be a reasonable doubt; and it is incumbent on the toxicologist, therefore, to prepare himself to meet every difficulty that can be raised, and to answer with clearness, and with as much certainty as the case will admit of, every objection that can be offered to the validity of his proofs. The records of our courts have abounded of late years in cases, in which the ingenuity of the counsel engaged for a prisoner's defence has detected some deficiency in the chain of proof supposed to be complete, or has thrown a legal doubt over the whole of a laborious and well-conducted investigation, by some insidious question which may really have had but little bearing upon the points under discussion, but which required a full and explicit reply to make its irrelevancy apparent.

Whilst the toxicologist boasts, therefore, of the improvements which have been effected in analytical operations, he must not forget that these improvements are of little worth unless they make the *whole* proof more certain; the suggestion of a doubt, not properly resolved, being sufficient to overthrow the whole fabric of reasoning that had been supposed to be erected with complete security, upon a foundation from which every source of error or weakness had been carefully excluded.

Toxicology, in fact, has now become such a very complex department of study, even in its juridical relations alone, that we are far from being satisfied as to the propriety of requiring that it should be made a subject of medical education, to any greater extent at least than is necessary to enable the practitioner to apply the proper treatment to such cases as may fall under his charge, and to exercise a due discrimination in the observation of symptoms and post-mortem appearances. When a case of

poisoning becomes the subject of juridical inquiry, we deem it much better that the aid of a professed toxicologist should be obtained, that the analysis should be conducted by him, and that he should be answerable for the whole preparation of the case, so far as its scientific bearings are concerned. This plan is the one generally pursued in France and Germany, and frequently also in Scotland; and, in our opinion, with the best results. The additional expense which it would involve is trifling in itself, and is as nothing compared with the advantages which would accrue. The ends of justice would be much better served; and the practitioner would be relieved from a responsibility to which he is seldom equal, and for which he can only qualify himself by neglecting studies more important, because more closely related to his ordinary duties.

We do not, however, at present intend to enter upon the subject of systematic toxicology; for there is another set of inquiries, which has for the time a stronger attraction for us. The *modus operandi* of the substances which commonly rank under the designation of poisons is closely related, on the one hand, to that of various agents which, when introduced into the system, give rise to well-known and specific forms of disease; whilst it is also related as closely to the agency of the remedies, by which we endeavour to counteract the operation of morbific causes and to restore the system to its proper state. It thus has a most intimate connexion both with the science and art of medicine; a connexion which is only now beginning to be duly appreciated, and which, when made an object of more systematic study, will be fertile, we feel assured, in the most valuable practical results. It is always an advantage to be able to watch the operations of external agents on the living system, under simple and definite conditions; because we thereby gain a much greater certainty as to the connexion of cause and effect, than we can do when the conditions are complex, and the operation of the agents less capable of being precisely specified. A poisonous agent, capable of being readily isolated, of being weighed and measured, of being experimented on in various ways, is introduced into the animal body. By carefully-devised experiments we may satisfy ourselves in regard to the channel through which it is taken into the circulation;—we may ascertain whether it exerts any appreciable influence on the constitution of the blood, and thus changes its qualities in regard to the entire assemblage of tissues and organs to which it is conveyed, so as to disorder, more or less, every function in the body; or whether its agency is exerted, chiefly and primarily at least, upon one single tissue or organ, and its effect upon the general system produced solely by the disturbance in the function of that one part;—we may further trace the means which Nature has provided for its removal from the body, either by excreting it in its original state, or by subjecting it to changes of decomposition and recomposition, whereby its original form and characters are altogether lost;—and we may examine how far beneficial results may be derived from remedial measures, and in what way these operate with the greatest certainty and efficiency.

Now the philosophic physician attempts to do all this, in regard both to the morbid poisons whose effects are presented to him as diseases, and to the medicines which he employs with a view to counteract their agency or to produce other curative effects on the system. But he can rarely succeed in doing this to his satisfaction. Very frequently the poison is

one which, although analogical evidence leaves him no doubt of its actual existence, he cannot succeed in isolating, and cannot detect by any reagents. He can neither see nor smell it, taste nor touch it; much less, therefore, can he weigh nor measure it. Who, for example, has ever collected and examined the paludal poison? Yet who would be hardy enough to deny the reality of its existence? The fact is that, just as light, heat, and electricity often make themselves known to us by their specific effects upon organized structures, even better than by their operation upon inorganic matter, so may we regard the living animal body as a more delicate test of the presence of certain material agents, whose operation is manifested in the disturbance of its ordinary functions, than the balance and the test-glass of the most accomplished chemist can yet be accounted. And there is no more reason for denying the existence of a paludal poison which produces uniform and definite effects upon large numbers of individuals, because the chemist has failed to isolate it and cannot tell how its properties can be rendered obvious to the senses, than there would be for denying the existence of any chemical base, whose presence in combination might be made apparent by its reactions, although of its properties when reduced to its simple form we may know nothing. But if he cannot isolate the poison, he cannot with positive certainty trace the channel of its introduction into the body; neither can he follow it through the course of the circulation, nor can he do much more than guess at the mode in which it produces its deleterious influence upon the system, nor ascertain the method of its elimination from the body. All his best information on these points is derived from a comparison of the phenomena of the disease with those resulting from the action of undoubtedly poisonous agents; the investigation of the latter, therefore, deserves to be carefully pursued, if only as a department of the science of medicine, strictly so called.

This is still more the case with regard to the action of remedies. There is scarcely one of these that may not be accounted a poison; for almost every remedy acts by inducing a change in the natural course of some function, which, if it take place to an excessive degree, produces a fatal disturbance of the whole. The proposition may be put in a converse form. Our readers are probably all acquainted with the adage "*majus venenum, major remedia,*" which, although not correct in every case, certainly has a general truth; for every poisonous substance is so in virtue of its producing some violent disturbance in one or more of the functions, such as, in a large number of cases, may have a therapeutic effect when more mildly excited in certain disordered conditions of the system. The practitioner has been too much accustomed to trust to his empirical knowledge of the action of the medicines he uses. This knowledge is founded only on their ostensible results; and their secret operations within the penetralia of the system have been considered beyond his ken. But here, too, the progress of chemical and pathological inquiry is exercising a most beneficial influence; and great progress is being made in determining the real *modus operandi* of our most valued remedies, and in thus laying a much better foundation for their safe and certain therapeutic employment. Now this investigation has the closest relation to scientific toxicology. In fact, there is no difference whatever in the mode in which the action of *remedies* and the action of *poisons* should be traced out; for in each case

an external agent is introduced into the system, which is capable of modifying its functions; and the same methods must be employed in tracing the precise nature of this modification, whether it is so great as to be attended with fatal consequences, or whether it is comparatively trivial and transient in its nature. Thus, the inquiry into the "physiological action of medicines" frequently becomes strictly toxicological; because, in order to obtain the *modus operandi* of a particular remedy, it is often necessary to administer it in doses large enough to produce death, by (so to speak) the exaggeration or magnification of its ordinary results.

We have said enough, we trust, to show the very intimate relation which exists between scientific toxicology and practical medicine. The title of Mr. Taylor's book led us to hope to find in it the development of this relation, but we find it presented in no other aspect than the following:

"Probably there is no branch of medicine in which we meet with a larger assemblage of truths ascertained by observation, and combined under one common character. To the physician, the pathologist, and the medical jurist, a knowledge of toxicology is of great importance; for cases are continually presenting themselves in which a practical application of the principles of this science is demanded—as, for example, in the treatment of an individual labouring under the effects of poison,—in drawing a clear distinction between changes produced in the body by disease, and those caused by poison,—or finally by aiding the criminal law in detecting and punishing those who have been guilty of the crime of poisoning." (p. 1.)

The relation here pointed out is that, merely, of medicine as an art to toxicology as an art; that is, it is one which merely arises out of the practical applications of the principles of the respective sciences; and has nothing to do with the fundamental connexion which we have shown to exist between the principles themselves, and which arises out of the community of the basis on which they are erected. We have been disappointed, also, in our expectation of finding some general notice of this connexion, in the article on Poisons in Dr. Copland's 'Dictionary of Medicine.' True it is, that this article is not yet completed; but as the part already published contains the general disquisition on poisons and poisoning, and the special account of nearly all the classes under which poisons are arranged by the author, we may reasonably infer that the subject will not be noticed otherwise than incidentally, if alluded to at all. We shall therefore take up the thread of inquiry; and shall endeavour to bring together some of the general facts of toxicology, in a form that may show their bearing on the science of medicine.

The first point of inquiry has reference to the *mode in which poisons act* upon the living body. In some instances the immediate local operation of the poison on the part to which it is first applied, is the most important part of its agency. This is the case with the corrosive poisons in general; the constitutional suffering being dependent upon the local injury, and proportionate to its amount. But because a poison has a local effect, and this effect happens to manifest itself in the part to which it was first applied, we are not always hence to conclude that such action is direct. Thus it may be questioned, as Mr. Taylor has pointed out (p. 18), whether the inflammation of the mucous lining of the stomach, which is one of the ordinary consequences of the ingestion of arsenic, is due to the contact of the poison with the membrane; or whether it is not really a remote effect depending on the absorption of the poison into the

circulation. In support of the latter view it may be urged, that there is no proof that arsenic applied in powder or in solution to a dead or living surface, has any corrosive or other chemical action;—that the degree of inflammation is not at all proportionate to the amount of the dose;—that inflammation of the stomach has been found in cases where the arsenic was applied externally to a wound or ulcer;—and that many other mucous membranes participate in the inflammatory affection, if life be prolonged sufficiently to develope it.

There are poisons which exert other than obviously *chemical* influences on the parts to which they are first applied. Thus various kinds of irritants owe their first effect to their peculiar influence upon the *vital* properties of the tissue on which they act; and in the present state of our knowledge, we cannot do otherwise than surmise, whether this influence is, or is not, exerted through a purely chemical change in the condition of the living part. So, again, there are many poisons which deaden the sensibility of the nervous structure, and weaken or destroy the contractility of muscle, when directly applied to these tissues; but such direct action has usually but little to do in bringing about a fatal result, this being almost always attributable to the influence of the poison in some remote organ, quite independently of its local action. The brain, the spinal cord, the heart, and other organs most closely connected with the maintenance of the vital functions, are but little subject to the direct agency of poisons; and these are usually introduced into the system through the medium of the alimentary canal, or in some instances through the lungs, and occasionally through breaches in the cutaneous surface.

But in what way is the influence of the poison transmitted to the remote organ on which its effects are chiefly manifested? This question, as our readers must be aware, has been long and keenly debated; some toxicologists maintaining that the remote action is always due to the absorption of the poison, and to the transmission of it in the current of blood through the organ on which its effects are manifested by the symptoms; whilst others have thought that the remote effects were *sympathetic*, the nervous system of organic life being the channel of their transmission. For some time the mixed hypothesis of Messrs. Addison and Morgan was generally received with the most favour,—namely, that poisons are absorbed into the circulating current, but that their remote action is due to the transmission of their influence through the sympathetic nerve. Of late, however, the tendency has been to revert to the first of these doctrines, almost to the exclusion of the second; the remote action of any poison being in reality its direct and immediate action upon some organ or organs, to which it is conveyed by the circulating current, and on which it has some specific effect. Mr. Taylor upholds this doctrine very forcibly; and cites abundant proof of the presence of poisons in the circulating fluid, part of which is furnished by their removal from it by the excreting processes. To these proofs we shall refer hereafter. But at the same time, he does not altogether give up the notion of sympathy; chiefly because in certain cases (as that of a powerful dose of prussic acid) the first effects are produced before there would seem to be *time* for the absorption of the poison and its conveyance to the nervous centres. He cites the rapidity of the action of the rattle-snake bite in proof of the

same position; but he is here, we think, rather unfortunate in his illustration, for the whole course of the phenomena of this kind of poisoning appears to us to indicate the action of the noxious matter upon and through the *blood*. That local lesions can produce serious changes in the vital functions through nervous sympathy only, cannot be doubted by any one who has attended to the phenomena of *shock*. In fact, syncope more frequently results from a depressing influence thus communicated to the heart from a remote source, than from any cause primarily affecting that organ itself. And we see this well marked, in cases of severe lesion of the viscera by corrosive poisons; a burning of the mucous membrane of the stomach by sulphuric acid causing much the same general prostration, as an extensive burning of the cutaneous surface by an actual flame. It is more difficult, however, to imagine how a poison whose local effects are of a comparatively trivial character, can produce remote effects of the most deadly kind by nervous sympathy only. Yet the instantaneousness of these effects in particular cases leaves us at a loss to account for them in any other way. Thus Mr. Taylor mentions (p. 28), that in experimenting upon cats with prussic acid, he has seen the effects produced so rapidly, that there was no sensible interval between the application of the poison to the tongue and their production; and that death took place in a period of time actually shorter than that, which is commonly regarded as the briefest that can be allowed for the absorption of the poison and the production of the first symptoms. It must certainly be admitted that, in the present state of our knowledge, the idea of sympathetic action, however improbable it may seem, is the one that best meets such cases as these.

But there can now be little doubt, that the action of most poisons on remote organs is really as *direct*, as if they were applied to the substance of those organs; that is to say, that the poison, being carried into the current of the circulation, has an immediate action on certain of the tissues through which it is transmitted. This conclusion, which seems best in harmony with the great mass of observations formerly collected, is strengthened by the recent experimental researches of Mr. Blake;* who found that when certain poisons, having a specific action on particular organs, were injected into the venous circulation, the time required for the first development of their effects corresponded closely, in numerous cases, with the time that elapsed before they could be detected in the arterial current; so that it may be fairly presumed that, in the case of other poisons so injected, they were carried along in the same manner, and took effect only when they had arrived on the organ specially influenced by them.

The time required for the action of poisons taken into the stomach, however, is subject to very considerable variation; the difference being dependent on the rate of absorption, which is modified by the state of the stomach at the time. That medicines as well as poisons act most quickly and certainly, when taken upon an empty stomach, has long been known; and there have been cases in which the action of large doses of powerful poisons has been remarkably retarded, apparently from the reverse condition of that viscus. Much light has been thrown upon this question by the observations made not long since by Mr. Erichsen,† in a case of extroversion of the bladder, which enabled him to collect the urinary

* Edinburgh Med. and Surg. Journal, October, 1841.
† Medical Gazette, vol. xxxvi, p. 363

secretion directly as it passed from the kidney. His first set of experiments was on the ferrocyanide of potassium, which, being swallowed in solution, was immediately looked for in the urine, and was detected in periods varying, as shown in the subjoined table, from *one* minute to *thirty-nine* minutes. It will be seen, by an inspection of the table, that the earliest appearance of the salt was always when the stomach was empty, and the latest when it was full.

EXPERIMENTS WITH FERROCYANIDE OF POTASSIUM.

No. of Experiment.	Quantity taken.	When first appeared in urine.	When last meal taken.	Nature of last Meal.
1	20 grains	12 minutes	2 hours	Meat, potatoes, and bread
2	40 „	2 „	4 „	Bread and butter, and coffee
3	40 „	6½ „	1½ „	Mutton, bread and butter, and tea
4	40 „	2 „	11½ „	Potatoes
5	20 „	1 „	11 „	
6	30 „	16 „	24 min.	Bread and butter, and tea
7	40 „	2½ „	4¼ hours	Plum-pie
8	40 „	14 „	1 hour	Bread and butter, and tea
9	40 „	27 „	2 min.	Bread and butter, and tea
10	40 „	39 „	2 „	Mutton, bread, and potatoes.

A second set of experiments was made with vegetable substances, of such a character as to pass unchanged into the urine, and capable of being distinctly recognised there. It will be seen from the subjoined table that these substances were, on the whole, much longer in making their appearance in the urine than was the ferrocyanide of potassium; but none of the experiments upon them exhibit either the shortest or the longest periods, as the observation was in no case made immediately after a meal or after a long fast.

EXPERIMENTS WITH VEGETABLE SUBSTANCES.

No. of Experiment.	Substance taken.	When first appeared in urine.	When last meal taken.	Nature of last Meal.
11	Infusion of galls	36 minutes	2½ hours	Bread, meat, and potatoes
12	Ditto	33 „	1½ „	Bread and butter, and coffee
13	Ditto	30 „	1¾ „	Bread and butter, and tea
14	Infusion of rhubarb	22 „	2¾ „	Potatoes and dripping
15	„ madder	16 „	3¼ „	Bread and dripping
16	„ uva ursi	35 „	1¾ „	Bread and butter, and tea
17	Tincture of rhubarb	31 „	2½ „	Liver and bacon, and bread
18	Decoct. of logwood	19 „	4¼ „	Bread and butter, and tea.

It is probable that the state of fulness or emptiness of the sanguiferous system would have a considerable influence over the rate of absorption, as well from the alimentary canal, as from any other part of the body. There cannot now be any doubt that the blood-vessels, rather than the lacteals, are the channel by which soluble salts, &c. are chiefly taken up from the walls of the alimentary canal; the same will be the case when poisons, &c. are introduced into a wound. When solutions of soluble salts, however, are applied to the skin, they seem to enter the lymphatics

more readily than the blood-vessels; as might be anticipated from the vast amount of the former distributed through its substance, and from the thinness of their walls. On the other hand, the absorption of poisons in the state of gas or vapour, through the medium of the lungs, will obviously be effected by the vast surface of capillary blood-vessels spread out upon the lining of the air-cells; and we find, as might be expected, that volatile substances are more rapidly and effectually introduced into the circulation in this manner, than in any other. We have a remarkable example of this, in the noxious effects of even a very minute quantity of arsenic, when it is inhaled in combination with hydrogen. Very frequently, too, the operation of these substances is modified in a remarkable manner by this method of exhibition; thus, ether and chloroform, taken into the stomach in a liquid state, do not produce by any means the same results as when respired in the state of vapour. These differences are probably to be explained by reference to the well-known fact, that the chemical operation of many substances is considerably modified according to the fineness of their state of division.—We are not aware that any precise observations have been made, tending to show a difference in the rate of absorption through the lungs, according as the vascular system is turgid or the contrary; but there are many indications that a deficiency in the fluids of the body greatly favours absorption of watery vapour from the atmosphere; and the generally admitted fact, that an exhausted or debilitated state of the system acts strongly as a predisposing cause in favouring the operation of poisonous miasmata, whilst a state of vigorous health, rather tending to plethora, is equally opposed to their agency, is probably referable, in part at least, to the same category. It is a circumstance of much importance, that the influence of poisonous gases is exerted as well through the skin as through the lungs; thus, Mr. Donovan states that a rabbit, whose body was inclosed in an atmosphere of sulphuretted hydrogen gas, but which was allowed to breath freely in the atmosphere, perished in ten minutes.

Having thus considered the mode in which poisons are introduced into the system, and are conveyed to the organ on which their peculiar influence is exerted, we have next to inquire *in what way their specific effects are produced*. On this point we have very little definite information; all that we possess, however, favours the hypothesis, that these effects are the result of a modification in the chemical composition or physical relations of the tissues acted on. Strong arguments from analogy may be urged in support of this view. The phenomena of catalytic action, in which a substance A modifies the relations of two other substances B and C, without itself entering into combination with either of them, seem to find their parallel in many of the phenomena produced by poisonous agents. Nor need the very minute quantities in which the latter frequently act, be brought as an objection to this view; for inorganic and organic chemistry alike furnish examples of similar agency. The following curious facts bearing upon this subject were cited by Sir G. Lefevre in his Lumleian Lectures for 1845.

"The sesqui-chloruret of chrome, chemically pure and sublimated, is completely insoluble in water, either cold or boiling. It is equally insoluble in all acid solutions. It is not attacked by concentrated and boiling sulphuric, nitric, or nitro-

muriatic acid. All of these are alike without effect on this combination, one of the most stable in the mineral kingdom. It is formed by $Cr_2\ Cl_3$.

"The protochloruret of chrome differs from the sesquichloruret by the abstraction of an atom of chlorine. It is represented by $Cr_2\ Cl_2$. This second salt is highly soluble, and absorbs with the greatest avidity the oxygen contained in the air and in water to form new product $Cr_2\ Cl_2\ O$.

"A quantity, however infinitesimal, of the proto-chloruret of chrome, mingled with one million times its weight of the sesqui-chloruret, transforms this from an insoluble salt into a salt soluble in every proportion, if the protochloruret is pure; but if it has absorbed any portion of oxygen it becomes inert, and no longer alters the solubility of the sesqui-chloruret.

"Here, then, is an example of a substance as nearly allied as possible in its chemical composition to a second substance, completely changing one of the most important chemical properties of that substance, even though mixed with it only in infinitesimal proportions; and this action is itself neutralized by the introduction into the poison (for the proto-chloruret is here, in reality, the poison of the sesqui-chloruret) of an infinitesimally small quantity of oxygen." (Med. Gaz. 1845, vol. i, p. 313.)

The following is another experiment of the same kind, in which an organic compound is concerned.

"If one ounce of iodic acid, half an ounce of oxalic acid, and two ounces of water be mixed together in a glass bottle, and be exposed together to a moderate heat (77° F.) and to the light of the sun, a process of combustion immediately commences. The oxalic acid is burnt, hydriodic acid is formed, carbonic acid is evolved, and after a short space of time the whole of the oxalic acid is destroyed. The same results, but to a less degree, are produced when tartaric, lactic, and formic acids are used.

"If in any one of these acid solutions a drop of hydrocyanic acid is let fall, the whole process of combustion is arrested. The two acids henceforth exert no influence on each other, and the mixture may be exposed for days and weeks to the action of the sun, and to a temperature of 60°, 80°, 100° C. (212° F.), without the slightest reaction taking place, or a particle of carbonic acid being evolved." (Op. cit., p. 313.)

That the operation of saline purgatives may be in great part accounted for on physical principles, has long been understood. It was found by Poisseuille that endosmose took place through animal tissues, from the serum of the blood, to seidlitz water, and to solutions of sulphate of soda, or common salt. And as the evacuations from the intestines produced by these purgatives have been found to contain albumen, it can scarcely be doubted that their operation is mainly due to an endosmotic action between the blood in the capillaries of the intestines, and the saline solution contained in the alimentary canal. It is less generally known, however, that the effect of an opiate in checking such a process is also referable to physical principles; it having been found by Poisseuille, that muriate of morphia, when added to saline solutions, very considerably weakens the endosmose from the serum to the solution, and ultimately changes the direction of the current.

The manner in which poisonous as well as medicinal agents of any description (except those which have a powerful chemical action exerted alike upon every tissue, dead or living), *single out* particular organs or tissues, is obviously analogous to the ordinary phenomena of nutrition on the one hand, and to those of chemical action on the other. On this point we have dwelt so recently (in the review of Mr. Paget's Lectures in

our last Number), that we need not now revert to the consideration of it. We may quote in connexion with it, however, a very suggestive remark by Dr. W. Budd (in his paper on the Symmetry of Disease) regarding the *toleration* of certain medicines which take effect after absorption into the system.

"It is well known that certain of these, so long as they accumulate in the system and circulate in mass with the blood, set up very serious constitutional disorder; but as soon as they become derived to a particular part, on which their action is then specially exerted, especially when this part affords an outlet from the body, the constitutional disorder ceases. . . . But what is most remarkable is, that when once this direction to a particular part has taken place, that very fact becomes a cause, just as a nucleus causes accretion of similar crystalline substances around it, for all future supplies of the same matter to be drawn to the same point; so that, however free the supply may afterwards be, there is less danger of accumulation in the general mass of blood, less danger of general disorder. Familiar examples of this are seen in the administration of mercury and tartar emetic." (Med. Chirurg. Trans. 1841.)

A most interesting and suggestive doctrine in regard to the action of poisons and medicinal agents, founded upon a long series of experimental researches, has been put forth not long since by Mr. James Blake, in successive reports communicated year by year to the British Association, and published in their transactions. As these are not adverted to, either by Mr. Taylor, or Dr. Copland, we had intended to prepare a condensed summary of these researches, for the information of our readers; but this has been done much more satisfactorily by Mr. Blake himself (now professor of anatomy in the university of St. Louis), who has recently transmitted a concise account of his experiments to the American journal of the Medical Sciences (Jan. 1848), under the following title:—"On the influence of isomorphism in determining the reactions that take place between inorganic compounds, and the elements of living beings." As few of our readers have access to this journal, and as the subject is one of deep and general interest, we shall not apologise for transferring to our pages all that we deem most important in this valuable contribution.

Mr. Blake's first experiments were made merely for the sake of observing the results that followed the introduction of certain saline compounds into the blood; but the phenomena that presented themselves were, in some instances, so striking, as to lead him to extend his researches much further than he had originally intended; and subsequently the indications of the existence of a new law, governing the reaction between the elements of living beings and inorganic compounds, induced him to carry on these researches, so as to comprehend, amongst the reagents on which he has experimented, compounds of no fewer than twenty-eight of the simple elements. The manner in which these experiments were performed, was to introduce a solution of the substance experimented with into the veins or arteries of a living animal. In most instances they were injected into the jugular vein; and would then, of course, pass first through the right cavities of the heart, and through the pulmonary capillaries, before entering the left cavities of the heart or being circulated through the general system. In some instances, the influence of the substance injected, in stopping the heart's action or producing a stagnation of the pulmonary circulation, was so sudden and decided, that, in order to

ascertain its action on the nervous centres, it was necessary to inject it into the arteries leading to them.

The phenomena principally observed and noted were those connected with the more important functions of the animal economy; such as respiration, circulation, and the functions of the nervous system. The hæmadynamometer gave important assistance in the observation of changes in the force of the current of blood, and revealed many interesting facts which must have otherwise passed unnoticed.

"If, for instance, a substance that had been introduced into the blood gives rise to an obstruction in its passage through the pulmonary capillaries, this is instantly shown by the descent of the mercury in the instrument; for the supply of blood which reaches the left side of the heart, and the arteries, being cut off, the pressure of the blood in the arterial system is necessarily diminished; and this will take place in four or five seconds after the injection of the substance into the jugular vein. But if the substance used passes through the capillaries of the lungs, the next point at which we can trace any signs of its action is when it circulates over the parietes of the heart, or when it arrives at the capillary terminations of the coronary arteries. Should the reagent be of a nature to destroy the irritability of the heart, its motions are found to be arrested in from seven to ten seconds after it has been introduced into the vein; in this case, the pressure in the arterial system suddenly falls, and no oscillations are visible in the column of mercury. Should even the slightest change be produced in the action of the heart by the passage of any substance over its parietes, this would be instantly indicated by a modification of the pressure in the arterial system. Should any effect be produced on the passage of blood through the systemic capillaries, the pressure in the arteries instantly becomes increased if the systemic capillary circulation be obstructed, and diminished if the passage of blood through these vessels is facilitated." (Op. cit., p. 65.)

Mr. Blake gives a tabular statement of the results of his experiments; specifying in the first column the substances experimented on and the quantity of it required to bring the circulation to a stand; in the second, its action on the heart; in the third, its effects on the nervous system; in the fourth, its effects on the respiratory organs; in the fifth, its effects on the capillary circulation; and in the sixth, he adds some general remarks. As this table, however, would occupy six of our pages, we deem it too long for introduction here; our purpose being rather to give to our readers an idea of the general facts which these results tend to establish, than to put them in possession of the mass of individual phenomena, for which the original memoirs must be consulted by those who desire to acquaint themselves with them.

One of the first things that becomes apparent, even on a superficial inspection of the table, is the entire discrepancy in the reactions which take place between the elements of living beings and the inorganic compounds introduced into their circulation, from those which might have been anticipated from a consideration of the ordinary chemical properties of the reagents employed:

"If, for example, we consider the action of some of these substances on the irritability of the heart, we find that by one powerful alkali (potash) it is suddenly destroyed; whilst by another (soda), it is increased; by some acids (the phosphoric and arsenic), it is deadened; by another class (the chloric), it is augmented in a remarkable degree; a strong caustic base (soda) augments it, whilst a comparatively chemically inert salt (the chloride of palladium), even in very small quantities, destroys it. A few grains of nitrate of potash arrest its movements in a few seconds, whilst more than twice that quantity of nitrate of silver only seems to

increase its irritability. As regards the passage of blood through the pulmonary capillaries, this seems impeded by all acids; but we find that the same effect is produced by soda, and the salts of silver and lead." (Op. cit., p. 71.)

Nor is there any greater agreement between the actual results, and the expectations which might have been formed on the supposed physiological actions of the respective substances:

"We see an astringent substance (sulphate of iron) facilitating the passage of the blood through the capillaries, and preventing its coagulation; whilst an alkali (potash) causes the blood to coagulate more firmly, and at the same time affords an obstacle to its passage through the capillaries. We find arsenic exerting no marked effect on the tissues, even when injected in large quantities, whilst a much smaller quantity of carbonate of potash is rapidly fatal; we find the same substances (the salts of baryta) destroying the irritability of the heart whilst they increase that of the voluntary muscles in a great degree (a fact which had been noticed by Haller as regards the salts of lead). Did these experiments only show the necessity of modifying our notions, derived from the common properties of bodies, when we come to consider their action on the living organism, they would be of some use in clearing away many false views which must exert an injurious influence on the study of physiological chemistry." (Op. cit., p. 72.)

But it is justly remarked by Mr. Blake, that their results are not simply of a negative character; they do not merely prove that the reactions which take place in the living body are not to be explained by the common chemical properties of matter;—but they point to a new law, which shows that, under these circumstances, in which the ordinary properties appear to lose their application, a new and more latent property of matter comes into play, and exerts its influence over a most extensive series of phenomena. A comparison of the results, when these are properly classified, shows that the various substances may be arranged into groups, according to the similarity or the dissimilarity of their effects; each group being distinguished by reactions not to be found in any other class, whilst the reactions of all its members closely resemble each other. The classes thus formed agree with those which are adopted by chemists in their arrangement of the different elements according to their isomorphous relations; and the only conclusion to be drawn from this interesting coincidence is, that *the physiological action of these substances depends upon some property they possess in connexion with their isomorphous relations.*

"Thus we find potash and ammonia agreeing very closely in the phenomena they give rise to; again we have strontia, baryta, and lead, all producing reactions nearly resembling each other, and all characterised by their influences on the muscular system. Soda and silver also agree very closely in the phenomena to which they give rise.

"We then find a very large family of substances, including lime, magnesia, zinc, iron, copper, manganese, nickel, and cadmium, all producing effects which resemble each other, and distinguished from all other bodies by their action on the nervous system.

"Platinum, palladium, iridium, and osmium readily arrange themselves in a distinct class, agreeing as they do with each other in most of their reactions; another well-marked group is formed by phosphorus, arsenic, and antimony. Selenium and sulphur are found closely to resemble each other in their reactions on the living organism; and between the remaining elements, chlorine, iodine, and bromine, the most striking analogy exists in their physiological action.

"This law has been arrived at by an experimental investigation of the physiological action of the compounds of the elements forming all the well-marked

isomorphous groups; and in no instance has there been found an exception to it; for the apparent exception which has presented itself by the separation of soda and silver from the potash group, I conceive to be owing to these substances having been united into one group on insufficient grounds; for, whilst the isomorphous relations between soda and silver are well-marked, and also between potash and ammonia, it still admits of doubt whether any well-marked relations exist between the first two and the last two substances." (Op. cit., p. 72.)

Mr. Blake seems not aware of the confirmation which his views derive from the experiments performed some years since by Dr. Daubeny on the absorption of mineral substances by the roots of plants. He found that if a plant naturally absorbs the compounds of any particular base, it will also take up those of another base which are *isomorphous* with them; for instance, most vegetables will absorb the salts of lime and magnesia with equal readiness. But salts, however soluble, which have a crystalline arrangement that differs from theirs (such as the salts of strontia) are not absorbed. Although these facts are themselves of a different order, yet they agree with those brought to light by Mr. Blake's researches, in indicating a relation between the isomorphic relations of inorganic substances, and their action upon the living structure; and thus confirm his general conclusion. Dr. Daubeny's experiments are quite in harmony with the general fact adduced by Mr. Blake as one of the deductions founded on the results of his experiments;—namely, that the substances which appear to exert the least injurious effect, or to produce the slightest change in the animal economy, when introduced directly into the blood, are those which either exist in the body as constituents of some of its fluids or solids, or which have isomorphous relations with some of these; whilst on the other hand, it is found that those substances, which have no isomorphous relations with the elements of the body, are those which are most fatal. Thus the compounds of arsenic, which are isomorphous with those of phosphorus,—and the salts of silver, which are isomorphous with soda,—are examples of the first proposition; since they produce but very slight effects when injected into the veins, except in large quantity. On the other hand, the very poisonous effects of palladium and baryta, which produce almost immediate death when injected in smaller quantities, are examples of the second. It is remarked by Mr. Blake as a fact of interest in this connexion, that silver appears to possess the power of entering into the composition of the animal tissues, without destroying their vitality or interfering with their functions; and he suggests whether it does not replace soda as an element in the composition of the tissues, as when the skin is coloured by the internal administration of nitrate of silver, in the same manner as other *substitutions* take place in organic compounds.

We shall now advert to some of the more interesting physiological phenomena presented by Mr. Blake's experiments; and first, to those which have reference to the action of the heart. The metallic salts in general agree in destroying the irritability of that organ; as do also the salts of potass and baryta; together with the arsenic and phosphoric acids. On the other hand, the greater part of the acids seem to increase the irritability of the heart; as do also the salts of soda and of silver (which last is a remarkable exception to the metals in this particular); whilst lime, except in large doses, exerts but slight influence over that organ. The

action of the chlorine group of acids is very remarkable; for they increase and prolong in a marked degree the irritability of the ventricles, whilst they seem to weaken that of the auricles. The ventricles will continue irritable for ten or twenty minutes after mechanical stimuli have ceased to affect the auricles; and circulation will continue for many minutes after respiration has ceased, with even more vigour than it ordinarily has during life. The pressure in the arterial system has been found equal to a column of mercury of seven inches, eight or ten minutes after respiration had ceased; and even when the pressure has sunk to two inches, it will again rise to five or six; the heart apparently receiving a fresh stimulus, although the animal had been seemingly dead for some minutes. The acids formed by the combination of chlorine, iodine, and bromine, with oxygen and with hydrogen, all agreed remarkably in the effects produced; the chief differences being in regard to the amount required to develope them, twenty drops of chloric acid being equivalent to ten drops of hydrochloric, to ten grains of bromic acid, to fifteen grains of iodic acid, and to three drachms of hydriodic acid. There is no relation whatever between the influence of different substances on the heart's action, and their power of preventing or retarding the coagulation of the blood. The apparent exceptions observed by Mr. Blake, to his general principle of the corresponding actions of isomorphous substances, so far as regarded their influence on the heart, had reference rather to the quantities of the substances required to produce the same results, than to the results themselves. Thus baryta and strontia are closely related as to their isomorphous properties; and they agree in depressing the irritability of the heart; but whilst *two grains* of any soluble salt of the former, injected into the veins, are sufficient to paralyse the heart, *two drachms* of the salts of the latter are required to produce the same effect, smaller doses diminishing the rate of the heart's action, and rendering it weak and irregular.

The following are Mr. Blake's remarks on the action of the various substances injected by him into the blood, on the nervous system:

"If we turn our attention to the phenomena recorded in the third column of the above tables, we find the same want of agreement between the physiological action of these substances and their chemical action. Sometimes it is by an acid, sometimes by an alkali, that we find the functions of the nervous system destroyed; in some cases we see that considerable quantities of what are considered the most powerful poisons, can be brought into contact with the brain, without producing any marked symptom, whilst small quantities of some of the neutral salts are extremely fatal to it. The well-marked analogy that exists between the action of the whole of the magnesian class on the nervous system, although including bodies which differ so widely in their chemical and physical properties, affords a striking confirmation of the connexion of the action of these substances with their isomorphous relations." (Op. cit., p. 74.)

The soluble salts of lime, magnesia, zinc, iron, copper, manganese, nickel, and cadmium, when injected into the blood, are stated to produce a remarkable prostration of the power of movement, the sensibility remaining unimpaired. But it is by no means clear that this prostration is due to the influence of these substances on the *nervous* system. Any general disturbance of the functions of the nervous centres would be manifested, one would suppose, in affection of the sensibility as well as

of the motor power; and the fact that all these substances depress, or even paralyse the *heart's* action, would seem to show that they have a special influence on the muscular system. Mr. Blake then continues:

"As regards the general action of these inorganic substances on the nervous system, I find they differ from the poisons derived from the vegetable kingdom. The only substance amongst them whose action at all resembles that of the vegetable poisons, is ammonia, a substance which appears to form the link between organic and inorganic compounds, and which (or at least some compound of nitrogen, according to a remark of Liebig) enters into the composition of all vegetable poisons. The action of this substance offers the only anomaly as regards the classification of these substances, from their effects on the nervous system, according to their isomorphous relations." (Op. cit., p. 75.)

The anomaly here referred to, is the complete inertness of the salts of soda and silver, as regards the nervous system, when compared with those of potassa and ammonia; the latter giving rise to spasms resembling those produced by strychnia, and, when injected in very strong doses, arresting the respiratory movements before the action of the heart ceases. The following needful caution is added by Mr. Blake:

"There are certain sources of fallacy which, in these experiments, may interfere with our appreciating with exactness the direct action of these substances on the nervous system; particularly the derangements that many of them produce in the circulation through the pulmonary and systemic capillaries. From both of these causes the brain becomes subject to increased pressure, in the one case from venous congestion, and in the other from the accumulation of blood in the arterial system; the pressure caused by the obstruction of the pulmonary circulation, occurring, as it does, in the vein, seems to be much more injurious than when a far greater amount of pressure is supported by the arteries. I have seen the functions of the nervous system but little affected by a pressure in the arteries equal to ten inches of mercury, whilst in the venous system the pressure of an inch and a half, or two inches, is sufficient to arrest the functions of the nervous system. There is one fact in relation to the action of those substances on the 'nervous system, which is worthy of note, viz. that nearly all those bodies which agree in producing great prostration, also agree in preventing the coagulation of the blood." (Op. cit., p. 75.)

One of the most curious of all the results arranged by Mr. Blake under this head, is that produced by the salts of baryta, strontia, and lead; all of which act strongly, as already stated, in diminishing the irritability of the heart, but seem to favour the continuance of the respiratory movements, even for some minutes after the heart's action has ceased and the circulation has come to a stand. We notice, too, that the acids of the chlorine group rapidly destroy the functions of the nervous system; particularly when injected into the arteries.

The account given by Mr. Blake of the action of the various substances experimented on by him, on the respiratory organs, is less satisfactory than we could have wished, owing to a want of information in regard to a very important portion of the phenomena. Thus we are told that the result of the injection of the acids, and of the salts of soda, lead, and silver, is to produce a congested state of the lungs, sometimes amounting to hepatization, with a frothy secretion that often fills the bronchial tubes; but we are not informed as to the rate of the respiratory movements, a diminution in which, such as is occasioned by section of the pneumogastric nerve, has been shown by Dr. J. Reid to be of itself a sufficient cause for these phenomena. The salts of the platinum group produce a curious

effect on the respiratory movements, rendering them intermittent in a marked degree. Sometimes the movements are altogether suspended for one or two minutes, and then recommence; and this may take place many times. The lungs are found to contain but little blood, after death from the injection of these poisons. The agreement of the salts of lead with those of silver, in their influence on the respiratory function, constitutes the only exception which this group of facts offers to the general law propounded by Mr. Blake.

The effects produced by the introduction of these agents upon the capillary circulation, both pulmonary and systemic, seem capable of being observed with tolerable accuracy, and suggest some considerations of much physiological interest. All the acids are regarded by Mr. Blake as producing an impediment to the passage of the blood through the *pulmonary* capillaries; and some of these substances appear to exert the same action on the systemic capillaries, whilst others pass through the systemic capillaries without producing the slightest obstruction. On the other hand, there are many substances that pass readily through the capillaries of the lungs, but cause great obstruction in the *systemic* capillaries; this is the case with ammonia and potash and their compounds, and still more remarkably with regard to the salts of the platinum group, as shown by the great pressure on the walls of the arteries, which does not diminish until some minutes after the heart has ceased beating. Mr. Blake is inclined to think that there are some substances, such as the salts of iron, zinc, manganese, &c., which even facilitate the passage of the blood through the capillaries; but he remarks that it is difficult to obtain any positive proof of this. This class of facts evidently possesses an important bearing on the question of the forces which influence the capillary circulation. It seems to us impossible that the results can be explained by the supposition, that the reagents employed alter the physical characters of the blood, in such a manner as to make it pass less readily through the capillary tubes; since, however well this might apply to cases in which the obstruction presented itself alike in the pulmonary and systemic capillaries, it seems directly negatived by the fact, that many substances which raise an obstruction in the pulmonary capillaries allow a perfectly free passage in the systemic, whilst others which produce no effect on the movement of the blood through the lungs, exert a marked action on its flow through the general system. Nor does it seem to us that these results can be well explained, by attributing them to any influence exerted by the reagents on the walls of the capillaries, whereby a change was effected in the calibre of the tubes, the resisting power of their coats, or any other of their physical conditions; since here, too, we should have expected the same effect to be manifested alike in both systems of vessels, it being difficult to suppose that the contractile properties of the walls of the pulmonary and systemic capillaries should be so different, as to be oppositely affected by the same reagents. It seems to us much more probable, that we are to look for the source of these phenomena in the influence of these reagents on the chemical changes, which the blood ought to undergo in the two sets of capillaries respectively. We can easily comprehend that some of them might so far alter the physical and vital properties of the blood, as to impede *both* these sets of changes; whilst others might affect those of one kind only,—those which oppose the

oxygenating and decarbonizing process which goes on during the passage of blood through the lungs, having no injurious influence on the converse process which takes place in the capillaries, even possibly facilitating it; whilst those which oppose the latter, would naturally have no injurious influence on the former. And upon the principles to which we have elsewhere alluded, any reagent which modifies the normal changes between the blood and the tissues, will thereby affect the flow of blood through the capillaries.

The only class of facts brought forward by Mr. Blake, that now remains to be noticed, is that which relates to the action of various substances on the blood, as ascertained by its more obvious physical characters. It might have been expected that, under this point of view, we should have found a close connexion existing between the physical and chemical characters of the reagents employed, and the effects they produced. Such, however, is by no means the case; for in no other class of phenomena do we find these substances acting so completely in accordance with their isomorphous relations. Thus ammonia and potassa have little influence on the coagulation of the blood, unless injected in very large quantity; the silver and soda salts occasion imperfect coagulation; the salts of the magnesia group generally, except those of magnesia and lime, prevent the coagulation altogether, this being peculiarly the case with respect to the salts of iron, the injection of which causes the blood to remain so fluid after death as to allow the complete subsidence of the red particles; the salts of the platinum group also impair or prevent the coagulation; the phosphorus, arsenic, and antimony compounds render the coagulation imperfect; whilst the sulphuric and selenic acids, and the acids of the chlorine group, allow the coagulation to take place firmly. The salts of lead differ from those of baryta and strontia (which seem to have little action in this particular) in maintaining the blood fluid; and are thus found to possess another analogy with those of soda and silver. One fact recorded by Mr. Blake deserves special notice; namely, the continuance of the alkaline reaction of the blood, after as much as a drachm of glacial phosphoric acid had been injected into the veins; which, as Mr. Blake justly remarks, "affords a striking instance of the manner in which the chemical properties of a substance may be masked or changed when brought into contact with the living fluids and solids." And to this we may add, in the last place, that whilst the salts of the baryta group so rapidly and completely destroy the irritability of the heart, the voluntary muscles retain their contractility and continue in movement for some time after death; so that the body has been pushed along by the extension of the hind legs, a quarter of an hour after the heart had ceased to beat, a *point d'appui* being afforded to the feet.

We quite agree with Mr. Blake in his concluding remarks.

"It seems from the foregoing observations, that, independently of the interest which many of the facts may possess in a physiological point of view, a closer analysis of the action of these substances on the different organs of the body, and on the blood, affords additional proof of the existence of the law connecting the physiological action of these inorganic compounds with their isomorphous relations. Considering the imperfect state of our knowledge as regards the isomorphous relations of the elements, it is surprising that more exceptions to this law have not presented themselves in so extended a series of experiments; and I have but little doubt that those which have been remarked will disappear before a perfect

knowledge of the molecular properties of matter; on which, I think, physiological experiments are destined to throw light." (Op. cit., p. 76.)

We now come to one of the most important parts of the whole inquiry, *the tendency of the living body to free itself from poisonous agents.* This tendency cannot be disputed by any one who will attend to the facts which are familiar to the toxicologist; and it is one to which the preservation of life and the recovery from the effects of the poison must be attributed, in those numerous cases in which little or no direct remedial treatment can be applied, as well as in those which, from want of medical care, are left altogether to the restorative powers of nature. We are not now speaking of the class of poisons which act as *ferments,* and which are distinguished by the circumstance that the dose introduced is a matter of little consequence,—a small dose frequently acting as powerfully as a large one, though requiring more time to develope the results. But we are speaking of those which have a distinct and specific action on some organ or tissue, or on the blood itself, the degree of which action is proportioned to the quantity introduced. Of such we believe that it may be stated as a general proposition, that the system tends to free itself of them, provided *time* is allowed for it to do so; and that, when death ensues, the fatal result is to be attributed to the fact, that the disorganization of structure or disturbance of function is too rapid and violent to allow the depurating or eliminating processes to be set in efficient operation. When the dose is smaller, its effects are more or less rapidly evanescent; and the question naturally arises *why* this should be,—whether the substance is expelled from the system, or whether the system becomes tolerant of its continuance within it. The answer to this inquiry has, of course, a most important bearing on the question of the *modus operandi* of medicines, the effects of single doses of which are likewise generally transient; and also upon that of various morbid poisons, which exert a certain specific effect (such as that which we witness in exanthematous diseases), and then cease to manifest any further injurious influence.

It must be freely admitted that our information on this subject is as yet very scanty; but we have quite enough to serve as an efficient guide in further inquiries. We need scarcely remark upon the obvious fact, that many irritant poisons are at once got rid of by their action upon the walls of the alimentary canal; the purging and vomiting which they excite being the most effectual means possible of removing them from the system, and preventing them from exerting any further deleterious influence. And this frequently prevents the remote effects of the poison from being manifested; the substance being rejected from the alimentary canal before it has time to be absorbed. This is well seen in the case of a large proportion of the narcotico-irritant poisons; the symptoms of narcotism and those of irritation being usually in the inverse ratio to each other. It is seen also among the multiform results of the administration of arsenic. But it is when the poison has been actually absorbed into the current of the circulation, and is exerting its specific effect upon some remote organ, that the inquiry comes to be most interesting, and at the same time most difficult of solution. In some instances we can readily trace the channel by which the poison is eliminated; in other cases, we can only infer its removal; but there is reason to believe that, *in all instances in which its*

effects have ceased to manifest themselves, the poison has been removed from the body, either in the form in which it was introduced, or in some new state of combination, or (it may be) after an act of decomposition which has completely altered its original character. True it is that, after the administration of *repeated* doses of particular medicines, the system becomes tolerant of their effects; but we have no reason whatever to believe, that any *single* dose ever remains long in the body, or that the brief duration of its effects is attributable to anything else than to the really brief continuance of its period of operation.

The most certain evidence on this point is, of course, drawn from cases, in which the substance can be detected in the excretions by chemical tests; and these are now very numerous, the urine appearing to serve as the principal channel for elimination. Thus in one of Mr. Erichsen's experiments already referred to, a dose of forty grains of ferrocyanide of potassium having been given, this substance was detected in the urine in two minutes, and continued to present itself for some time; after the lapse of twenty-four hours, however, no trace of it could be discovered in that fluid. It is rather remarkable that, when the dose was reduced by one half, the salt was detectible in the urine for an equal and even a longer period of time, namely, twenty-eight hours. Here we may conclude that the urinary excretion was the chief, if not the sole, means of eliminating this substance from the blood; and this would seem to be the general fact in regard at least to saline substances, especially if they be such as excite the action of the kidney, e. g. nitrate and iodide of potassium. Wöhler found in the urine of dogs and horses iodine, sulphuret of potassium, sulpho-cyanide of potassium, the salts of nickel, the oxalic, tartaric, citric, malic, gallic, succinic, and benzoic acids. Orfila has added to this list a large number of metallic, alkaline, and earthy salts, and the mineral acids. A case reported by Dr. Letheby shows that even when sulphuric acid is taken in a concentrated form, it is liable to be absorbed and to become eliminated in the urine. A boy, aged nine years, swallowed an ounce of the acid and recovered in a few days; and for the first four days, a large quantity of sulphuric acid was passed with the urine. Arsenic and tartarised antimony have both been detected in the urine of persons to whom these substances have been administered; and Orfila thinks that this is the chief mode of its elimination. On the other hand, MM. Danger and Flandin think that it escapes by the liver, and by the pulmonary and cutaneous exhalations, having detected its presence in all of these situations. They have ascertained that the salts of copper, when taken as poisons, are more readily detected in the bronchial secretion than in the urine. The iodide of potassium has been detected also in the salivary, mammary, and cutaneous secretions; and, as a general fact, it would seem that soluble salts may be traced into the milk. With regard to organic compounds, the evidence of their elimination in an unchanged state is generally less satisfactory. We have seen, however, that several were detected by Mr. Erichsen in the urine; and that several others were detected by Wöhler; but these are, for the most part, of a kind rather distinguished by the stability of their composition, and by the comparative inertness of their operation upon the living body. Until the recent experiments of Flandin, no one seems to have unequivocally detected the presence of opium in any of the secretions; and we are not aware that any

one has succeeded in doing this for strychnia, or for any other of the alkaloids, which have the most powerful action on the nervous system. There can be no reasonable doubt, however, of their elimination from the body, in cases where life is prolonged and their effects pass off; and we shall presently point out how this is probably effected—first stopping to notice, however, the results of experiments on the *time* required for the elimination of arsenic by the kidneys, the records of which we find collected by Mr. Taylor.

"MM. Danger and Flandin found that in sheep to which a large dose of arsenic (half an ounce) had been given, the poison first appeared in the urine and fæces in about twenty-two hours; that it was still discoverable in the urine fifteen days after it had ceased to appear in the fæces; and that it was altogether lost in the excretions thirty-five days after the ingestion of the poison. When the animal was killed on the thirty-eighth day, not a trace of arsenic could be discovered in its body.—It is difficult to infer from the results obtained by such experiments the period required for the elimination of the poison from the human system. Even in animals there is a difference. Thus, in young and vigorous dogs, arsenic was completely eliminated in from six to ten days, while in sheep the period varied from thirty to thirty-seven days, and the flesh was then safely employed as food. M. Flandin assigns from one to two weeks for its complete disappearance. In other experiments it entirely disappeared from the body in three weeks after fifteen grains had been given. . . . . From the experiments of M. Bonjean, of Chambery, it would appear that arsenic was detected in the urine of a patient, who, *one month* before, had taken in twenty-four days only *three quarters of a grain* of arseniate of soda. The proportion thus eliminated may increase after the first day. M. Flandin's experiments on sheep establish this so far as the urine is concerned; but in no instance did they find the quantity of poison thus passed to exceed three-hundredths of a grain, even when the dose of arsenic was half an ounce." (pp. 23-4.)

It is probable that, in such cases as the last, a large quantity of the poison is rejected in the fæces without being absorbed at all; that which is passed off through the kidneys forming the chief part of the amount absorbed into the blood. Some further irregularities are noticed in the appendix.

"From the researches of MM. Millon and Laveran, it appears that there may be intermissions in the elimination of certain metallic poisons. In giving to their patients from one grain and a half to five grains of tartarized antimony, they remarked that it was eliminated by the urinary secretion, but in some instances slowly and unequally. They therefore examined the urine, not only several days after the introduction of the medicine, but for some days after it had ceased to appear in this secretion. They then found that its elimination underwent a marked intermission, and that, in a most unexpected way, it appeared to remain for a certain period fixed in the body. In two patients they detected traces of it twenty-four days after its administration. In the body of one who died of phthisis, they found antimony in the liver. In a third case, antimony was detected in the urine after twenty days; in two others after nineteen days; and in three others after sixteen, seventeen, and eighteen days respectively." (p. 822.)

There are a great many cases, however, in which poisons cannot be traced in the excretions, but in which their effects, when they are taken in a moderate dose, pass off so completely, that there can be no doubt of their being no longer present, as such, in the system; and the poisons of this class are of a nature and composition which renders them susceptible of change, when subjected to the influences which they will encounter in the living body, and more especially when exposed in a state of very fine division to the agency of oxygen. As a very familiar illustration of this fact,

we may advert to the effects of alcohol, taken in a moderate dose. These effects, though violent for a time, are well known to be transient. The state of excitement often ceases almost as suddenly as does that produced by the inhalation of nitrous oxide. We have heard sailors say that they always know at once "when the grog is out of them." Even if the dose be large enough to produce insensibility, this state passes off, and spontaneous recovery occurs, provided that the respiratory movements be not interfered with. Now it is rare to find alcohol in any of the excretions; and the question naturally arises, why its effects should cease, and what has become of it. This question can be answered without much difficulty in the present case. The alcohol is gradually eliminated by the respiratory process, its carbon and hydrogen uniting with the oxygen taken into the lungs, and forming carbonic acid and water which are carried off by expiration. There can be no reasonable doubt that this is the fact regarding alcohol; and one of the most remarkable proofs of it is the manner in which large doses of spirit can be borne without producing excitement, when it is subjected with peculiar rapidity to the combustive process,—as during exposure to severe cold, during great muscular exertion, or in the state of exhaustion from fever or other wasting diseases, when all other combustive material has been burned up, and the proper temperature of the body can only be maintained by the repeated administration of alcohol. Here, then, we have sufficient evidence that although alcohol can rarely be detected, as such, in the excretions, it is not the less effectually eliminated from the body by the excreting processes; and we can scarcely refuse to admit the same explanation in regard to the parallel phenomena which present themselves in other cases of poisoning, —as those induced by opium, strychnia, prussic acid, &c. There can be no reasonable doubt that these poisons usually act by absorption into the blood; and yet it speedily becomes very difficult to trace them in the circulating fluid. Thus all chemists seem to be agreed that morphia, so soon as it is absorbed, undergoes some change which renders it undetectible by the ordinary chemical tests; and it would seem that prussic acid is rapidly altered in the living body, for Dr. Lonsdale found that its odour could not be perceived in the blood or in the cavities when life was prolonged beyond *fifteen minutes*, although, when death took place within a shorter time, the poison might be detected in the body by the odour for eight or nine days afterwards. Numerous other instances might be quoted, in which organic substances are decomposed within the body and are carried off by the respiratory process. We know this to be the case with regard to farinaceous, saccharine, and oleaginous matters; we know it to be true also of the vegetable acids, unless they be ingested in unusual quantity; and there would seem to be a continual formation of lactic acid in the system, most of which is decomposed and eliminated in this manner. There is no difficulty, then, in understanding how the same process may be the most effectual means of getting rid of substances of a much less stable composition; or why the narcotic effects of a dose of opium, for example, should be as limited in their duration as are the calorific powers of a pound of fat.

That the cessation of action of poisonous organic compounds, like that of mineral substances, depends on their elimination from the system, is further apparent from the very remarkable series of facts attending the action of an intoxicating fungus, the *Amanita muscaria*,

which is used by some of the inhabitants of the north-eastern parts of Asia, in the same manner as alcoholic liquors by other nations. The state of nervous excitement produced by it has many interesting peculiarities, which show that it possesses a very decided and specific effect in *exaggerating* the normal actions. Thus it is said, that if a person under its influence wishes to step over a straw or small stick, he takes a stride or a jump sufficient to clear the trunk of a tree; a talkative person cannot keep silence or secrets; and one fond of music is perpetually singing. These effects, however, like those of other excitants, have a limited duration; and a man who is thus intoxicated on one day will "sleep himself sober" by the next. Now we have distinct evidence in this case, that the active principle is carried out of the system, not by being decomposed and burned off like alcohol, but by being eliminated with little or no change by the kidneys. For although no one has yet succeeded in proving its existence in the urine by any chemical reagents, the properties which it imparts to that secretion constitute ample evidence of its presence there. If a man who has been sobered by sleep (or rather by the elimination of the poison *during* sleep) take a cup of his urine the next morning, he will be more powerfully intoxicated than he was the preceding day; and we are told that it is not uncommon for confirmed drunkards to preserve their urine as a precious store in the event of a scarcity of the fungus. We are further assured that, by a repetition of the same act, the intoxicating effect may be kept up for a week or more; showing that the use of the same substance, over and over again, has the same effect as the introduction of a fresh dose. And this is true, not merely as regards each single individual, but also with respect to the transmission of the agent from one individual to another; for we are assured that if a second person drink the urine of the first, the third that of the second, and so on, the intoxication may be propagated through a party of five,—perhaps unlimitedly. Hence it can scarcely be questioned that the active principle of the Amanita passes either unchanged, or in some state of combination that does not affect its properties, into the urine; and it is further obvious, that its elimination by the urine is the cause of the cessation of its peculiar influence upon the nervous system. Dr. Letheby has obtained results of the same order in regard to opium, belladonna, hemlock, aconite, &c., the elimination of which by the urine was proved by the production of the characteristic effects of these poisons, when that fluid was administered to other animals.

There are other cases in which it would appear that organic substances may undergo such changes within the system, as are not destructive of their character as proximate principles, but produce a considerable alteration of their properties; thus benzoic acid is said to be eliminated in the urine in the form of hippuric acid; and the inhalation of the vapour of oil of turpentine very speedily gives to the urine the odour of violets, which must be derived from some new volatile compound produced within the body, to the formation of which the vapour imbibed into the circulation essentially contributes.

Thus we see that, as a general rule, the system tends to get rid of any substances introduced into it, of which it cannot make advantageous use, or which are positively noxious. But, in order that it may do so, *time* must be allowed; and where death results from the ingestion of a poison,

it is either due to the suddenness and violence of its action, or, in slower cases, to the persistence of some morbid process to which it has given rise. We cannot have a better example than that furnished by the action of sulphuric acid applied to the animal tissues; its disorganizing effects are irresistible; and these are usually severe enough to cause speedy death, either by asphyxia if the poison should have reached the larynx, or by the depression of the heart's action occasioned by extensive lesion of the stomach, or by the more remote effects which Mr. Blake's experiments show that its introduction into the circulation will produce on the respiratory process. But if these causes do not act with sufficient power to produce a primarily fatal result, the poison, as we have seen, is gradually eliminated from the system, and complete recovery may take place. But it not unfrequently happens that death takes place as a secondary effect of the local injury, long after the complete elimination of the poison may be presumed to have been effected; just as it may do from a purely mechanical injury. In these cases, the fatal result may be postponed for weeks or months; thus we well remember a case which occurred several years ago in the Middlesex Hospital, in which a young woman died after an interval of nearly eleven months; the lining membrane of the œsophagus never having recovered its healthy condition, the whole canal becoming so much contracted and so irritable as not to allow the passage of nutriment, and death resulting at last from exhaustion.

Many poisons, however, which have a most powerful *immediate* action, have no secondary effect; in other words, if they do not kill at once, they do not kill at all. Thus we are not aware that death has ever occurred from a dose of prussic acid at a longer interval than an hour. If the dose be not sufficiently powerful to check the vital functions within this period, it becomes ineffectual, probably because it undergoes decomposition. Thus in a case reported by Mr. T. Taylor, a stout healthy man swallowed a dose equivalent to nine tenths of a grain of the anhydrous acid; he remained insensible for four hours, at the end of which time he vomited, and began to recover; and the vomited matters had no odour of the acid. In the case of strychnia, the symptoms do not come on so rapidly, but they do not last very long; so that, if life be prolonged for five or six hours, recovery will probably take place. When a poisonous dose of opium has been taken, the results may not manifest themselves for several hours, and the fatal coma may be considerably postponed by causes which it seems difficult to understand; still the general rule is, that if the patient survive for twenty, or twenty-four hours, he is safe.

Let us now briefly turn to the consideration of the *remedial means* which we have it in our power to exert in cases of poisoning. Of course we can only touch upon the more *general* curative indications; our particular object being to show the bearing of toxicological principles upon the practice of medicine. Our first object is obviously either to get rid of the poison from the alimentary canal, or to render it inert, if it remain there, by the administration of antidotes; the one or the other course being preferred, according to circumstances. In a case of narcotic poisoning, our great object is to prevent absorption; we have not the power of rendering the substance inert whilst it remains in the alimentary canal; and our business is, therefore, to remove it as speedily as possible. In a case of poisoning by a mineral acid, on the other hand, the remote

effects are but little to be dreaded in comparison with the local; and it is our object to check these as rapidly as possible by some neutralizing substance.

When absorption has once taken place, and the poison is exerting its baneful influence on the nervous system, the heart, or any other organ which it may chiefly affect, we can do little or nothing in the way of counteracting its effects by direct antidotes. But we are not, on that account, to yield the case to nature, for treatment may often do much. Our treatment will be best directed, however, by studying the course of the symptoms, ascertaining the particular organs on which the poison operates, and carefully observing the means by which nature effects a cure when the fatal result is avoided. For although we may not be able to neutralize a poison by its *chemical* antidote, we may counteract its effects by a remedy of an opposite character, or by what we may term its *vital* antidote. Thus the sedative influence of prussic acid may be best counteracted by diffusible stimulants, especially ammonia, and by the cold affusion; this last appearing to have a special effect in relieving the tetanic fixation of the respiratory muscles, which is usually the immediate cause of death. In fatal cases of narcotic poisoning, the immediate cause of death is the cessation of the respiratory movements, from paralysis of the portion of the nervous system on which they are dependent; and our treatment, therefore (after we have done all we can for the removal of the poison from the alimentary canal, so as to prevent the absorption of any additional quantity), must be specially directed towards sustaining the respiratory process, whilst we also endeavour to prevent the whole nervous system from sinking into a state of lethargy. Here it is that the effect of *time* becomes so striking. Every hour during which we can keep the patient alive, is a positive step gained, even though no decided amendment should display itself; for during every hour, as we have shown reason to believe, a certain quantity of the poison is being eliminated, and progress is thus made towards its final removal. The efficient maintenance of the respiratory process thus comes to be of the first importance; not merely as being indispensable to the continuance of the circulation and other functions, but also as being probably the chief means by which the poison is eliminated from the body.

But, in endeavouring to neutralize a poison, or to antagonise its effects on the functions, we are not doing by any means all that nature points out to us for the relief of the system which is suffering under its influence. We should also aid, so far as we are able, in its elimination from the body; by promoting those excreting processes, which are the ordinary channels of its removal; and this more especially, when the poison is one that can exert a powerful secondary effect at a period long subject to its ingestion. This part of the treatment has not, we think, been sufficiently studied; but it must be practised with a caution which is not required in the application of the corresponding principle in medicine; since, as Mr. Taylor has pointed out (p. 334), in regard to Orfila's proposal to employ diuretics in the case of arsenical poisoning, if any of the poison be remaining in the alimentary canal, the exhibition of such remedies will favour its absorption into the circulating current. Where the poison is of a *septic* kind, producing a morbid change in the animal fluids and solids by a process analogous to fermentation,—we usually find diarrhœa to be one of the

symptoms; and on this account such poisons have been ranked as irritants, although their agency is of a very different character. Thus when meat, sausages, eggs, cheese, &c., have acquired poisonous properties by decomposition, it seems highly probable that they act as ferments; the blood itself being the tissue (if so we may term it) on which their specific influence is exerted. The diarrhœa which they generally excite, appears to us to be a salutary effort; tending, in the first place, to get rid of whatever noxious matters may be still remaining in the alimentary canal; and, in the second, to eliminate from the blood the *materies morbi* which may have been generated there, by the excreting powers of the glandular apparatus lining the intestinal tube. Hence this diarrhœa should be rather encouraged than checked, so long as there is reason to believe the blood to be contaminated, and the discharge does not itself become a source of too great depression. Thus, the presence in the blood of a poison derived from unwholesome food is not unfrequently indicated by urticaria; and where this is the case, we almost always find headache to prevail, with depression of spirits, want of appetite, and other symptoms of general disturbance; until an attack of diarrhœa or a dose of purgative medicine restores the blood to its normal condition by the elimination of morbific matter.

We feel that we may trust to the good sense of our readers, to make their own practical application of these views. They have no other aim than to show that, in the study and treatment of *natural* disease, we may derive important assistance from the data afforded us by what we may term *artificial* disease. The train of symptoms induced by the ingestion of a dose of opium, is as much a *disease*, in the philosophical sense of the term, as is that which results from the accumulation of urea in the blood, and which so closely resembles that of narcotic poisoning, as to have been mistaken for it. No one has any hesitation in speaking of hydrophobia as a *disease* resulting from the introduction of a poison into the blood; and whilst, on the one hand, its symptoms are closely related to those of tetanus and other spasmodic diseases, the mode in which the symptoms are generated, must be placed in the same category with the introduction of a paludal miasm, the fomites of an exanthem, or the contagious poison of erysipelas; since in all these cases, it is the blood which is primarily contaminated by the introduction of a morbific cause from without. Yet, again, hydrophobia is commonly treated of in toxicological works like the present; the poison of a rabid animal being ranked between that of venomous serpents, insects, &c., and that of glanders, putrescent or diseased corpses, &c. In fact, all those diseases in which the disordered condition is dependent upon the presence of a *materies morbi* in the blood must be treated on the very same principles as cases of ordinary poisoning; for it is of no consequence whether the poison have been introduced from without, or whether it be generated within the body. The blood is as much poisoned by the retention of its own carbonic acid or its own urea, when any circumstances prevent their elimination, as it would be by the introduction of carbonic acid into the lungs of the animal, or by the injection of urea into its veins. The chief difference amongst the agents which have been termed "morbid poisons," lies between those which are recognisable by chemical tests, and have a certain definite action, the intensity of which is strictly proportionate to the quantity that is in operation; and those which, though not recognisable by chemical tests, may be presumed

to act as ferments upon the blood, exercising their noxious influence on the system at large through the changes they produce in its constitution, and their potency rather depending upon the susceptibility of the blood to their peculiar influence, than upon the quantity that may be brought to act upon it. The former of these classes is that which most corresponds with ordinary poisons; we have at present, however, but few *isolable* matters to rank under it; and these consist for the most part of the substances ordinarily generated in the system, and continually excreted from it, which are noxious or poisonous if not eliminated,—such as urea, uric acid, biliary matter, carbonic acid, and lactic acid. There are probably, however, very many more; such as oxalic acid, the poison of lepra, psoriasis, &c. The latter includes those which give rise to what are now commonly termed *zymotic* diseases; as well as some others in which the introduction of the poison is a matter of more certainty, since they can be propagated by inoculation, such as hydrophobia, syphilis, glanders, pustule maligne, and the like. We believe that the merit of recognising the real nature of this second class, and of pointing out that the principles on which they should be treated are the same as those by which we are guided in the treatment of a case of poisoning, belongs to the late Dr. Robert Williams, whose treatise on 'Morbid Poisons'* we regard as having in a very remarkable degree anticipated the pathological views now prevalent, and as being a work of sterling value, whose merits were not appreciated at the time of publication, simply because they were in advance of the then prevalent tone of thought.

That the existence of a *materies morbi* in the blood may often be predicated from the *symmetrical* nature of the disordered action induced by it, was pointed out by Dr. William Budd, in a paper read before the Medico-Chirurgical Society in the same year with the publication of Dr. Williams's second volume. As, however, we have so recently directed the attention of our readers to this point (vol. i, p. 405), we need not again dwell upon it here.

Now in considering the principles of treatment of the two classes of poison-diseases (if we may so term them) which we have endeavoured to establish, we shall find that, with much that is common to both, there are some essential points of difference. In the first class there is a continual *new generation* of the poison within the system; and our first object is, therefore, to check its production, so far as this may be possible. This principle is constantly being acted on in the dietetic and regiminal treatment of the lithic, lactic, and oxalic acid diatheses, of lepra and psoriasis, &c. Secondly, we endeavour to destroy or neutralize the poison, if we have any remedies that possess this kind of action upon it. There are few instances, however, in which we have the power of so directly antagonising the poison; perhaps the curative influence of arsenic in some of the chronic skin diseases is one of our best examples of a real antidote. Thirdly, where we cannot thus destroy the poison, we must endeavour to moderate its action on the system as much as possible; this is the rationale of the antiphlogistic treatment of rheumatic inflammation. But fourthly, our main object must be to eliminate the poison from the system as rapidly as possible by the various channels of excretion; acting upon

* Vol. i, published in 1836; vol. ii, in 1841.

these by remedies which will increase their activity, or which will so alter the condition of the noxious matter as to enable it to be more readily drawn off. We believe that the judgment of the well-informed physician, in the treatment of diseases of this class, is more shown in his discriminative selection of the best means of thus aiding Nature, than in any more apparently heroic measures; and that a candid review of the most approved systems of treatment for the class of diseases on which we have been dwelling, will show that their efficacy depends upon the degree in which they harmonise with the indications which we have pointed out.

In our second class of poison-diseases, consisting of those in which the poison is introduced *ab extra*, the course of the morbid phenomena produced by it is usually more definite and specific, and its duration more limited; because there is no fresh source from which a new supply of the poison is continually arising; and its operation ceases, therefore, as soon as it is entirely eliminated from the system. But there is this peculiarity in the action of many of the poisons in question,—that they have the power of *multiplying* themselves within the body; thus, for example, when smallpox has been communicated by the inoculation of an excessively minute portion of the virus, hundreds or thousands of pustules are generated, each of them charged with a poison equally potent with that from which they originated. This multiplication may either be due to an action resembling fermentation; or to a process of cell-growth, like that concerned in the increase of yeast. It is impossible to say with certainty to which kind of agency this multiplication is due; but we are disposed to think that the former is the more general, if not the sole, method. The phenomena of poisoning by the bite of venomous serpents, from which death takes place in small animals within a few minutes, and in man within an hour (if the quantity and intensity of the poison have been sufficient), with symptoms referable to the loss of the vitality of the blood, appear to us inexplicable, on the idea that any process of cell-growth can be concerned in them; whilst they harmonise well with the results which we might anticipate from a powerful ferment. And between this, which we may regard as an extreme case, and the exanthemata, typhoid fever, and the like, there may be shown to be a series of poison-diseases so connected together, that we can scarcely refer them to any other than a common order of causation. In some of these diseases we find that the change in the qualities of the blood produced by the introduction of the poison, is such as to give it a morbid action on certain organs or tissues only; their phenomena in this respect corresponding closely with those of ordinary poisons. Such may be said of hydrophobia, vaccinia, gonorrhœa, primary syphilis, &c., in which the general functions of the body are only disturbed *through* the local disorder. But in other cases we find that the contamination of the blood is such as to produce more or less disturbance in all the functions; but a special determination to one organ or system may still be a characteristic of the disease, as to the skin in the exanthemata; or it may result from the predisposition afforded by the patient's system, as was remarkably seen in the great variety of local affections that manifested themselves in influenza.

Now in nearly all these diseases, there is a natural tendency to the self-elimination of the poison, provided its action be not too rapid and violent, and provided the excreting organs be in full and healthy operation. Thus

in the case of syphilis, we find a large proportion of individuals affected with primary sores getting well under simple regiminal treatment, and never suffering from the remoter effects of the poison. The question between the mercurialists and the non-mercurialists is not now whether syphilis may get well without mercury, for this has been long since decided in the affirmative; but whether the poison may not be more rapidly and more certainly neutralized or eliminated by the assistance of mercury than without it. That there is a tendency to spontaneous elimination of the poison in fevers, the exanthemata, &c., is a doctrine now so generally admitted by intelligent pathologists, that we need not dwell upon it. And we may go further, and say that the local affections which present themselves in these disorders frequently appear to be the results of the efforts at elimination by the organs in which they manifest themselves. We have elsewhere adverted (p. 137) to the affection of the intestinal glandulæ, which seems to result from the exercise of their proper function on noxious materials. And the opinion is gaining ground that in the exanthemata, the cutaneous surface is really the special organ for the removal of the morbid matter from the blood, its disordered state being incident to this operation. It is a very old observation that the suppression or repression of the eruption is usually accompanied with severe constitutional disturbance, which abates when the eruption has fully developed itself. The following remarks of Dr. W. Budd, in the essay already referred to, upon the pathology of Lepra, appear to us equally applicable to the more acute forms of disorder we are now considering.

"The appearance of the eruption is almost invariably preceded, for many days, by severe symptoms of general disorder. The eruption at length comes out, and the general disorder at once subsides. Now if we seek from analogy a rational interpretation of these facts, there will scarcely be a doubt that accumulation of the lepra-matter in the blood is the cause of the general symptoms. So long as this matter accumulates, and continues to circulate in mass with the blood, it is free to set up disorder in the system at large; but as soon as it is appropriated by the skin, the general disorder at once subsides."

We must not forget, however, that in the exanthemata, the check given to the normal functions of the skin by the morbid action thus induced in it, becomes of itself a cause of further constitutional disturbance; this is especially the case in smallpox, when the cutaneous surface is crowded with pustules,—a condition which may of itself prove fatal; just as when the skin of an animal is plastered over with an unctuous substance through which neither gas nor vapour can pass.

Whatever may be thought of these theoretical notions, we believe that all intelligent practitioners are now agreed upon the principles of treatment of zymotic diseases; and this practical agreement, as Dr. Williams has pointed out, serves as an important confirmation of the pathological ideas founded upon their symptoms and course. For if they result from the action of a poison on the blood, and this poison be of the nature of a ferment, we can have little hope of neutralizing that action by any antidotes; our main trust must be in the self-restorative powers of the system; and our duty is rather to watch for indications, and to act on them when action is called for, than to set up any new process which may interfere with the salutary operations of nature. That such is our best course in the treatment of fevers, influenza, and the exanthemata, we could prove by

citations from the first medical authorities of the day. We have so recently alluded to the case of fever (vol. i, p. 318), that we need not again advert more particularly to it. In regard to influenza, let Dr. Holland speak.

"Any remarks on the medical treatment of influenza must be prefaced by the consideration that we have here a disease depending on a specific cause, to meet which we possess no specific remedy. We are notoriously unprovided with any means to resist the access of this cause; or to remove it from the body, when received and in action there. Nor is there any explicit proof that we have the power of shortening the period of its action, or of altering its course and direction as regards particular parts. What alone can be effected in our present knowledge is to watch over the symptoms severally; to mitigate their excess; to promote a healthy and sufficient state of the natural excretions, and to obviate injury to any particular organ or function." (Medical Notes and Reflections, 2d ed., p. 216.)

That these remarks, written with reference to the epidemic influenzas of 1831 and 1837, are equally applicable to the severe epidemic with which we have been recently visited, will be, we think, within the experience of every one who has been called upon to treat it. We could scarcely point to a more remarkable example of the restorative powers of the system, than is afforded by the general history of this disease. Its invasion was usually very sudden, and the symptoms frequently alarming; an extreme general depression being probably the most uniform result of the influence of the poison, and the seat of the local affection being usually determined by predisposition. In by far the greater number of cases, where no other remedial measures were adopted than simple rest and abstinence, with mild evacuants, the disease ran its course within a short period; and the rapidity and completeness of the recovery were scarcely less remarkable than the violence of the morbid action. And where a fatal result ensued, it was much more to be attributed to the unfavorable nature of the concurrent causes,—such as predisposition to pulmonary or other diseases, advanced age, general debility, unwholesome habitations, &c.—than to the simple violence of the influenza-poison.

In the treatment of poison-diseases of this class, therefore, our main object must be to aid the efforts of the system in the elimination of the poison by the judicious use of evacuants, to support its powers when there seems reason to fear that they may fail before this object is accomplished, and to antagonise (so far as we have the means of doing so) any local disorder which may threaten to aggravate the general mischief. Here theory and experience are in complete accordance.

We trust that we shall not be misunderstood, as to the views which we thus endeavoured to lay before our readers. Nothing is further from our intention than to recommend a *do-nothing* practice. But, on the other hand, every observer of disease must have seen reason to condemn a *meddlesome* practice. How, then, are we to discriminate between the class of cases, in which we are merely to watch for indications whereon to act, whilst Nature herself cures the disease, and those in which we are called on to exert all the resources in our power for combating the disease itself? We believe that the first great step in this most important inquiry consists in the due discrimination between the diseases resulting from the presence of a *materies morbi* in the blood, and those resulting from a morbid

action, in which the solids are at least as much primarily concerned as the fluids. Of the former class, we have now said enough. In the latter, to which the phlegmasiæ belong, we have by no means the same tendency to spontaneous cure; the mutual reaction of the solids and fluids often operates to keep up the morbid action when it is once established; and remedial measures have frequently a direct and almost specific influence in checking or abating it. Here, then, is the demand for active measures, directed towards the subduing of the disease itself. The well-informed physician who looks calmly on whilst a fever runs its course, and shrinks from interfering with the salutary processes by which nature frees the system from the poison and restores it to healthful action, may be the very one who most boldly and successfully attacks a severe case of pneumonia or peritonitis. To apply the expectant system to the latter, would be as blameable as to treat the former on heroic principles.

We have endeavoured, in the preceding pages, to give formal expression to views which we believe to be current amongst many intelligent practitioners, but which have not yet, so far as we are aware, been distinctly or fully stated. We claim no novelty for ourselves in regard to them; and it would be difficult to say with whom they have originated. They are, in fact, a part of the great stream of public opinion, which on this subject, as on many others, is steadily but silently bearing us onwards; its progress only becoming apparent when we are led to compare our present position with our ancient landmarks.

In our next Number, we shall consider Mr. Taylor's work, of whose general merits we have already recorded our opinion (Vol. I, p. 522), as a treatise on practical toxicology.

Art. XI.

Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation. By Peyton Blakiston, M.D., F.R.S., Physician to the Birmingham General Hospital, &c.—*London,* 1848. 8vo, pp. 368.

Had Dr. Blakiston's volume been of a less solid character than it is, and less fitted to outlive the hour of its appearance, we should have regretted the length of time it has remained unnoticed in this journal; as it is, our continued silence (the result of accident, not of choice) can do the author no mischief, and therefore, on his account, gives us no pain. That we have formed a high estimate of his labours, may be gathered from this statement; but the higher that estimate, the more disposed, we fear, we shall be to criticise (perhaps, he will say, to carp at) certain views and doctrines which appear to us not altogether accordant with sound experience; the errors of A. B. may be allowed to circulate with impunity; the presumed slips of Dr. Blakiston are too attractively set forth not to invite proselytism, and are *pro tanto* dangerous.

Concerning the first of the series of chapters, into which Dr. Blakiston's work is divided, we have nothing particular to say. The chapter in question proposes to describe in a number of propositions the "Properties of Sound."

Chapter II is devoted to the consideration of "Sounds elicited by Percussion." The subject is dismissed in four pages, and appears to us to be handled with less attempt at precision of language and method, than its importance calls for. The simpler facts of percussion practice are sufficiently well known to stand in no need of repetition in a non-systematic work of the present class; and, concerning the difficult points of the matter, Dr. Blakiston maintains perfect silence. Thus, he mentions the familiar fact, that the percussion-sound is duller over the right, than the left, pectoralis muscle of persons who labour hard with the right arm; but makes no allusion to the curious circumstance, that, in a certain proportion of instances, where the parietes of the chest are perfectly identical on both sides, and where (as post-mortem examination proves) both lungs are perfectly free, both from consolidation-changes and from rarefaction-changes, the right apex-regions give out a harder and a less clear note than the left. We believe that we have in numerous instances succeeded in substantiating, and in demonstrating to others, the existence of this peculiarity; on its importance in connexion with the diagnosis of incipient changes at the apices, it is needless to insist. We are unable to explain it,—but the matter of clinical interest is, that the occasional existence of a *natural* deficiency of tone in the spots referred to should be known and allowed for.

Dr. Blakiston perpetuates a serious error (as we conceive it) of many of his predecessors, by limiting the dullness of the heart in the state of health to "a space varying from the size of a shilling to that of a half-crown piece."* The statement is doubly at variance with fact: the *form* of the heart's dullness is not circular, but rudely triangular; the *extent* of the heart's dullness is materially greater than that set down by the author.

Dr. Blakiston's four pages are singularly barren of information regarding alterations of *quality* of percussion-sound,—a fact the more strange, perhaps, as their author leads us to infer from certain of his aphoristic enunciations on the properties of sound, that he (unlike the Professors of Natural Philosophy of the day) is pretty well satisfied as to the material condition regulating the quality of tones. Of the wooden, tubular, and amphoric resonance nothing is said; and we are mysteriously told that a "sound has sometimes been elicited by percussion, which has been styled the *bruit de pot fêlé*," as though the production of this sound ranked among the rarest of phenomena, and occurred as an affair of chance, instead of invariably arising, whenever a certain set of well-known physical conditions coexists. Dr. Blakiston rejects the notion that this peculiar quality of sound arises from the forcible expulsion of air from an excavation into the air-tubes,—a notion strongly advocated by Dr. Walshe, in his work on Physical Diagnosis. Our author objects that "if this were the true explanation of the sound, we should not find it appearing and disappearing in the same individual, which may not unfrequently be observed." And why should we not? Why may not the condition of the bronchial openings vary? Why may not vibrating shreds of inspissated sputa, or even pseudo-membrane, pend from a bronchial mouth at one time, and be absent at another? Why may not the manner

* Bouillaud and other French writers, who exhibit perpetual inacquaintance with the true positions of the inner edges of the lungs, have given currency to this imaginary description of natural dullness in the cardiac region.

of the blow given by the percussor, its strength, and the angle at which the chest is struck, vary,—and these conditions are well known to modify the character, and even regulate the existence, of the phenomenon in question, so much so, that A will produce the special sound, B the next instant fail, and A, returning to the charge, again succeed? Nay more, the air-expulsion theory seems the only one accordant with the two following facts (which we hereby present gratuitously to Dr. Walshe for his next edition, or to Dr. Blakiston for his, provided he, in the meanwhile, repent and be converted). First, the cracked-metal note may, by careful percussion, be produced from a cavity, which auscultation proves to be at the moment free from fluids. Secondly, the tubular, amphoric, or amphoro-tympanitic note, which is almost invariably producible at the upper and antero-internal part of the affected side, at a certain period of the progress of pleuritic effusion, may sometimes by careful manipulation be temporarily rendered cracked-metal in its quality. And how? Simply by percussing in such manner as is likely to force air suddenly and sharply from the subjacent bronchial tubes, which are pushed unduly forward as a result of the disease; here, assuredly, there are no air and fluid to mix, to be jogged and to "splash."

In Chapter III, the discourse falls upon the auscultation of the sounds of respiration. Speaking of the proportional length of the expiratory and inspiratory sounds, Dr. Blakiston refers to the "still vexed" ratio of two to ten; and observes, "practical auscultators will readily admit that in the great majority of persons no sound, or at most only a short puff, is heard during expiration." No; *all* "practical" auscultators assuredly will not admit this,—for the very man (Fournet) who suggested the above ratio, was peculiarly deserving of that title in the highest sense of the word. Besides we, for our own parts, believe that M. Fournet sins rather by valuing expiration too low, than (as Dr. Blakiston would signify) too high. But we freely confess that we think the point requires further inquiry.

The author turns to the "Formation of the natural respiratory sound," as the next subject of comment; and argues, not very happily, as it seems to us, for the exclusion of certain possible causes of sound. Thus he would reject the "gliding of the lungs on the ribs during their expansion and retraction," as a cause of respiration-sound, *because* this gliding takes place in inspiration and in expiration, whereas sound is, in most cases, inaudible during the latter act. How comes it then, that when sound is really engendered by the gliding of the two pleural laminæ on each other (that is, in the varieties of friction-sound), the sound may accompany both movements of respiration, or, as is frequent, be limited to inspiration, or, as is rare, attend expiration alone? We believe with Dr. Blakiston that the collision of the pleural surfaces is, in health, noiseless; but we found our belief on direct observation in men and the lower animals.

The commonly accredited notion, that the respiratory murmur is produced by the air entering and leaving the air-cells of the lungs, is rejected by Dr. Blakiston; his rejection is ingenious, but we question whether it is sound, and we are certain it is not convincing. We do not delay with the refutation of M. Beau's pharyngo-laryngeal theory,—a theory which, in point of fact, never could have, and never has had, supporters among

clinical observers. The doctrine professed by Dr. Blakiston is that "the pulmonary sound is principally formed by the air rushing through the smaller bronchial tubes." At this localization he arrives *per viam exclusionis* (as he conceives); there remains the excess of sound in inspiration, as compared with expiration, to be accounted for. But this part of the business is easy enough, for it appears that—"In a very ingenious paper, read by Dr. James Carson, of Liverpool, in 1841, it is stated to be the opinion of the writer, that the muscular fibres, which surround the smaller bronchial tubes, contract during inspiration for the purpose of narrowing the tubes, and thereby forcing the air up into their vesicular terminations. If such be the case, then an obstruction is offered to the entrance of the air, which exists only at the very commencement of its departure from the lungs, and ceases on the relaxation of the muscular fibres." The only point in the above quotation at which we should not (had we space to spare) be disposed to cavil, is the implied ingenuity of Dr. James Carson; this experimentalist comes of an ingenious stock, and as the parent was, even so is the son. But what numbers of ascertainable clinical facts are at variance with Dr. Blakiston's application of the Carsonian ingenuity! Take one. If bronchial muscular contraction be an element in the production of inspiration-sound, how comes it that when that bronchial muscular contraction is nearly carried to its extreme height, there is no true inspiration-sound at all? The clinical observer of true unadulterated spasmodic asthma knows that this is the fact. And he knows further, that when muscular spasm is relaxed for a moment by Laennec's well-known plan, then, momentarily, natural inspiration-sound may be heard.

Dr. Blakiston delights in establishing the mechanism of respiratory phenomena,—here is his notion concerning the crepitant rattle. "If tubes are made use of, of much less size than that of a crowquill, it will be seen that the bubbles do not burst till they reach the extremity of the tube during the time the air is forced into it from the mouth or Indian-rubber bottle. The sound formed by the bursting of these small bubbles at the extremity of the tubes, gives the ear no idea of moisture, but seems like a fine crackling. The exact sound is heard in the first stage of pneumonia, œdema of the lung, and in certain forms of pulmonary apoplexy; and here, as in the tube, it is confined to inspiration." Dr. Blakiston, taking this view of the matter, finds it necessary to set aside the doctrine of Dr. Walshe. But he seems to misunderstand this writer. Dr. Walshe does *not*, as Dr. Blakiston states, "attribute the crepitation to the unfolding of the vesicles, the sides of which, he supposes, stick together during expiration;"—but attempts to explain the production of the rhonchus in the following manner: "Its physical cause is the sudden and forcible expansion of the *parenchyma*, glued together, as it were, by the viscid exudation with which it is infiltrated. Each single crepitus or click would thus signify the expansion of a cell, and be produced by the unfolding of surrounding glutinous tissue necessary for that expansion." We confess we do not attach any great importance to the determination of the question of true *crepitation-mechanism*, considered in itself; but we do think that the notion of the *intra-parenchymatous* origin of the phenomenon (that is, its origin in a site where no air is present) is well worthy of consideration, from the light it throws on the generation of rhonchi within the pleura. That rhonchi, dry, crackling and bubbling, may in

truth be produced within the pleura, is a fact of which we have had numerous opportunities of satisfying ourselves, since our attention was first drawn to the subject in the writings of Dr. Walshe.

Dr. Blakiston enters into a disquisition concerning the respective advantages of the solid and the hollow stethoscope; and decides the question in favour of the former. To us it appears, that viewing this matter practically, much time and ink have been wasted in its consideration: the real point of importance seems to be this,—can one person be made, by means of one variety of stethoscope, to hear a certain sound or sounds, or to appreciate more fully the properties of this sound or sounds, which he fails to hear or to appreciate by means of another? And this question we are bound by experience to answer in the negative; insomuch that we have in our own persons grown as placidly indifferent to the announcement of alleged "improvements in the stethoscope," as mathematicians have long become to the reported success of new squarers of the circle.

The writer of this volume appears by his records of cases to hold all minute investigation of physical signs in particular horror; nay more, we find him (p. 59) laying claim to the gratitude of students, because in his pages "the different sounds of the chest have been simplified, and reduced to as small a number as possible." We have difficulty in conceiving how a man of the practical attainments of Dr. Blakiston can assume the promulgation of imperfect knowledge to constitute a merit. Let his principle be fully carried out, and we might bid a long good night to all advancement in the art of diagnosis. The student educated in his school, and *in verba magistri jurare paratus*, might fairly scoff at all distinctions hitherto made of liquid rhonchi;—why separate crepitant from cavernous rhonchus, might the simplifying philosopher urge; is there not a regular catenation between the two, by means of the mucous class? Probably Dr. Blakiston would shrink from this broad realization of his doctrine of simplicity; yet he seems to us to have made no trifling step in the same direction, when (p. 27) he speaks of the rhonchi heard in the first stage of pneumonia, and in œdema of the lung, as being exactly the same! We venture to affirm, that the higher order of students even would feel disposed to cavil at this "simplicity,"—a kind of simplicity which, it is evident, might lead to most dangerous errors in practice.

We have read with considerable satisfaction Dr. Blakiston's lucid analysis of the doctrines set forth concerning the heart's natural sounds. His inference as respects the systolic sound is, that it is at once muscular, valvular, and impulsive, and besides, in part caused "by the collision of the blood against the orifice of the aorta and pulmonary artery." Of the real play of the first three agencies in the production of the sound, no doubt can be entertained; the fourth (of much more dubious character) is insisted on by the author on the following grounds:

"A bellows-sound may be engendered (in disease), by the blood thrown into vibration by its particles forcibly striking the orifice of the aorta during the systole of the ventricles. Now, in certain cases, it requires only a very slight increase in the heart's action to produce a bellows-sound; hence, it may be concluded that there is a point, a little short of this, when the collision of the blood against the orifice of the aorta strengthens the systolic sound, and in such a case is an auxiliary to it."

Dr. Blakiston gives four chapters of his work to the subject of thoracic

aneurism; and these chapters contain information at once sound, well digested, and abundant. Speaking of the progress of such aneurism, he observes, that its precise situation, the state of its walls, and the nature of its contents, influence this most seriously. Aneurisms springing from the part of the aorta within the pericardium, or from the portion of the vessel compressed between the left bronchus and the diaphragm, usually run their course without producing serious results by concentric pressure. In the former instance, because they usually burst before they attain any great magnitude; in the latter, because even when acquiring great bulk, they only interfere with "the bases of the lungs, the apex of the heart, and the œsophagus." It seems scarcely wise (especially without explanation), to set down the œsophagus as a part that may be pressed on with comparative impunity. The portion of the aorta between the pericardium and the left bronchus is surrounded with so many important parts, that aneurisms implicating it "can hardly fail to interfere with the functions of some of them." Much, however, as the author notes, will depend even here on the direction in which the aneurism grows; if it spring from the anterior aspect of the vessel, and come forwards, the deep-seated parts are vastly safer than under the converse circumstances. This is a fact of much practical importance, and one which we have been in the habit of aphoristically expressing thus: the centrifugal and the centripetal pressure-effects of intra-thoracic aneurism are in an inverse ratio to each other.

As respects the walls of an aneurism, the attenuation (by dilatation) they undergo, is counterbalanced not unfrequently by an inflammatory process which causes thickening. But this thickening renders the walls stiff and unyielding, and unable to accommodate themselves to the parts they encroach on: if it be an antidote, it is likewise a bane. The common cases of pressure on the vena cava illustrate the influence of a thick and stiff sac; in a case (22) given by Dr. Blakiston, the sac yielded, and the vein was found lodged in a deep groove, and was thus protected from pressure; here there was no œdema.

Dr. Blakiston records five cases, in which imminent death from rupture of an aneurism was warded off by the occurrence of plugging of the orifice with coagulum; in one of these cases, life was preserved for many months after this had occurred. Such, too, was the case of the late Mr. Liston.

The causes of aneurism are submitted by Dr. Blakiston to a scrutiny which yields nothing novel. Diminution of elasticity and loss of substance are set down as the proximate causes of the disease—these changes being themselves the results of "deposition of horny patches, atheroma and calcareous concretions." These horny patches are the "plaques cartilagineuses" of Bizot (well known to be perfectly free from true cartilage, and not deserving the epithet of "horny" either, seeing that hitherto this term has been applied solely to epithelial products), and have been shown to originate in common exudation thrown out on the inner surface of the vessel. They do not encroach upon the middle coat, nor is it clear that they play any direct part in the generation of aneurism. Hence atheroma-deposit, and calcification of this, are the conditions of the vessel directly leading to aneurismal dilatation; and the remoter agencies, giving importance and activity to these conditions, are nervous

palpitation, hypertrophy of the left ventricle, age, and sex. Atheroma stands thus in a relationship of extreme closeness to aneurism; but whence comes atheroma? That its deposition depends upon what is conventionally termed a diathesis, we are as well satisfied as Dr. Blakiston appears to be; but we cannot perceive that the development of that diathesis is rendered more intelligible by the suggestion that "all circumstances which materially interfere with the process of digestion and assimilation, *may* produce the atheromatous diathesis." Where is the proof of this? Is the tabes mesenterica of childhood attended with atheromatous change? Have not full-grown adults daily "*indigested*" for years before their death, and yet been free from atheroma in their blood-vessels?

Inexpressibly more valuable than these loose speculations is a clinical analysis of thirty-seven cases of aneurism of the thoracic aorta, with which Dr. Blakiston next presents his readers. Much as has been written, much as has been thought, much as has been demonstrated, concerning the signs of thoracic aneurism, who is there that has not stood before a dilated aorta and doubted; who is there that has not seen and felt the eccentric signs of tumour, and yet *feared* there might be aneurism? For if aortic aneurism in some cases announce itself by more or less facile and unequivocal signs, in not a few it loves obscurity, and manages to betray the most habitual auscultators into totally overlooking *what is*, and ingeniously detecting *what is not*. Thanks, then, to Dr. Blakiston for another attempt to unmask this arch enemy to sure diagnosis,—even though the results, to which he conscientiously comes, are rather negative than positive.

Of the pulse? A distinct difference in width and force of the pulse at the wrists is well known to be an important sign of aortic aneurism, when it exists; but it may be so slight as to be of doubtful reality, or it may be absent altogether. Suppose it, however, to be present distinctly, the cause may be an intra-thoracic tumour, as well as an aneurism (a circumstance of which Dr. Blakiston makes no mention); and unless from collateral evidence, these two causes of difference of the radial pulses cannot be distinguished. Again, "in Case 38, the right pulse was very small, and a rasp-sound and purring thrill were discovered under the right clavicle, which signs were produced by a *narrowing of the origin of the arteria innominata by atheroma.*" Here would appear a new source of difficulty. But, upon referring to the narrative of the case, we do not find the slightest proof of the justness of the inference on which the above assertion is based. We find it simply stated, on the one hand, "when the woman was examined, the pulse was small," [which pulse?]: and, on the other, "the arteries *of the arms* were diminished in calibre by the deposition of atheroma in their middle coats." The innominata artery is not mentioned at all; and *both* arms were the subjects of arterial change. "There is another state of the pulse which may sometimes assist us in *preventing aneurism*, or detecting it at an early period of its development." A something, by the knowledge of which, assistance in the prevention of aneurism may be obtained! These are glad tidings of great joy,—to all fair seeming. But, alas, to seeming only; for this alleged important state, is simply "the sharpness and rapidity of stroke, which results from a diminution of elasticity of the coats of the arteries." Now, in the first

place, who has ever *proved* that such characters are caused by diminution of elasticity? In the second, does diminished elasticity of the radial arteries rank, as its necessary antecedent, aneurism of the aorta? In the third, admitting that such characters of the pulse are producible by loss of elasticity, they are certainly caused by other conditions also; and the idea of forthwith ascribing them on their discovery to aneurism, (coming or actually arrived) seem to us a most singular piece of clinical fancifulness. After this, with what colour of justice, can Dr. Blakiston declaim against the advocates of *minute* physical diagnosis?

"When a pulsation is both seen and felt over a circumscribed prominence in the chest, it has always, in my experience, denoted the existence of an aneurism." This inference is important, as coming from the analysis of thirty-seven cases; but our own observation has made us acquainted with two cases in which prominence and pulsation coexisted, and yet there was no aneurism. But pulsation, visible or palpable, is a much less common attendant on aortic aneurism than is generally imagined:

"Of thirty-two cases of thoracic aneurism, originating without the pericardium, there were only ten in which pulsation occurred immediately above or below the clavicles; and it is remarkable that pulsations in this situation were observed in but one of those cases in which the aneurismal tumour did not reach the surface of the chest at the time of examination."

Dr. Blakiston judiciously insists also on the converse source of fallacy,—the frequency of pulsation above the clavicles in cases of hypertrophy of the heart without aneurism.

In two cases, thrill existed before any elevation of surface had been produced, and proved a sign of great diagnostic value; so too, in our own practice have we been twice *led* to the diagnosis of dilatation (not sacculation) of the arch by well-marked local thrill, corroborated in its signification by more positive signs.

Speaking of the signs depending on pressure of the superior vena cava, Dr. Blakiston introduces a case of mediastinal cancer,—where

"The signs of a mediastinal tumour were unequivocal, but it was not supposed to be an aneurism, because although it was evidently in contact with the surface of the chest to the right of the sternum, and conveyed the sounds of the heart with unusual clearness to the ear, yet there was no trace of pulsation over that spot."

But, as Dr. Blakiston himself admits, the same deficiency of pulsation may exist over tumours really aneurismal, compressing the cava descendens, hence the distinction is fallacious.

The loud whistling sound accompanying respiration, and dependent on tracheal pressure, has sometimes been mistaken for the result of inflammatory constriction of the larynx; and tracheotomy has been performed for the relief of a supposed inflammation, which proved to be a mere pressure-effect of aneurism of the arch. The author, however, records a case showing that acute laryngitis may be caused by the irritative influence of such an aneurism.

We have been seriously disappointed by Dr. Blakiston's notice of Percussion as a means of diagnosis; he tells us that in one case only of the thirty-seven did it furnish a valuable sign of the disease. We cannot but feel persuaded that percussion was, if such be the fact, either neglected altogether, or carelessly and irreflectively practised in many instances:

"Where we find evidence of an existence of an aneurismal tumour, with little or no murmur, and at the same time we find the action of the heart to be strong, we may conclude that the aneurism is large and sacculated, and that it communicates with the aorta, either by a very large or a very small orifice, or else that it is, from some cause, very inelastic; a conclusion, however, which would be drawn from a consideration of the manner in which such sounds are produced, rather than from practical observation."

In these statements we concur, and desire to draw the reader's attention also to the announcement that "there is one quality of sound which I have never found but in aneurismal dilatation, and that is, a hollowness of tone."

Case XXVIII, narrated with deplorable conciseness (it is so deficient in detail as to the state of the coats of the aorta, for example, that we might, were we so disposed, in part question the correctness of the inference Dr. Blakiston draws from it), appears to support the idea (plausible enough considered *à priori*), that double strong murmur may be accompanied with circumscribed dullness, "near the upper part of the sternum," and be more intensely developed there than at the heart (though really depending not on aneurism but on disease of the aortic valves), *through the influence of solidified lung*. There is one single fact which would prevent error under these circumstances,—the clearness of the percussion-note on the middle line at the top of the sternum, seeing that a tuberculised apex does not encroach upon the mediastinum. We very much doubt, too, that the double murmur of diseased aortic valves would be rendered *louder* at the top of the sternum, than at the third left costal cartilage, through the sole intensifying influence of a solidified apex; calcareous deposition to a notable amount has always, in our experience, coexisted in such cases in the coats of the aorta.

The following observations are well worthy of attention.

"It has been shown that a diastolic valvular murmur can only be formed by aortic regurgitation, or a moderate amount of obstruction at the auriculo-ventricular orifices. In neither of these cases is the murmur carried to such a distance from the spot where it is engendered, as when it rises from aortic obstruction. Hence, a diastolic murmur, heard *very much* louder on either side of the upper part of the sternum, than at the præcordial region, is generally, if not always, characteristic of aneurism. Again, if it is heard in such a spot only, it is absolutely characteristic of this disease, because we can suppose no other state of the vessel which could give rise to it. And not only this, but it denotes the existence of sacculated aneurism, the elements for its production not existing in dilatation; and this whether it is accompanied by a systolic murmur or not."

Commenting on the deficiency of respiratory sound, produced by pressure on either bronchus, or a main division of either bronchus, the author refers to Case X, where

"The respiratory sound ceased in the upper part of the right side, on the patient attempting to place himself in an upright or recumbent posture, which showed the existence of a moveable tumour in front of the division of the right bronchus, which supplied the upper portion of the lung; and as nothing but an aneurism would satisfy these conditions, this sign was considered characteristic."

Why nothing but an *aneurism?* Why not a tumour of some other kind? Does Dr. Blakiston mean to affirm that tumours in the mediastinum of other kinds are less prone to be moveable than aneurism? If he does so as matter of his own personal *experience*,—the affirmation is of

importance, and may serve as a guide to diagnosis in certain cases of intrathoracic obstruction.

The general propositions to which the author's analysis of his thirty-seven cases leads him, are thus set forth by himself:—

"No diagnostic sign was furnished by the character of the pulse, or by the presence of pulsation [alone] above or below the clavicles.

"When a pulsation was seen and felt over a prominent spot in the chest, it indicated the presence of a sacculated or mixed aneurism.

"Purring thrill was only valuable as a sign of aneurism, in conjunction with other signs.

"A systolic murmur, heard at a distance from the heart, even though it were not heard at the præcordial region, only affords evidence of the existence of aneurism, when it was combined with other signs, denoting the existence of a circumscribed tumour.

"A double or diastolic murmur confined to the spot, at a distance from the præcordial region, denoted the existence of a sacculated aneurism.

"When a hollow murmur was heard, a dilated aneurism was present.

"The intensity of aneurismal murmur was in a great measure proportioned to the force of the heart's action.

"Aneurism, of both kinds, existed without the slightest trace of pulsation or murmur.

"Aneurisms arising within the sac of the pericardium, were not indicated during lifetime by any characteristic signs."

In regard to the treatment of thoracic aneurism, Dr. Blakiston strongly urges the importance of distinguishing cases of dilatation from those of sacculation: in the former, prevention of increase, or of rupture, is the object; in the latter, obliteration of the sac by deposition of fibrin. This distinction has been tacitly acknowledged in practice; but the writer is perhaps authorised in taking credit to himself for insisting more fully on the distinction than his *literary* predecessors.

Increased dilatation, or rupture of a dilated aneurism, may be accomplished by "strengthening the walls of the pouch, or by diminishing the force of the current of the blood, or by both means." The production of inflammation in the walls of the pouch (to the generation of lymph), is the only method "by which we can hope to strengthen those walls." This *possibly* might be promoted by the use of a highly stimulating food; but the "proceeding would, however, be very hazardous," and so is pronounced indiscreet, and unadvisable. The second indication is to be effected by venesection, low diet, sedatives, purgatives, and mental and bodily repose.

On the other hand, the rational method of treating sacculated and mixed aneurisms, would appear to consist in the use of sedatives and purgatives, of a moderate amount of nutritious food, rest of mind and body, and the application of cold to the surface of the chest. Bleeding, we believe, with the author, to be peculiarly contraindicated, from its effect of diminishing the ratio of the coagulable element of the blood;* and as to the depletory system of Valsalva, the reasons against its adoption (admitting it to be practicable), are numerous, backed as they are by all sound and impartial experience. Of the application of ice to the chest, we scarcely think so well as Dr. Blakiston; in many cases (as in two observed by himself), it is productive of intense pain, and its real influ-

* Why is the establishment of this fact accredited to Messrs. Becquerel and Rodier; when, as is well known, Magendie had long before them bled myriads of dogs to prove it?

ence in promoting coagulation may be doubted—we mean clinically, not physiologically.

But Dr. Blakiston is hardly true to himself in his speculative disquisition on the treatment of aneurism. Has he not, in an earlier part of his volume, bound, in an indissoluble nexus of cause and effect, atheroma and aneurism? And has he not further dwelt, in most complacent reverie, on the agencies producing the constitutional state requisite for the deposition of atheroma? Surely, then, in the consideration of the treatment of a given disease, the material textural change producing it should not be forgotten;—more especially as the extension of that textural change must have for effect the extension of that given disease. No, had Dr. Blakiston pushed his philosophy to its natural terminus, he had taught how atheroma-deposition may be speculatively controlled. And this is not critical cavil on our parts; plainly, the sound management of aneurism must, in ultimate analysis, be a dependence upon that of fat-precipitation in the arteries.

The next subject to which attention is drawn by the author, is "the progress and terminations of chronic heart diseases." Something more, as Dr. Blakiston justly intimates, is required for practical purposes, than the separate study of each of the organic alterations of the muscular and valvular structures of the heart, with the effects they each may have on the blood's circulation. He therefore proposes to examine the manner in which the health is impaired by the various derangements of the heart's action thus induced, and the steps by which a fatal termination is approached. Now, death may result from direct stoppage of the action of the heart, or from impediments offered to one or both of the circulations. Of the first of these modes of death, it is not the author's intent to speak; on the second he dwells largely. For, in the first place, he shows that existing theories on the subject are irreconcileable with facts daily observed; and in the next, produces two series of cases to establish a thesis of his own. His thesis is, that it is neither hypertrophy of the ventricles, as some suppose, nor disease of the orifices of the left side of the heart, as others maintain, which causes the congestive and dropsical evidences of obstructed circulation;—but that it is dilatation of the heart which plays the major part in causing systemic obstructions. It had already been taught by Dr. Hope, as our readers are aware, that dilatation was the real agent in generating these obstructions; but he supposed *attenuation, with consequent weakening* of the walls of the dilated ventricles to be their immediate cause; now, this explanation does not apply to those cases of frequent occurrence, in which hypertrophy is joined with dilatation. Andral endeavours to meet the difficulty by the supposition that the impediment "arises from the excess of the capacity of the heart, relative to that which has been preserved in the blood-vessels;" but cases in which hypertrophy and dilatation to an excessive amount have existed for many years, without giving rise to symptoms of impeded circulation, invalidate the theory of Andral. Hence, Dr. Blakiston infers, "it is probable that there is either some cause of obstruction connected with dilatation as yet undiscovered, or that one of its known causes has in many instances been overlooked;" and he sets out in search of the one or the other in the only sound manner—he compares two series of cases (the one exhibiting no traces of obstruction of the general circulation, the

other attended with such obstruction), and seeks to ascertain the distinctive organic condition in the latter.

The author relates (with great conciseness) the main particulars of 11 cases of cardiac disease without, and of 26 cases with, obstruction of the general circulation; and then gives the subjoined tabular view of the morbid states revealed in these cases, as well as in 118 others. This table, comprising 155 cases, is sufficiently valuable for extraction.

	CLASS I. 39 Cases Without obstruction of the general circulation.	CLASS II. 116 Cases With obstruction of the general circulation.
Incomplete aortic orifice ...	9	16
mitral ditto	16	37
aortic and mitral ditto	8	6
pulmonary orifice ...	*4	1
Tricuspid obstruction	1	0
Tricuspid regurgitation from disease ... ... 1, dilatation ... ... 0, both *4	5	5, 60, 41 } 106
Dilatation of left ventricle ...	10	9
right ditto ...	†3	22
both	†15	83
Hypertrophy of left ventricle	7	13
right ditto ...	1	2
both	12	32
Adherent pericardium	6	13
	Of the cases in this class death resulted from pulmonary obstruction in ... ... 20 Pulmonary disease in 3 Both in ... ... 9 Sudden stoppage of the heart in ... 10	Of the cases in this class there was anasarca in ... ... 102 Ditto and cerebral apoplexy in ... 7 Cerebral apoplexy alone in 7

Now, not to keep the reader in suspense, we may as well state at once that in *tricuspid regurgitation* Dr. Blakiston finds the condition associated with dilatation, which is the real and essential cause of systemic obstruction. His observations and reasonings concerning the tricuspid orifice and valve are extremely important; their pith may be conveyed somewhat in the following manner. Tricuspid *obstruction* was seldom found, atheroma and calcification being rare in this situation. *Regurgitation* was induced either by disease or by simple dilatation; and the author particularly insists upon the fact, that in every case, *except those wherein the foramen ovale was open*, the changes interfering with the efficiency of the valves

* "In three of these cases there was open foramen ovale."

† "In fifteen of these cases there was also dilatation of the tricuspid orifice, but in twelve of them the valves were so large that they closed the orifice and prevented regurgitation, and in the other three the foramen ovale was open."

were such as prevented them from closing, or, in other words, permitted regurgitation. Corrugation of the valves, adhesion of their walls to the ventricle, or shortening of their cords, were the conditions observed; the adhesion of the valve-wall was in some cases of distinctly inflammatory origin, in others, "no trace of any lymph, old or recent, could be found. In the 46 examples of tricuspid *disease* in the second class in the table, there was adhesion of 1 valve in 10 cases, shortening of the cords in 20, thickening and shrivelling of the valves in 16. Since the mitral valves were diseased in 53, and the tricuspid valves in 51 cases, "it follows that, contrary to the generally received opinion, the one orifice is as liable to disease as the other." But, observes Dr. Blakiston,

"The disease of the mitral orifice is generally palpable, while that of the tricuspid valves may very often escape the eye of a superficial observer. And yet the shortening of the cords of the tricuspid valves, so easily overlooked, would probably, for reasons before stated, have a much more serious effect on the circulation, than an obvious thickening and narrowing of the mitral orifice, or a deposition of vegetations on the fringes of its valves. It must be remembered, however, that this amount of tricuspid disease was found in persons, all of whom, during lifetime, had exhibited unequivocal signs of cardiac derangement, and in the greater number of whom, death took place from obstruction of the pulmonary or general circulation. But, were I to add to these cases, all those in which death resulted from other causes, yet in which there were also diseased cardiac valves, then the proportion would be greatly altered, and the number of cases in which the valves on the left side of the heart were diseased, would more than treble the number of those in which the valves on the right side were affected."

When the tricuspid orifice was dilated, so, generally (in nine tenths of the cases), were the right ventricle and auricle; the contrary to what took place on the left side of the heart. The comparative frequency of changes leading to induration and thickening of the mitral zone, accounts for the rarity with which it undergoes dilatation. Whatever were the reasons, dilatation of the tricuspid orifice was found in 91 out of 105 cases of dilatation of the right ventricle, and in 10 cases without that of the latter; in 46 cases disease of the valves coexisted. Dilatation of the orifice was not noted unless its circumference exceeded five inches in the male and four inches and a half in the female. Referring to the opinion of Hunter, Adams, and T. W. King, that slight tricuspid regurgitation takes place in health, Dr. Blakiston argues, that it is clear, regurgitation must take place in dilatation of the orifice, unless the valves are proportionately increased in size. Now, as this increase in size of the valves took place in 15 out of 18 cases (Class I), of dilatation of the right ventricle, in conjunction with that of the tricuspid orifice, there was in them no regurgitation. "Thus the valves were increased in size in 15 cases only out of 121, (both Class I and II), in which the orifice was dilated; a very different result from that which Dr. Hope supposed took place, as he conjectured that the valves generally increased in size with the dilatation of the orifice, and that, consequently, such dilatation did not induce obstruction to the general circulation by means of tricuspid regurgitation."

The late Mr. King argued, as is sufficiently well known, that the natural tricuspid regurgitation was a means of protection against arterial congestion of the lung. There can be little doubt, whatever be the truth in regard of the condition prevailing in health, that when regurgitation takes place

to any amount, the strong counter-current forced backwards by the systole of the right ventricle, offers a most powerful obstruction to the passage of the blood from the veins into the heart. We have always believed this; and the following summary of Dr. Blakiston's experience gives additional solidity to this view. (*a*) Tricuspid regurgitation took place in four cases out of thirty-nine in Class I. In three of these the foramen ovale was open, so that the full force of regurgitation was not sustained by the general venous current alone; but as the blood also entered the left auricle through the foramen ovale, its effects were felt in part by the pulmonary veins, and thus were divided between the two venous circulations. On this account, probably, argues Dr. Blakiston, the obstruction to the general circulation was insufficient to give rise to anasarca. These three, then, being rather apparently, than really, exceptional cases, there is but one inexplicable exception to account for. (*b*) On the other hand, in Class II, there was tricuspid regurgitation in 106 of the total number of 116, leaving but 10, in which there was general obstruction of the circulation without that regurgitation. "Here, then, we have the solution of the difficulty with which we started. Dilatation is the main cause of general obstruction . . . . *because* it is accompanied by incompleteness of the tricuspid valve, in consequence of which a powerful back current is forced against the blood returning from the veins of the general circulation."

In addition to this important proposition, Dr. Blakiston draws the following inferences from his analysed cases. 1. A considerable amount of obstruction may exist at the aortic orifice of the heart, without seriously affecting the general health. 2. Mitral regurgitation is one of the most direct and frequent causes of pulmonary venous congestion. 3. Except in conjunction with regurgitation through the auriculo-ventricular orifices, hypertrophy of the ventricles, in many cases, rather assists the circulation than promotes congestion."

Dr. Blakiston's chapter on the "diagnosis of valvular disease" disappoints us; we had hoped that so zealous an investigator would have succeeded in detecting some new sign to add to the not always serviceable list at present trusted to; but his contributions are rather destructive of old, than communicative of new knowledge. However, some such contributions are not to be despised. Thus he with perfect justness objects to Dr. Corrigan's sign of aortic regurgitation, namely, visible pulsations of the arteries, that such pulsation may be witnessed in certain cases of simple hypertrophy: though a useful, it is therefore not an infallible sign of the former state. In regard of the mitral valve, *obstruction* to a very great amount may exist here without murmur,—a proposition generally admitted; again, in several of these cases, where mitral *regurgitation* must have taken place, no trace of murmur was detected,—an exceptional condition of things, very certainly. Nay, more, a systolic murmur, most marked at the apex, existed in two or three cases where the mitral valve was found complete: in two of these cases, "the ventricular sides of the valves were covered with rough vegetations, so that, probably, the murmur was caused by the blood passing over this roughened surface." Mitral regurgitation, *joined with dilatation of the left auricle*, was announced by faint pulsation or undulation between the second and third left ribs,—a motion of which the synchronism with that of the ventricles was often difficult to deter-

mine. Dr. Blakiston insists on the rarity with which tricuspid regurgitation is productive of murmur; and also on the fact, that the absence of venous pulsation in the neck by no means proves the absence of such regurgitation: both of these propositions are in accordance with common opinion.

Dr. Blakiston thinks meanly of the practical philosophy of systematic writers, who lay down a particular line of treatment for each particular morbid alteration of the heart and its appendages. He would object to these writers, that as Nature has the habit of uniting cardiac changes, and of not permitting any one to grow and advance in a state of simplicity, it is of little use to frame lines of conduct for circumstances which never, or scarcely ever, arise. He conceives that the true guide to treatment is to be found in the absence or presence of symptoms signifying obstruction of the pulmonary or the general circulation. And he is right.

Before turning to the observations he has to communicate concerning these treatments, he offers a few remarks on that which may prevent the development of chronic heart-diseases. Inflammations, ill managed, lead to such diseases,—pericarditis among the number. "If acute pericarditis were treated with mercurial frictions and leeching, rather than by copious venesection, I cannot but think . . . that the heart would less frequently adhere to the pericardium, and that hypertrophy, dilatation, and valvular disease would be less common." We should be glad to know whether Dr. Blakiston has anything to present in the shape of solid support of a surmise of such important signification as this. But, meanwhile, we do not believe that the habit of bleeding from the arm in pericarditis, is so prevalent as the author's phrase would imply: for instance, we ourselves have *never bled thus in a single case of pericarditis*, and the number we have treated (rheumatic and non-rheumatic) is far from inconsiderable; nor have we ever seen reason to regret that we had thus abstained, though we confess we know not if the abstinence has lowered the quota of hypertrophous hearts among our patients. Dr. Blakiston suggests the propriety of rubbing in mercurial ointment over the heart in all cases of acute rheumatism, whether there be endo-pericardial signs or not;—his confidence in the powers of mercury is greater than our own, and greater than that of any man may legitimately be, who reads the analysis of Dr. Taylor's valuable cases, as given in the closing number (No. XLVIII, p. 565) of the 'British and Foreign Medical Review.'

We find nothing novel in the statements concerning the treatment of heart-disease without capillary obstruction-symptoms. Why, in the name of historical truth, is the promulgation of the facts that digitalis acts best as a sedative when given in tincture, and best as a diuretic when given in infusion, accredited to a gentleman named Munck? Why the "points" were part of our therapeutical grammar, we dread to think how many years ago.

Where pulmonary obstruction is present—hypertrophy coexisting with left valvular disease—the treatment will turn on the nature of the latter. If this be mitral regurgitant, the energy of the heart's action must be cautiously controlled; because the imminent tendency of the pulmonary congestion is to entail dilatation of the right heart. If, on the other hand, the valvular disease be obstructive (either mitral or aortic) or aortic

regurgitant, undue cardiac energy must be most cautiously interfered with, —seeing that that energy tends to obviate the ill effects of the valvular difficulty.

We leave the heart, and come to the pleura. Six cases of circumscribed pleurisy are here most concisely reported, and the author records his impression that "a careful perusal of them may possibly aid in removing some of the difficulties with which the diagnosis of the affection is surrounded." It is plain the author should himself have analysed the cases; it is scarcely likely that persons, who have not the pleasurable feeling of *property* in connexion with them, will undertake the task.

Dr. Blakiston makes up for this little piece of delinquency by analysing 78 cases of chronic pleurisy, which occurred under his own observation. The most important inference from the series is, as follows:—"The subjects of 10 of these cases have been lost sight of. The state of health of 15 others cannot be accurately ascertained; it is only known that they are living. *Of the remaining* 53, *not one has become phthisical.* Fourteen are subject to a cough, in 5 of whom it existed previously to the pleuritic attack; in all, it is accompanied by expectoration of tough, gray-coloured mucus." This is a signal and most valuable confirmation of one of the main doctrines of Louis in the etiology of phthisis; we trust it may reach the eyes and, if possible, the understanding, of some of those loose but determined perverters of clinical truth, who teach young men that the serous inflammation inflicts a sequence of consumption on those it visits. And two other of Dr. Blakiston's inferences (and, be it remembered, the experience from which these are drawn is neither limited nor careless) likewise strengthen the confidence we, for our part, have always felt in the accuracy of the great French physician. First, he observes, "To these might be added numerous other cases, in which the results of pleurisy have been detected, both during life and after death, and in which the disease ran its course, and terminated *favorably without any treatment.* From these facts it is hardly too much to deduce that, in general, chronic pleurisy is attended with but little danger either immediate or in prospect." And, secondly, he well urges, that a consideration "of the terminations of chronic pleurisy would lead to the conclusion that there is seldom a necessity for having recourse to the operation of paracentesis." Dr. Blakiston records his objections to venesection in cases of acute pleurisy; and again do we completely agree with him. His main dependence (after leeching in cases where severe pain is present) has been upon an ointment of mercury, opium, and camphor, the chest being well covered with linseed-meal poultices in the intervals of the frictions, and aperient or diuretic medicines prescribed according to circumstances.

Dr. Blakiston terminates a chapter on Plastic Pneumonia by "submitting that, as in pleuritis, bronchitis, and other similar inflammations, so, in pneumonia, there is a *serous, sero-plastic,* and *plastic* form; that the granulated surface of the red and gray softening and induration may, in some cases, depend on the presence of lymph in the air-cells; and that the *semi-transparent gray granulations of the lungs constitute a disseminated form of plastic vesicular pneumonia.*" The peculiar versions of facts (we do not mean to say they are strained or falsified, but simply that they *are* peculiar) and arguments, on which the last proposition is

founded, must be sought for in the author's pages, as they scarcely bear condensation.

Dr. Blakiston supplies valuable testimony to the efficacy of the treatment of sthenic pneumonia by a single full venesection and the use of tartarized antimony (administered to the extent of six grains the first day, and gradually increased to one scruple daily). Of 61 cases thus treated, in which the disease was simple or only complicated with a moderate amount of pleuritis, 3 died, and 58 recovered. In the asthenic or "typhoid" form of the disease, he still employs tartarized antimony, but combines it with tonics. Hence, in all pneumonias, tartarized antimony forms the basis of treatment,—to it being added bloodletting or tonic remedies according to circumstances. In this doctrine we have ourselves full faith, and have invariably acted upon it in both its elements.

Speaking of the causes of pulmonary phthisis, Dr. Blakiston takes this opportunity of expressing his dissent from one of M. Louis' etiological propositions;—Louis denies that proof exists of the power of *mental and physical depression* to produce phthisis; Dr. Blakiston holds that it possesses that power. Here is the author's train of argument on this point.

"The influenza of 1837 was not accompanied by long continued febrile action, but was characterised by extreme depression of the nervous system. Now the number of cases of phthisis which seemed to owe their origin to an attack of this disease was very great; having occurred in persons who, previously to their suffering from the influenza, had enjoyed good health, and had presented no traces of strumous diathesis. So also chlorosis has very frequently terminated in phthisis, more especially where the periodical menstrual discharge of the female has been arrested. As illustrative of the effects of mental depression, it may be stated that many cases seemed to arise out of an attack of syphilis or gonorrhœa. Several of the young men thus circumstanced had previously enjoyed excellent health, and were apparently free from any kind of constitutional taint whatever. In most of them the symptoms of the venereal affection had not been violent; but living in the constant dread of its discovery by their friends, they suffered greatly from anxiety of mind. This is only one of the sources of mental depression which, in my experience, has been so often followed by phthisis, that I cannot refrain from considering it to be a special cause of this disease." (p. 301.)

Dr. Blakiston contributes some very interesting particulars concerning the spontaneous suspension of phthisical disease,—a suspension, the frequency of which was first recognised by that generation of sages, the quacks; and great hath been the profit they have drawn from the recognition,—hence the reign of the pipe, of naphtha *et hujus generis omnis*, hath been a reign full of joy and gladness to those occupying the throne. Our author has had great difficulty in determining the ratio of cases of suspension, to those of onward and constant progress of the disease; but the following statement is nevertheless useful.

"All that can be stated at present is, that out of nearly 4000 persons supposed to labour under phthisis, treated between the close of 1835 and the commencement of 1846, thirty-one survived at the latter time, in whom the most unequivocal signs of a tubercular cavity in the lungs had been observed, either by Mr. Alfred Baker, late house-surgeon of the Birmingham General Hospital, or by myself. Of ten of these no more is known than that they have been reported by their friends as living. The other twenty-one have been seen by me within the last two years.

"Of these, eight still labour under the disease in a chronic form, four are tolerably well, but expectorate muco-purulent fluid, and have signs of a cavity remaining. The other nine enjoy an average amount of health, have slight cough, with expectoration of pearly gray mucus, and have lost all traces of a cavity. There is in most of them, however, coarse and feeble pulmonary sound in those situations where a cavity had existed." (p. 316.)

The diagnosis of acute phthisis is well known to be of most difficult attainment; the *via exclusionis* has been the route to the goal. Dr. Blakiston scarcely smooths the path by "submitting that when symptoms of high fever and great pulmonary obstruction are observed, unaccompanied by signs of acute bronchitis, pleurisy, pneumonia, or heart-affection, the disease may safely be pronounced to be *acute phthisis*, or," [in accordance with a notion of the author's already referred to] "vesicular pneumonia." Dr. Stokes insists on the importance of gradually increasing and slight dullness (supervening on signs of bronchitis); but in this dullness—or rather in its appreciable qualities—Dr. Blakiston has small faith.

Dr. Blakiston has done naphtha the singular honour of trying it in 100 cases of phthisis. The reader will not be surprised to learn, "that the progress of the disease was not arrested in any one case; and, on comparing 50 cases (of simple tuberculous consolidation without cavity or softening) treated thus with 50 others, tubercular softening was found to have come on as rapidly in one class of cases as in the other." If anything surprise us in this result, it is that Dr. Blakiston should have thus found naphtha an *innocent* agent; but he neither tells us whether he gave it alone in these cases, or how long in each instance he continued its administration. It seems tolerably certain that it cannot safely be prescribed for any length of time, uncovered and unprotected—without some combined medicine to counteract its special ill influences.

Cod-liver oil was likewise tried in 100 cases, and judged to be "not a specific remedy for phthisis, but useful, in some cases, when combined with a judicious system of regimen and diet."

As respects the three *plans of treatment* of phthisis, the *antiphlogistic*, the *expectant*, and the *tonic*, which have had their several classes of supporters, Dr. Blakiston ranges himself as an advocate of the last. With tonics he unites sedatives and counter-irritation of the thoracic surface.

And here must our analysis of the volume close,—not that it lacks further matter of interest, but that we have exhausted our space. Dr. Blakiston's production not only gives him a place in the rather thin ranks of sound and accomplished physicians, possessed of a true notion of the importance of their science, and of the means by which it should be cultivated, but adds to English medical literature one of the few really *inductive* works by which it is adorned.

ART. XII.

1. *A History of British Crustacea.* By THOMAS BELL, F.R.S., F.L.S., F.Z.S., &c., Professor of Zoology in King's College, London. Parts I to VI. Illustrated with Wood-engravings.—*London*, 1844-1848.

2. *A History of British Mollusca and their Shells.* By EDWARD FORBES, F.R.S., Professor of Botany in King's College, London; and SYLVANUS HANLEY, B.A., F.L.S. Parts I to VI. Illustrated with Copper-plates. —*London*, 1848.

THESE works form the latest additions to the series of beautifully illustrated volumes, descriptive of the various groups composing the British Fauna, for which we are indebted to the enterprise and liberality of Mr. Van Voorst. The first of them, by a distinguished member of our own profession, though commenced some years ago, is not yet complete; and, we are sorry to say, does not advance very rapidly,—the last three numbers having been published at intervals of a twelvemonth.

To all who are interested in this department of natural history, Mr. Bell's work is a valuable boon; and we trust that the learned author may be stimulated by the example of his younger *collaborateurs* to a more rapid progress. We shall condense from the last-published part, some interesting details respecting the lobster. Few, perhaps, have any idea of the enormous number of this crustacean annually consumed in Britain, or of its importance as an article of food, and as furnishing a means of employment to fishermen. The London market is largely supplied from Norway, whence we derive in the course of the season no fewer than 600,000 lobsters; as many as from 20,000 to 25,000 being often brought to Billingsgate from various sources in a single day. If we only allow as many to be eaten in the whole remainder of England, as in London, the number annually consumed must be reckoned by millions. Mr. Bell has received from Mr. Saunders, a fish-salesman of extensive dealings, the following curious information:—It is stated by Mr. Saunders that he has reason to suppose the lobster to be very stationary, seldom wandering fifty miles from the place of its birth; and he adds, what could scarcely have been anticipated as probable, that the lobsters brought from different localities "are as varied in appearance and character as a white man and an African; so that I could tell by looking at them (says Mr. Saunders) the part from which they are brought." This curious fact, the bearing of which upon the question of the origin of the diversities in the human species makes it not a little interesting and important, is corroborated by Mr. Couch; who, in his 'Cornish Fauna,' has the following observation: "Lobsters do not stray far from their haunts, and hence the discovery of a new station is a fortunate circumstance for the fisherman; and each situation is found to impress its own shade of colour upon the shell." We have ourselves noticed the extraordinary similarity which the hue of some small shrimp-like crustacea, that we observed swimming in little pools left by the receding tide, bore to that of the sand which formed the bottom; the correspondence being so close, that the animals could not be distinguished save by

very carefully watching their movements. Some small fishes inhabiting similar localities bore an equal resemblance to the prevailing hue of the boundaries of their habitation. In both cases, as we ascertained by microscopic examination, the colour was due to the nature of the secretion of the pigment-cells, most of which presented a sandy-yellow pigment, whilst these were interspersed with others in which black colouring-matter was included, so as to give to the integument precisely the minutely-speckled aspect of the sandy bottom over which the animals were moving.

The shedding of the claws of the crab and other crustacea, not merely in cases of severe injury, but under the influence (as it would seem) of alarm,—a circumstance well known to those familiar with the habits of these animals,—is a fact of no little interest to the physiologist. On being seized by one of their limbs, the captive member is left in the possession of the captor, and the animal escapes, leaving his *arms* on the field of battle. Mr. Saunders further states that the claws are frequently "shot" after thunder-storms or discharges of cannon; so that the entire produce of a voyage is thus at once rendered valueless. Mr. Couch, as quoted by Mr. Bell, considers it to be an erroneous opinion that these organs are ordinarily thrown off in consequence of violence done to them, and afterwards renewed; although the separation readily takes place if the wounded animals be roughly handled. In the common crab, he tells us, if the limb be crushed or broken without great violence, it is sometimes retained, and the creature will in no long time bleed to death; and to save the crab, the fishermen proceed to twist off the limb at the proper joint, or give it a smart blow, when it is rejected; and in either case the bleeding is stopped.

"It is only the legs, including those bearing the *chelæ* or nippers, that are readily and willingly thrown off by the animal; and in some cases, as in Porcellana platycheles, this is not only done on the infliction of violence, but as if to occupy the attention of some dreaded object, while the timid creature escapes to a place of safety. The general method of defence is to seize the object with the pincers, and while these are left attached, inflicting, by their spasmodic twitchings, all the pain thep are able to give, the crab, lightened of so great an incumbrance, has sought shelter in its hiding-place. It is by the short and quickened muscular action of the limb itself, and not by any effort of the body or peduncle, that this is effected; and as the convulsion will continue for a considerable time after the separation, it follows that the twisting-off of the claw, where the animal has seized human flesh, for instance, or any other sensible object, is the direct way to increase the violence of the grasp." (p. 247.)

A curious physiological question arises out of these facts, as to the source of the muscular power thus continuing to maintain the contraction of the pincers after the member is detached from the body, and has consequently been withdrawn from the influence of the great nervous centres. It strikes us as probable that subordinate ganglia exist in the limbs themselves, analogous to those with which the nervous cords of the arms of the cuttle-fish are studded, and which seem to be the immediate instruments whereby the contraction and attachment of their suckers are maintained. An excellent and conscientious observer, Mr. Peach,—an officer in the preventive service, who has made

large contributions to our knowledge of the natural history of the Cornish coast,—communicates the following statements made to him by intelligent and credible fishermen respecting the parental instincts of the lobster, which, if correct, prove that the attachment of these creatures for their progeny does not cease on the deposit of their spawn:

"I have heard the fishermen of Goran Haven say, that they have seen in the summer, *frequently*, the old lobsters with their young ones around them; some of the young have been noticed as six inches long. One man noticed the old lobster with her head peeping from under a rock, the young ones playing round her; she appeared to rattle her claws on the approach of the fisherman, and herself and young took shelter under the rock; this rattling, no doubt, was to give the alarm." (p. 249.)

We hail with peculiar satisfaction the appearance of the 'History of British Mollusca,' by Professor Forbes and Mr. Hanley; believing, as we do, that, by the conjunction of these two gentlemen in the task, a work will be produced, of the highest scientific value, as well as of a character most interesting to every lover of natural history. We understand that the arrangement of the mollusks of our shores into their classes, orders, and genera, has been undertaken by Professor Forbes, whose philosophic acumen will doubtless enable him to throw a new light upon many subjects hitherto unsatisfactorily treated; and from whom we confidently expect a more perfect classification of the mollusca than has yet been offered. On the other hand, the discrimination of species, and the very intricate department of synonymy (that is, the identification of species described by various authors under different names), will be the work of Mr. S. Hanley, whose untiring industry and penetrating sagacity in matters of this kind are worthy of all admiration. It has always been to us a most beautiful example of the adaptation between the objects of human knowledge and the powers and tendencies of the human mind, that the very different objects of study involved in natural history, requiring very dissimilar tastes as well as different habits of thought for their prosecution, should yet all find their willing and even ardent votaries; so that what would be the most painful drudgery to one is the eager pursuit of another, and the thread whose end has been picked out of a tangled skein by the patient and acute searcher, is taken up and completely unravelled by some more steady and experienced hand. We could ourselves no more spend six weeks (as we are assured that Mr. Hanley has done) in settling the synonymy of a single species, than *he* could spend the same length of time in poring through the microscope in the search for organic structure in fragments of shell. Let every man work out his own object of pursuit with a due appreciation of the value of the labours of others; and the result, we feel assured, will be complete and harmonious.

The parts of this work as yet published commence with a sketch of the history of the tunicated mollusks; a group which presents numerous features of great interest to the physiologist. It combines, in a most remarkable manner, the general plan of molluscan structure with the leading features of zoophytic life; for these creatures are, for the most part, attached during all but their embryonic life to solid bodies; and in this condition they propagate by gemmation, in such a manner that a

considerable cluster is budded forth from one individual. In this class we also find some of the most remarkable examples of the supposed "alternation of generations," which we shall examine at a future time; it also presents the simplest condition of the nervous system which we know to exist in any class of animals,—only a single nervous centre having been detected, and this being mainly occupied with the respiratory acts, the movements concerned in which are almost the only ones performed by the adult animal; and we also find in this group a very remarkable condition of the function of circulation; the whole current being from time to time reversed (just as if our right ventricle could force our blood backwards through the vena cava to supply the system, and the current were to return through the aorta and to be driven through the pulmonary veins to the lungs, returning to the heart by the pulmonary arteries), and after a short interval resuming its pristine course. The group is further of the highest interest to the zoologist, on account of its very close relationship to another which has hitherto been ranked amongst the higher zoophytes; but which should be raised, in the opinion of Professor Forbes, to a place in the molluscous subkingdom. We refer to the *Bryozoa* or *Ciliobrachiate polypes*.

From the tunicated acephala, the authors pass on to the ordinary bivalves; beginning with the *Pholadidæ*, the group which includes the teredo and its allies, as well as the pholas tribe. We have an interesting collection of evidence, with regard to the methods in which these boring mollusks excavate their dwelling-place in wood, stone, &c.; the authors inclining to the conclusion that the perforation is effected by the mechanical agency of the shell. They then proceed to another family of borers, the *Gastrochænidæ*; this, with other less known genera, includes the common *Saxicava*, which riddles the surface of almost every sea-beaten rock of mountain limestone, and which has attacked the Plymouth breakwater to such a degree, as to cause alarm for its ultimate safety. We quote the following remarks upon this genus, as an interesting example of the mutual relation now being developed, especially by the labours of Professor E. Forbes, between geological research and the geographical distribution of existing animals:

"Great interest attaches to the British species of this genus in a geological point of view; one, if not both of them, owing a wide distribution in the present epoch to events which occurred in pre-adamite ages. The researches of geologists have made known to us, that, previous to the present state of things, within the area of our islands, there existed climatal conditions much more severe than those which now prevail,—that, in fact, the climate of Greenland, and the fauna and flora of the regions in which that climate is now met with, then extended over the greater part of Europe and Northern Asia, having its southern bounds somewhere in a line with the southernmost part of the British Islands as they are now constituted. At that time, however, the greater part of our country was under water; and represented by ridges of land and small islands, rising in the midst of an icy sea. During this chilly epoch, the *Saxicavæ* extended their range almost round the whole of the northern hemisphere; and, when the bed of the glacial ocean was upheaved,—as geological research proves to have been the case, previous to the present arrangements of our region, and preparatory to a more general assemblage of conditions,—the shells of these mollusks were preserved in the raised sea-beds, and are found in them now, even at elevations of several hundred feet above the level of the present sea. Thus we find them in Sweden, where their inland position

attracted the attention of Linnæus, whose all-inquiring mind was deeply impressed with this curious, and, in his time, inexplicable phenomenon; in Norway, where the importance of the fact was fully recognised by the great German geologist, Baron Von Buch; in Canada, whence we have seen specimens brought home by Mr. Lyell; in distant and inland regions of Russia, where the glacial beds were traced by Sir Roderic Murchison and M. de Verneuil; and at home, where numerous observers have noted the inland occurrence of the *Saxicavæ,*—above all, Mr. Smith of Jordanhill, who, bringing the knowledge of the conchologist, and the discrimination of the field-naturalist, to bear upon these critical investigations, was the first to show that these shells alone, from peculiarities of variation and locality, indicated of themselves that the conditions under which they lived were dissimilar from those now regulating the distribution of animals in our seas. We could scarcely cite a more triumphant instance of the necessity of a minute study of the character and habits of our native shells to the geologist who seeks to interpret the complicated phenomena of the changes which preceded the present epoch; whilst, on the other hand, he may fairly appeal to the naturalist equally to recognise the services rendered, in return, by geological research; for assuredly it is as vain to attempt to explain the distribution of existing beings on the surface of the globe, without the aid of geological science, as it is to work out its physical geography without a careful study of the changes the earth has undergone in time." (p. 140.)

The British *Myadæ* are next brought under review; and the geographical distribution of our two existing species of *Mya* is shown to be in like manner dependent on past changes. The extent of their range is attributed in part to their power of accommodating themselves to alterations in the amount of saltness of the water they inhabit; such alterations, however, when carried too far, appear to induce *deformity* of the animals and shells,—of which two striking examples are mentioned as known at the present time, which render it probable that the numerous and singular varieties of *Mya arenaria,* found in the mammaliferous crag on the eastern coast of England, may have been distorted by the melting of the icebergs which then chilled that region.

Much attention has been given by Professor E. Forbes to the classification of the genera more or less closely related to the Myadæ, to which many additions have been made of late years, even from the British fauna: and he has thought it right to establish several new families for their reception, deeming the differential characters furnished by the *animals* of more importance than the points of agreement furnished by the conformation of the *shell.* Hence he separates the *Corbulidæ,* the *Pandoridæ,* and the *Anatinæ,* from each other, and from the Myadæ. In this arrangement he is remarkably supported by the characters furnished by the investigations of Dr. Carpenter into the *ultimate structure* of the respective shells; investigations to which he refers as "among the most important of recent contributions to Malacology." Thus, the complete isolation of the genus *Pandora,* indicated by the structure of its shell, is fully borne out by the peculiarity of the combination of characters presented by the animal; and the intermediate position of the genus *Lyonsia,* between Pandora on the one hand, and the Anatinæ on the other, with its nearer affinity to the former, is as clearly indicated by the intimate structure of its shell as it is by the anatomical characters of the animal.

The plates which illustrate this work are etched by Mr. G. B. Sowerby, jun.; which is an adequate guarantee for their fidelity. We could wish,

however, that they possessed somewhat more of the artistic beauty by which the illustrations of the works published by Mr. Van Voorst are generally distinguished.

We trust that the punctuality with which the first six parts of this work have appeared, will be sustained throughout the period of its issue, which cannot even then be less than three years. We shall report its progress from time to time.

ART. XIII.

Histoire Générale des Membranes Séreuses et Synoviales, des Bourses Muqueuses et des Cystes, sous le rapport de leur Structure, de leurs Fonctions, de leurs Affections, et de leur Traitement avec la Découverte d'un grand nombre de Séreuses et de nouveaux Sièges de Maladies. Par M. le Docteur EDOUARD GELEZ.—*Paris*, 1845.

General History of Serous and Synovial Membranes, Bursæ mucosæ and Cysts, in relation to their Structure, Functions, Diseases, and Treatment, with the Discovery of a large number of Serous Membranes and new Seats of Disease. By Dr. EDWARD GELEZ.—*Paris*, 1845. 8vo, pp. 356.

THE work before us is intended by its author as a contribution to general anatomy, and if we may judge by the egotistical tone of his composition, he regards it as by no means an unimportant addition to the science. We annex in the opposite page a synopsis of his classification of serous membranes with which he concludes his work, but which we select to head the few remarks we intend to make, as giving at a glance our author's arrangement. It will be seen by reference to the table, that his object is to associate together a series, or rather we should say, a number of serous (?) membranes, distinct from those "*of lubrication,*" which he constitutes into a *new* class, and designates as "*cyst-membranes of inclusion.*" Had M. Gelez, with this object in view, published in any medical, anatomical, or physiological journal, or even in a separate tract, his opinions on this subject, we should have heartily approved of his *endeavour;* but we cannot disguise to ourselves the extreme bad taste, to say the least of it, of the work before us, abounding as it does in egotisms, so needlessly amplified, and so entirely gotten up in the spirit of book-making. While out of 356 pages all that contains any matter of interest lies between pages 257 and 347,—and this indeed might have been written in one third the space,—we have made up for the occasion a full-sized octavo volume. Of this a considerable part is taken up by a long "*prologue,*"—much of it far better adapted for a general lecture than for a work on a special science, and much of it most fulsome adulation of his favorite author Bichat, interspersed with not a few hints that M. Gelez is one of a kindred spirit. If any one doctrine be more prominently brought forward than another in this prologue, it is that general anatomy is a subject of reasoning as much as of minute and careful investigation, a trite expression of which, from this *his own* prologue, he adopts as the motto of his work "L'anatomie se décrit, mais aussi elle se raisonne."

SYNOPSIS OF THE NEW DOCTRINE OF CYSTOLOGY, WITH ITS NOSOLOGY.

1st Class. *Cyst-membranes of lubrication or Membranæ hypohydratæ* (Bichat): *Normal and accidental.*

Visceral Serous Membranes	Arachnoid	....
	Pleura	....
	Pericardium	....
	Peritoneum	....
	Serous membrane of the scrotum and vulva	....
Articular Synovial Membranes	Synovial membrane of joints	of all the diarthrodial articulations
	Subcutaneous synovial bursæ	as of the knee, &c.
	Synovial tendinous bursæ (sheaths)	as of the fingers, &c.
	Inter-organic tendinous bursæ	between the angle of the jaw and the parotid gland, &c.

2d Class. *Cyst-membranes of inclusion or Membranæ hyperhydratæ* (Author): *Normal and accidental (cysts).*

MEMBRANES.		AUTHORS.	CONTAINED FLUIDS.	DISEASES.
Oculo-serous	Of the anterior chamber of the eye	Demours	Aqueous humour of the two chambers	Inflammation
	Of the posterior chamber of the eye	Author	,, ,, ,, ,,	Hydrophthalmia
	Crystalline	,,	Crystalline lens and liquor Morgagni	Cataract
	Vitreous or Hyaloid	,,	Vitreous humour or body	Adhesions
	Sub-choroid	Jacob	Liquor Jacobi	Posterior staphyloma—amaurosis; Internal ophthalmites
	Sub-sclerotic	Arnold	Liquor Arnoldi	Idem
Oto-serous	Labyrinthic membrane	Author	Liquor Cotunni	Internal otitis, effusions, adhesions
	Ventricular membrane	,,	Liquor Scarpæ (Otocønic)	Dropsy, tubercles, deafness
Cerebro-serous	Multiventricular	Bichat	Unnamed	Alterations in quantity or quality of the cerebro-spinal fluid: internal and external meningitis, dropsy, convulsions, tubercles, &c.
	Of the fifth ventricle	,,		
	Of the olfactory lobes, &c.	Author		
	Sub-arachnoid	,,	Sub-arachnoid fluid (cerebral)	
Medullo-serous	Central lining of spinal cord	,,	Unnamed	
	Sub-arachnoid	,,	Sub-arachnoid fluid (spinal)	
Amphiarthro-serous	Pubic	Pailloux	Synovia	Puerperal inflammations. Pott's disease
	Sacro-iliac	and		
	Inter-vertebral	J. Cloquet		
Ovarico-serous	. . . . .	Author	Fluid of the vescicles of De Graaf (ovarine)	Ovarian dropsy
Ovo-serous	Amnios	,,	Liquor Amnii	Moles
	Sub-amnios	,,	"False waters"	
	Decidua	,,	"Waters" of the decidua	
Odonto-serous	(?)	F. Cuvier	Fluid of the enamel	Alterations of the enamel
Adipo-serous	(?)	,,	Fat	Diseases of the adipose substance.

3d Class. *Mixed cyst-membranes.*

Angeio-serous	Endo-cardium		....
	Lining membrane of vessels	Arterial	Blood
		Venous	Chyle
		Capillary and erectile	Lymph
		Lymphatic and lacteal	

M. Gelez divides his cyst-membranes into three principal classes, as seen in the table. On the first of these he expends much paper. This is a natural group, and so generally understood that there is no occasion to follow him through his voluminous remarks on the "situation, number, symmetry, extent, thickness, colour, density," &c., of synovial membranes, and as little to notice his lengthened controversies, the personalities of which can interest only a few individuals besides those to whom they refer. The third class explains itself: it is therefore on the second only we shall make a few remarks. Of the so-called cyst-membranes here grouped together, a mere glance at the table will show us that they possess so little in common, that they belong to the same class on no other grounds than such as connect them with those from which he separates them: indeed various of his membranes in this second class can scarcely be considered in the light of serous membranes at all, while others, we are of opinion, should not be separated from the class of "synovial membranes." This most heterogeneous assemblage might far more candidly be classed under such a term as "Heterocletæ" or "Incertæ," by which titles some of the older botanic systematizers were content to group some of their more refractory plants, than under any name which infers that they have any character common to all, and by which they are separated from others. We will, however, cite a few instances from our author on this subject. He says (p. 240), "I call cyst-*membranes of inclusion* all those serous membranes which present contents, known in anatomy under the name of humours, fluids, water, centre, &c. The amnios is the type of this class of organs." To select one of his instances we take the eye. This M. Gelez considers as composed of a series of his cyst-membranes of inclusion, and though this view may be applied well enough to the two aqueous humours of the fœtal state, and although we concede to him the absorption of the adhering surfaces of these two serous cysts, as constituting the obliteration of the *membrana pupillaris,** yet when he comes to treat of the crystalline and vitreous bodies, we think his cyst-membrane theory will ill apply. To account for the membranous intersections of these bodies, he cites the "*analogy*" of the bands of fibrous adhesions in serous membranes *after inflammation.* This is a specimen of our author's analogies. There is certainly much which is ingenious in his system and grouping, but his analogies are so forced that we can accept no conclusions from them. He offers us one table of analogies (p. 275)—carried out rather minutely too, —between the nervous centres, the internal organ of hearing, and the globe of the eye; and another (p. 308) between the blood and nervous substance with their respective envelopes. We are constrained to ask, is this the reasoning to which M. Gelez looks for the advancement of general anatomy? For our own part, we cannot see how these strained analogies can aid the progress of science, and we deem it needless to follow M. Gelez through his other instance. All the useful hints which the work affords may be seen in the synopsis which we have extracted.

* The recent inquiries of Mr. John Quekett render it probable, that the *membrana pupillaris* is nothing else than the anterior capsule of the lens, and that its supposed disappearance is nothing else than the cessation of its vascularity.—ED.

PART SECOND.

Bibliographical Notices.

Art. I.—*Dysphonia Clericorum, or Clergyman's Sore-Throat: its Pathology, Treatment, and Prevention.* By James Mackness, m.d., Consulting Physician to the Hastings Dispensary.—*London*, 1848. 8vo, pp. 125.

The affection which constitutes the topic of this treatise, is one which, though frequently trifling in itself, becomes of serious importance to the individual who suffers from it; curtailing his powers of usefulness, and, it may be, depriving him of his livelihood. There is no impropriety, then, in making it a special object of consideration; and it is well that the attention both of the profession and the public, should be formally called to it. We dislike all attempts, however, at popularizing strictly medical works; and think that Dr. Mackness would have done much better if he had addressed himself to one or the other class separately, rather than to both conjointly. The medical reader scarcely needs the introduction about Demosthenes and Cicero, Luther and Burke; nor does he require either the anatomical account of the air-passages and organs of speech which occupies the first chapter, or the exposition of the structure and properties of mucous membrane which fills the second. On the other hand, a large proportion of the subsequent details must be "caviare to the general." In our apprehension, Dr. Mackness would have done well to have addressed to the *speaking* public his monitions on the causes and prevention of the disorder; and to have put forth his opinions on its pathology and treatment through the medical journals.

We believe that our author is, on the whole, correct in the view which he takes of the affection; namely, that it is in its simplest form a state of chronic irritation of the mucous membrane of the larynx and adjacent parts, frequently aggravated by a disordered state of the general health, and even passing on to ulceration; but that it is only in cases in which a tubercular diathesis previously exists, that this presents the characters of the disease described by Dr. Horace Green, of New York, under the title of "Tubercular Sore-Throat." He imputes the frequency of the ailment among clergymen, to the prevalent want of constitutional vigour and insufficient attention to the laws of health; and in this, also, we believe, that he is not far wrong. He lays great stress, therefore, on hygienic measures; and devotes to them a long chapter, which is, in fact, a brief "self-contained" treatise on the art of preserving health, such as would be quite superfluous in a purely medical work. In regard to local treatment, he speaks highly of the application of a strong solution of nitrate of silver to the diseased

surface, in the manner recommended by Dr. Green. Full details on this point, however, having been very recently given by one of our predecessors, we need not here repeat them. (Brit. and For. Med. Review. Vol. XXIV, pp. 500-3.)

We think it well to notice one point in the etiology of this disorder, which seems to have been overlooked by Dr. Mackness, and which is of considerable importance. Dr. Mackness adverts to the continued strain upon the vocal organs which is kept up in the exercise of clerical or forensic duties, as a source of laryngeal irritation; but he leaves out of consideration the interruption to the ordinary respiratory process, which, we are convinced, has much to do with the general exhaustion and debility consequent upon such efforts in most of those who are frequently called upon to make them. Let any one observe the respiratory movements of a public speaker; and he will see that, instead of breathing from sixteen to eighteen times in a minute, his respirations are not above half, or even one third of that number; moreover the inspirations are short and hurried, whilst the expirations are retarded to such a degree as to occupy nearly the whole of the period that ought to be equally divided or nearly so. In carrying on a conversation, the continual interruptions give the needed relief by allowing the renewal of the ordinary respiratory movements; but in the delivery of a sermon, lecture, or speech, no such opportunities occur, and the sum total of the respiration during the period thus occupied *must* be far less than its normal standard. We need not tell our readers what must be the consequence of the habitual operation of such a cause; but we may direct their attention specially to the fact, as not improbably aiding in the development of the tubercular diathesis, which is favoured by anything that impedes the due aeration of the blood.

What means, then, can be suggested for the prevention of this evil? Obviously the performance of a *natural* respiratory movement as frequently as possible during the elocutionary effort. This, no doubt, may occasionally somewhat mar the rhetorical effect; yet we believe that the general fault of public speakers is too great rapidity, and that more frequent and longer pauses, during which the chest may be emptied and refilled, would be beneficial to their audience as well as to themselves. Thus one piece of advice given to young preachers by the late Rev. C. Simeon, of Cambridge, was to the following effect: "Let there be the same kind of pause and of emphasis, as a man has in conversation when he is speaking on some important subject." We have reason to believe that a secret for the prevention of the discomforts alluded to, which has been sold at the rate of twenty guineas to each individual to whom it has been revealed, and which has been found effectual enough to induce many clergymen to spend their money in purchasing it, is nothing more nor less than the rule we have just hinted at,—namely, to make pauses as frequently as possible, during which the chest should be first emptied and then refilled. The voice should always proceed from a *full* chest; for if vocalization be continued until the lungs are emptied or nearly so, it can only be by a most injurious retardation of the expiratory movement. It is a common error, when pause is made, to make a quick and sudden *inspiration*; this only serves to replace the air already expired, and the remaining carbonised atmosphere of the interior of the chest is still

retained there. In order to get the full advantage of the pause, the *expiration* should be first completed; and the lungs then refilled with pure air. A little attention will soon enable this method to be practised without serious injury to the elocutionary effect, and with a most beneficial change in the feelings of the speaker.

Notwithstanding the comments we have thought it right to make on Dr. Mackness's treatise, we are bound to say that it contains a better account of the disorder in question, than has yet been laid before the British public; and that it may therefore be advantageously consulted by the medical practitioner as well as by the public speaker.

ART. II.—*Treatise on the Falsifications of Food, and the Chemical Means employed to detect them.* By JOHN MITCHELL, M.C.S., Author of 'Manual of Practical Assaying.'—*London*, 1848. Fcap. 8vo, pp. 334.

THE objects of this little treatise, as stated by its author, are "to point out a few of the adulterations practised by the manufacturers and vendors of various articles employed in domestic economy; as well as to show in what manner such adulterations can be readily and easily detected." Many of our readers, doubtless, remember the treatise on 'Culinary Poisons,' published some thirty years ago, by Mr. Accum, for the purpose of calling attention to the adulterations practised upon almost every article of food; and will recollect that either the same or some other work having similar objects, was advertised under the title of 'Death in the Pot.' Since that time the ingenuity of the fraudulent dealer has suggested many new adulterations, in which he has been aided by the advance of chemical knowledge; whilst a corresponding progress in the analytical department has led the chemist towards more ready and certain means of ascertaining the ordinary impurities in articles of food, as well as towards the detection of the newer and more carefully devised sophistications. A new treatise was therefore required; and Mr. Mitchell has supplied the deficiency in a very creditable manner. The following are enumerated by him as the purposes of these adulterations.

"1. To make the substance more saleable by improving its appearance, by the addition of some body either innocuous or otherwise.

"2. To depreciate its quality, by adding to it some substance which will diminish its *real*, without altering its *apparent* strength or general appearance. This is generally a very deadly fraud.

"3. To depreciate its quality by the addition of some simple substance, as water, or if it be a solid body, as plaster of Paris, sand, &c." (p. viii.)

As an illustration of the first may be noticed the adulteration of bread with alum, carbonate of ammonia, carbonate of magnesia, the sulphates of copper and zinc, &c.; all of which are employed to improve the appearance of the article, especially when it is made from inferior flour. From the statements of the author, it would appear that the London bakers' bread almost invariably contains alum, in quantities varying from 34½ grains to 116 grains in the 4lb. loaf. As an example of the second, may be cited the adulteration of porter by cocculus indicus, quassia, &c.,

which seems to be still carried on to an immense extent, in spite of very stringent enactments to the contrary, and the instantaneous conversion of new beer into old, by the addition of sulphuric acid. And the third is familiar to us in the watering of our milk, the sanding of our sugar, &c. &c.

Besides intentional falsification, our food is subject to receive contamination from accidental or unintentional causes; especially through means of the vessels employed in cooking it. The first section of Mr. Mitchell's work is, therefore, very properly devoted to this branch of the subject; and he then treats of the impurities to which water is liable. Flour, bread, milk, beer, cider, wines, spirits, coffee, tea, chocolate, sugar, honey, lozenges, &c., cheese, vinegar, pickles, anchovy sauce and paste, catsup, olive oil, pepper, mustard, and soap (which, although not an article of food, is placed on the same footing with the preceding as an article of domestic consumption), are then successively passed under review; the principal adulterations in each being specified, and an account being given of the chemical means available for their detection. Much useful information is contained under these heads; but we think that the work is very capable of improvement. Several of the chemical processes are insufficient for their purpose; others are needlessly complex; and a great deal of troublesome analysis might, in many instances, be saved by the simple appeal to microscopic evidence, especially in the case of substances in powder. Thus flour may, by its means, be detected in milk, sugar, mustard, &c.; potato-meal in flour, bread, tapioca, sugar, &c.; factitious in real pepper, red lead in cayenne pepper, &c. The following is a curious and not unimportant fact, showing an unexpected source from which contaminations may be derived, and of interest also in a physiological point of view.

"M. Coulier, inspector of police, having purchased some bread, submitted it to chemical analysis, and, to his astonishment and horror, found that it contained traces of mercury. At first he thought he was deceived, made a second and third analysis, and with the same result. M. Coulier, on this, made the strictest inquiries at the baker's, and found that one of the workmen employed was labouring under a frightful disease, requiring the exhibition of mercury; and the mercury existing in the bread had proceeded from the arms of the man in question. Although the author has never met with bread thus contaminated, it is very probable indeed that much of the same kind is distributed from the same cause in London." (p. 71.)

We add the following, as a matter of more practical importance.

"The author has also examined buns and other pastry obtained from various shops in the metropolis, and in many of them, more especially, he has found alum, plaster of Paris, chalk, and sand, in very large proportions, far greater than in bread. In one bun alone, the author found as much as three grains of alum and ten grains of chalk." (p. 72.)

Such adulterations are, of course, the more to be deprecated on account of the deleterious effects which they may produce upon children, who are the chief consumers of such articles.

We have little doubt that a far greater amount of disease than is generally suspected, arises from contaminations of food and of water; and our readers will render good service to the public, if they will apply their chemical knowledge to the detection of these, under the guidance afforded them by Mr. Mitchell.

ART. III.—*The Stars and the Earth; or Thoughts upon Space, Time, and Eternity.* Parts I and II.—*London,* 1847. 12mo, pp. 107.

IN the first part of this curious little production, the author endeavours to render the "ideas of space, time, and eternity generally and easily intelligible," by showing that, according to the known laws of the transmission of light, an observer endowed with adequate powers of vision, if transported in an immeasurably short time from a star of the twelfth magnitude to the vicinity of this our globe, must have seen completely, in this short space of time, the reflection of everything which has passed during four thousand years upon the surface of the hemisphere directed towards him; and thus that, in the sight of such an observer "a thousand years" might be *literally* "as one day."—In his continuation, the author endeavours further to illustrate these ideas in a similar manner; and "to deliver to the public in a comprehensible form those truths and ideas which have hitherto been the exclusive property of professed philosophers." In order to show how completely *relative* is our notion of Time, he supposes an observer to pass *from* the earth towards a fixed star of the second magnitude, which would be twenty years in receiving the luminous rays reflected from the surface of our globe. In this way, if he occupied twenty years and one day in his journey, he would have before him during the whole of that time the progressive changes which had taken place on the earth during the single day at the commencement of which he started from it, and would thus be enabled to watch the expansion of a blossom, for instance, during a period of twenty years, and to distinguish the most sudden changes with the greatest accuracy and leisure. We should thus have, as the author quaintly observes, a *microscope for time.* In like manner, if *all* the consecutive changes of which we are cognisant, whether terrestrial or cosmical, were to be accelerated or retarded at a uniform rate, we should not be conscious of any change, since all our standards of comparison would be equally affected. Thus time does not exist *in* and *for* itself, but is only a condition in which the occurrence of events comes to our knowledge; and we may imagine their rate to be indefinitely accelerated, or indefinitely retarded, without any essential change in their own nature.

In the same way the author attempts to do away with the idea of Space as a reality, and to show that it is a mere mode of contemplation, a condition by which objects are rendered perceptible to us.

These speculations and reasonings are ingenious and interesting; but we must own that, in our apprehension, they do not advance us perceptibly nearer to the solution of the great problem, of which it seems to be the author's purpose to give an intelligible explanation,—namely, how the universe, with its manifold changes and phenomena, can be the product of One First Cause. We have never felt the difficulties which he seems to attach to this doctrine; because, although philosophy has never yet mounted so high as to be actually able to comprehend the multiform phenomena of nature under a single generalization, its whole tendency is in this direction,—apparent diversity being continually resolvable into real unity, superficial complexity into fundamental simplicity.

ART. IV.—1. *Ragguaglio storico di una Paraplegia curata e guarita perfettamente coll' Agopuntura.—Venezia*, 1825.

2. *Trattatello diviso in* 11 *Articoli sull' Agopuntura.—Venezia*, 1841.

3. *Appendice ai Articoli sull' Agopuntura.* Del Dottor G. B. BELLINI.—*Fano*, 1845.

THESE three pamphlets on Acupuncture are written by Dr. G. B. Bellini, surgeon of the chief hospital of Florence.

The author has employed acupuncture extensively and variously. In the treatment of *varices*, he makes numerous perforations in the dilated vein within a spot the size of a sixpence, with the view of producing ecchymosis and adhesive inflammation. He does not give an exact account of the result of his experience, but says he has "not been too fortunate" in his cases.

In cases of *strangulated hernia*, where the reduction of the intestine is rendered difficult by its great distension with gas, Dr. Bellini argues that it would be better to perform acupuncturation of the intestine, than to make any considerable enlargement of the ring by incision. He has not done it himself, but states that it has been very successfully performed by Dr. Pasi, of Meldola.

In cases of *hydrocele*, the author's experience has led him to regard acupuncture as a palliative remedy only.

With regard to *ascites*, he relates the case of a woman, aged thirty-five, supposed to be pregnant, but really "afflicted with voluminous engorgement of almost all the abdominal viscera," who had undergone a course of mercurial frictions, baths, deobstruents, and counter-irritants for six months, and had been tapped, with the effect of removing only a very small quantity of fluid.

> "All believed her to be really in the arms of death, when acupuncture was performed without distinction in every part of the abdomen to a great depth, especially in the epigastric region and towards the liver; the hardness began to disappear and the strength to return in such a degree, that after twenty days, although she was not perfectly cured, she was able to depart for her village, where the next year she was tolerably well (*passava discretamente*)."

It certainly appears to us rather *bold practice*, but at any rate it is a curious fact that Dr. Bellini has repeatedly passed needles "into the stomach, the liver, and the intestines without any injury."

Several cases are recorded in which ganglia and bursal tumours were cured by acupuncture, and the author insists on its value as an exploratory and diagnostic process, in cases of tumours of doubtful nature. He refers to a case which occurred in Florence, when a popliteal aneurism was opened with the belief that it was an abscess, and the patient died. This would not have occurred had an exploring needle been used. It is in this way that acupuncture is most usefully employed, and most generally in England.

There is nothing new in the author's remarks upon acupuncture in hydrorachialgia, or spina bifida. In the treatment of ganglia and other chronic tumours, he perforates the sac in numerous points, and scratches the internal surface, in order to excite adhesive inflammation. The needle

is passed for some distance beneath the skin before it is made to perforate the sac.

In cases of suspended animation, not content with the practice of Leroy d'Etiolles of inserting needles close to the course of the diaphragm and communicating them with a voltaic pile, Dr. Bellini recommends acupuncturation of the heart itself. He does not appear to have ever seen it done, but states that Dr. Gamberini, of Bologna, in the case of a Swiss soldier affected with epilepsy, introduced a needle, "very deep, perpendicularly between the fifth and sixth ribs during the paroxysm, and the patient was not aware that he had been punctured. After two days, the fit came on again, the same operation repeated and the fit cut short, this time *perhaps* for ever," says the doctor.

We shall conclude this notice with an abstract of a case of *paraplegia* treated by acupuncture by our author.

"A female, aged 48, plethoric, and sanguineous in temperament, excitable, subject to rheumatic affections, and who had been obliged to live close to a large torrent of water, by which her habitation had been destroyed, a river having overflowed its channel, complained of chronic lumbar pains, which gradually descended to the articulations of the lower extremity, and were followed by loss of motion and diminished sensibility, so that she was obliged to remain in bed, and could not raise herself to evacuate the bowels. She was brought to the hospital, and the usual remedies having proved inefficacious, six fine sewing-needles were inserted to the depth of an inch into the vasti and rectus muscles, and allowed to remain ten minutes. No pain, or bleeding, and no inflammation followed. On the second day the same needles were inserted into the same muscles, but in different spots, and they were left for half an hour. No effect. On the third day, they were left for an hour in the gracilis, semitendinosus, semimembranosus, and biceps. Slight movement observed for the first time. On the fourth day the punctures were prolonged for two hours in the peronei, tibiales, and the extensors. Sensation of numbness produced. On the 5th day the needles were maintained for two hours in the iliacus internus, psoas, and pectineus, in the superior part of the sartorius, gracilis, and adductor longus, to the depth of two inches. In the evening, with the support of two persons, she made a few steps. On the 6th and 7th days, the needles were left six hours in the glutei. Her steps were less uncertain. On the 8th and 9th days, four needles were left for ten hours in the abductor of the little toe and the abductor of the great toe. Her steps were more measured, and she did not slide on the soles of the feet as before. On the 10th and 11th days, the number of the needles was increased to twelve; they were inserted in the nates, but were obliged to be removed after six hours, as the patient felt irritable, and perspired greatly. On the 12th and 13th days, the gastrocnemius, soleus, and short extensor of the toes were perforated; the needles were removed after three hours, from the same cause. On the 14th day, the glutei were again acted on; nothing was done on the 15th, and on the 16th and 17th, eighteen needles were inserted in the thighs and legs. Her motions became much quicker, and she was able to stand upright unsupported. She improved in health and left the hospital, but being obliged to live in a miserable cabin during the following winter, the same condition returned. Acupuncture was again practised, commencing with twenty needles, left for three hours, and afterwards augmented to 31, and left six, nine, and fourteen hours daily, without causing any suffering. She rapidly improved, and two years afterwards remained in good health, supporting herself and walking without any assistance, and the last accounts, seven years afterwards, state that no relapse had occurred."

In a case of paralysis from cold, destitution, and mental causes, probably the comforts of a hospital may have operated as beneficially as the acu-

puncturation. Still it appears that other remedies had been tried without success; and as it seems clearly proved that no ill effect follows the insertion of needles into muscles, the practice would be certainly worth a trial in similar cases.

The papers of Dr. Bellini are rather rambling and controversial in style, but still they show that he is, what we have the pleasure of knowing him to be, an ardent and enthusiastic lover of his profession.

Art. V.—*Chemical Examination of the Urinary Calculi in the Museum of the Medical Department of Transylvania University; with Remarks on the relative Frequency of Calculus in Lexington, K. Y., and the probable Causes.* By Robert Peters, M.D., Professor of Chemistry and Pharmacy in Transylvania University.—*Lexington*, 1846. pp. 37.

Dr. Peters undertook his investigations with a clear view of the general bearings of the subject. His principal object was "to ascertain what relation in composition is borne by the calculi of this region [Kentucky] to those of the other parts of the world, as far as statistical information can be obtained, and to endeavour to learn what influence is exerted in the production or composition of these troublesome concretions by the use of the *hard* or *limestone* water, and the rich 'hog and homminy,' or bacon and corn-bread diet, so general among the mass of the inhabitants of the middle states, and especially in Kentucky."

Of the 78 calculi from the human subject which Dr. Peters analysed, the composition of the *nuclei* is as follows:

Uric acid . .	mainly in	32	Phosphates . .	mainly in	7
Urate of ammonia .	"	26	Foreign substances .	"	4
Oxalate of lime .	"	7	Cystin	"	2

The bodies are composed as follows:

Of Uric acid . .	mainly in	34	Mixed phosphates .	mainly in	16
Urate of ammonia, &c. .	"	2	Triple phosphates .	"	4
Oxalate of lime . .	"	16	Cystin . . .	"	2

In addition:

Uric acid . exists in *notable proportion* in		4	Mixed phosphates exist in *not. prop.* in		12
Urate of ammonia	"	24	Phosphate of lime	"	5
Oxalate of lime	"	9			

The *cortex* or outer layer was of

Uric acid	in	34	Mixed phosphates . .	in	37
Urate of ammonia, with phosphates	"	2	Triple phosphates . . .	"	2
Oxalate of lime	"	9	Cystin	"	2

Dr. Peters very properly directs attention to the fact, that when more than a single calculus are removed from a patient, that circumstance ought to be mentioned and taken into account.

It is the *number of patients*, not the *number of calculi*, that affords his valuable statistical information regarding the frequency of the various forms of urinary concretions.

"Dr. Bird, in his paper on the calculi in Guy's Hospital Museum, gives a very satisfactory account of 353 of these concretions, of which 269 are mainly composed of uric acid and water. But he merely incidentally mentions the fact that 142 of these were taken from *one* patient, and is altogether silent as to any other duplicate calculi which no doubt exist in the collection . . . . Mr. Smith*

* Med.-Chir. Trans., vol. xi, p. 10.

remarks, in relation to the 74 calculi of uric acid in the Bristol collection, that "in several instances three, four, five, and six were removed by one operation."

A case has been mentioned in this country, that of the late Chief Justice Marshall, in which the number of calculi was said to amount to a thousand.

In the Lexington collection, thirty-one, principally composed of uric acid, were taken from one patient, and two, principally of urate of ammonia, two of fusible phosphates, two of the triple phosphate with beans for nuclei, and two of cystin, were each severally taken from one patient. The comparison made, therefore, of the number of *cases* would be materially different from that made with the number of *calculi*, as it is usually drawn, and would, moreover, be of much more value in an etiological sense." (p. 17.)

The peculiarities presented by the Lexington collection as compared with the others whose history is known, are as follows:

A. *As regards the nuclei.*

1. A great deficiency in the proportion of pure uric acid.

2. A great excess in the proportion of nuclei of urate of ammonia, and of the earthy phosphates.

B. *In relation to their general composition.*

The same peculiarities pointed out above in regard to nuclei, with the additional one:

3. An excess in the proportion of mulberry or oxalate of lime calculus.

From statistical data it appears that calculous affections are more frequent in the city of Lexington than in any other part of the world in which inquiries regarding this subject have been substituted. We extract the following table illustrative of the fact:

Countries.	No. of cases per annum.	Authority.
Ireland, the pauper population	1 in 875000	Yellowly
city of Cork	1 800000	Civiale
Bohemia	1 347000	"
Island of Malta, Goze, and Cumino	1 300000	"
Cornwall and Devonshire	1 293000	Smith
Glasgow	1 77000	Yellowly
Ionian Isles	1 60000	Civiale
Bristol	1 41000	Yellowly
Lombardy	1 38500	Civiale
London	1 38000	Yellowly
County of Norfolk	1 34000	"
Copenhagen	1 22000	Civiale
City of Norwich	1 21000	Yellowly
City of Leamington	1 16050	Peters.

From a careful review of the chemical nature of the food and water of the district, Dr. Peters arrives at the following conclusions:

"It appears, therefore, highly probable that the use of the limestone water, and a corn and bacon diet, may be the principal cause of the great excess in the proportion of the earthy phosphates and oxalate of lime in the calculi of this region; that the earthy carbonates with water, by neutralizing to some extent the natural acidity of some of the animal fluids, may be the cause of the very small proportion of pure uric acid, and the great proportion of urate of ammonia to be found in them: and that both these causes acting together, and superadded to all the other circumstances predisposing to urinary deposits to be found everywhere, occasion the great increase in the proportion of calculous disease which is presented in this country." (p. 32.)

ART. VI.—*Observations on the Cultivation of Organic Science, being the Hunterian Oration delivered February* 14, 1848, *before the Royal College of Surgeons of England.* By RICHARD D. GRAINGER, F.R.S., Lecturer on General Anatomy and Physiology at St. Thomas's Hospital. —*London*, 1848, 8vo, pp. 60.

MR. GRAINGER well deserves the thanks of all who heard, and of all who shall read, this admirable Oration, for having selected a theme so appropriate to the occasion, and for having developed it in so lucid a style and so attractive a form. His object is to show how completely the progress of modern physiological science has been in harmony with the spirit of the labours of that great master whose name we delight to honour; and how wonderfully clear and expressive we now find his enunciations of truths which were not understood in his own age, because it was not prepared to receive them. Mr. Grainger's address principally consists, therefore, of a comparison between the older and the more recent methods of pursuing organic science; and of a contrast between the barrenness of the one and the wonderful productiveness of the other. We fully agree with him in the conviction, that, from some cause or other (probably an insufficient perception of the practical bearing of many of the discoveries of greatest scientific interest), "the acquisitions of the last few years have, in more than one quarter, been either undervalued, or have not commanded that prominent degree of attention to which it is conceived they are so pre-eminently entitled." And we entirely accord with him, also, in the following estimate of the real value of the discoveries alluded to.

"It would not be difficult to show that, within the space of these few short years, the whole aspect of organic science has been changed; that discoveries, in no degree second in intrinsic importance to that of the circulation, have been made, casting a flood of light on the most universal phenomena of animal bodies; that generalizations, relating both to properties and laws, having a value and a signification in the organic creation so vast, that they can only be properly compared with those of gravitation or chemical affinity in the domain of common matter, have been firmly established; that, whereas, till a period so brief, that it is as nothing in the ordinary calculations of human progress, there was scarcely a prime question, relating whether to structure or function, which was not either unknown or involved in doubt, there are now few on which a mass of knowledge is not possessed, which, even by the most sanguine, must have been hoped for, rather as the fruits of centuries than of years." (p. 8.)

Into the masterly review of the past and survey of the present, which Mr. Grainger has managed to comprise within the brief space allotted to him, we regret that our limits prevent us from following him. And we must content ourselves with expressing our hearty concurrence with every sentiment he has uttered on this topic,—our admiration for the manly independence of his criticisms, and for the clearness of his own scientific conceptions,—and our earnest hope that this Address may be widely diffused and carefully perused, and may contribute towards the further development of that philosophical spirit which his own course, both as an investigator and as a teacher, has so worthily manifested.

Art. VII.—*Sanitary Reform and Agricultural Improvement; or how to promote Health and Abundance. In three Letters.* By Charles F. Ellerman. Letter I. *Drainage, Sewerage, Urinaria, and Cloacæ.* —*London*, 1848, 8vo, pp. 70.

Although this pamphlet must be regarded to a certain extent as a *puff* of the deodorizing fluid, noticed in our last Volume, yet its main object and argument are so just and important, that we think it well to place them before our readers. Mr. Ellerman addresses Lord Morpeth with especial reference to the recommendations of the Metropolitan Sanitary Commissioners on the subject of Sewerage; and strongly urges the incompleteness of any system, that shall go no further than the removal of all offensive matter, and its discharge into the nearest stream. It may very possibly prove, that the members of the Sanitary Commission regard their functions as limited to the conservation of the public health, and feel themselves especially called upon to devise measures for the immediate removal of existing nuisances,—leaving it to other bodies, public or private, to devise means of safely and effectually applying to useful purposes the matters which *they* have only to ascertain the best means of clearing out. At any rate, Mr. Ellerman's appeal to Lord Morpeth, to include in his Bill for Sanitary Reform such provisions as shall at least recognise the importance of such applications, is well timed, and may, we trust, not prove ineffectual. The following are the two points which he especially urges.

"I. Any plan is unhealthy, uncleanly, and enormously wasteful, which consigns the excreta of the population to rivers or water-courses.

"II. Any plan whereby the refuse of towns is employed to fertilize the country, is seriously defective, unless due provision is made, that nuisance and injury to public health shall not arise; that substances pernicious to vegetation shall not be mixed with those which are beneficial to vegetation; that the smallest possible quantity of the latter shall be suffered to escape; and that it shall be saved in such a form as may admit of its being rendered available in all parts of the kingdom, and in such a state of dilution or concentration as varying soils, seasons, or other circumstances may require." (p. 36.)

Of the immense economic value of the preservation of human excreta, when we are sending whole fleets in search of those of birds, which consist of precisely the same materials in a less advantageous form, no thoughtful man can entertain a reasonable doubt. Various estimates are given by Mr. Ellerman from different sources, which place this matter in a strong light. The *lowest* estimate,—that of Mr. Smith, late of Deanston, well known as a distinguished agriculturalist,—rates the average annual value of the excreta of each individual as £1; so that, taking the whole population of Great Britain as 28 millions, we are positively *throwing away* every year that which is equivalent to *twenty-eight millions sterling*. The actual saleable value in Belgium of the excreta is 37 shillings for each individual; and at this rate we may be said to be annually depositing the worth of fifty-one millions sterling in the ocean that washes our shores. It seems to us that there is an essential fallacy in all such estimates of the actual money loss or gain, involved in the difference between perfect and imperfect sanitary arrangements; since it does not by any means follow, that because a certain

quantity of manure would at present fetch from 20 to 37 shillings, the same quantity multiplied twenty-eight millions of times would have a value twenty-eight million times as great. But the immense importance of preserving human excreta, and of applying them to the production of food, is perhaps better understood from the ascertained results of their employment in agriculture. According to Dr. Lyon Playfair, a pound of urine is capable of increasing the production of grain by an equal weight; so that, even allowing for some exaggeration in this estimate, the human urine at present wasted in this country would serve to produce more than all the grain required for the consumption of its entire population, besides affording, through its fertilizing influence on lands at present imperfectly tilled or not tilled at all, a source of employment to our superabundant population. We cannot too strongly urge an alteration of our present system,—by the introduction of some general system for the profitable application of sewage-manure, which shall be encouraged if not enforced by the legislature,—as one of the most important measures of public economy, and as likely to conduce more, if fully and effectually adopted, to the support of our national prosperity, than any other, affecting its *material* elements alone, to which public attention has yet been directed.

But the difficulties in the way of the solution of this problem have been as yet too great, to admit of the adoption of any of the systems hitherto proposed. Mr. Ellerman points out the chief objections to the various schemes before the public; and we are disposed to agree in most of those which he urges. Of course he considers that he has something unexceptionable of his own to propose; and if his premises be granted, we admit that he realizes, in theory at least, all the requirements of the second of the desiderata just specified. He would have the contents of water-closets, urinaria, &c. conducted through a distinct system of tubing, into close reservoirs sunk in the ground, each receiving the discharges from a limited district, in the centre of which it should be placed. His deodorizing fluid should be mingled with the excreta during some part of their passage to this reservoir; so that, by the time of its arrival there, it would be deprived (in Mr. Ellerman's apprehension) of all offensive odours, and of all power of generating injurious miasmata; and from these reservoirs it might be pumped up, and carried away in covered carts, to be either conveyed to a short distance for application in a liquid state, or to undergo a process of concentration whereby its bulk would be reduced, for convenience of carriage to parts of the country remote from the source of its production.

We have said that the plan appears to us unexceptionable—save in the enormous cost of its first introduction,—provided the premises be granted; the great condition being, that Mr. Ellerman's deodorizing fluid shall not merely render a cesspool inoffensive to the nose, but shall deprive it of its power of giving origin to emanations of any kind injurious to health; and also that it shall not mingle with the manure any substances injurious to vegetation. As no new evidence on either of these points is produced by Mr. Ellerman, we content ourselves with referring to our former remarks on the claims he has put forward. (Vol. I, p. 261.)

ART. VIII.—*Discorso Inaugurale pronunciato nella Tornata Pubblica del* 16 *Ottobre, all' Apertura dell' Anno Accademico* 1846-7, *nella Società Medica d'Incoraggiamento di Malta.* Dal Dr. T. CHETCHUTI, di lei Presidente.—*Malta,* 1846.

Inaugural Discourse, delivered to the Public Meeting of the 16*th October, at the Opening of the Academic Year,* 1846-7, *to the Medical Society of Encouragement of Malta.* By Dr. T. CHETCHUTI, President of the Society.—*Malta,* 1846. 8vo, pp. 25.

THE learned President of this ancient Society has taken, as the subject of the discourse before us, the present aspect of medical practice; with the view of examining into the real merits of the therapeutic systems at present in vogue, and of pointing out the course to be pursued in the search after more truly scientific methods of treatment. He feelingly deplores the uncertainty of many of our boasted remedies; adverts to the frequency of perturbing and injurious results from the fashionable polypharmacy; and does not think it disgraceful to his professional standing to lay stress on the success which has often crowned a system of negative therapeutics. In proof of the truth of the last proposition, he states two interesting facts:

"It was thus that the pills of Dr. Andrea Pasta effected so many surprising cures, and brought him an immense fortune; pills which the same Dr. Pasta, when at the point of death, revealed to his son, also a physician, to be composed solely of crumbs of bread. In the same way Dr. Filippo Zammit, with his neutral alkaline salt, obtained brilliant results especially in chronic diseases, and amassed it is said £5,000, which he afterwards piously left to the Saura Hospital." (p. 9.)

"In May last, in our insane asylum, a gastro-rheumatic fever arose from unknown causes in the female division among the youngest of the patients, and in thirteen of them assumed a typhoid character more or less severe. In nine cases, on account of their obstinately refusing all internal medicine, I was limited to local depletion, external derivatives, and to cold water as the sole internal remedy. In the other four, two of whom were not insane, the ordinary medicinal treatment in such fevers was pursued, and I observed, not without a certain sense of humiliation, that the course of the fever was shorter, the convalescence more decided, in the first nine than in the other four." (p. 9.)

While ably arguing that therapeutics and the use of drugs are far from being synonymous terms, he still allows the efficacy of drugs as remedies, and inveighs not against their use but their abuse. "It is consoling," he says, "to observe that generally, and also among ourselves during the last few years, the mania for exsanguinating the sick, for pursuing them with the lancet to the tomb, is diminishing, as well as the other not less deadly, for saturating them with poisons and heroic medicines." (p. 10.) The second law of therapeutics should be to assist, the first to do no harm.

He gives the following as the general character of medical practice in Malta:

"With the generality of our practitioners, medicine in the last forty years has oscillated between an excessive medicinal activity communicated to us by the English military polypharmacy; a mania of exclusive systems diffused by various channels from the neighbouring fervid Italy; and an easy, calm, philosophical method of treatment, which the most distinguished of our countrymen have continued to inculcate by example and precept, and not without some good result." (p. 21.)

We also learn from these pages that patent medicines are much abused in Malta. The author states that he has been consulted in two cases, in which the capsules of balsam of copaiba produced acute gastro-enteritis, which in both was followed by the death of the patients. Until the last few months, also, the secale cornutum was used indiscriminately by the native midwives, and the effects upon the life of the offspring became so evident, that the government by a circular to the druggists prohibited its sale. The author wisely recommends that a similar prohibition should be placed upon the sale of many other drugs without medical prescription.

We congratulate the Maltese Medico-Chirurgical faculty, with the author, "on far greater simplicity and efficiency in the method of treating disease, and further on being more conscientious, and far more undeserving of the insulting reproach to physicians of lavishing their remedies and drugs solely upon others." (p. 25.) Our Maltese brethren now form an integral part of the British faculty, and with industry alone will well support its credit. The want of industry, of continued steady application, of generous emulation, must be confessed to be a characteristic of a large portion of the Maltese practitioners of the day; it is being supplied, and nothing has contributed more to the regeneration of the profession than the re-establishment of the Medical Society of Encouragement, which, founded nearly two hundred years ago, ranks as one of the most ancient in Europe. We hope to see it become a bond of union between the English and native practitioners, and lead to a series of exact statistical observations which will confer a real benefit upon medical science. "Deeds not words" must be the motto, and patient investigation replace worthless wordy discussion.

ART. IX.—*Recent Advances in the Physiology of Motion, the Senses, Generation, and Development: being a Supplement to the Second Volume of Professor Müller's 'Elements of Physiology.'* By WILLIAM BALY, M.D., F.R.S., Physician to Millbank Prison, and Lecturer on Forensic Medicine at St. Bartholomew's Hospital; and WILLIAM S. KIRKES, M.D. With Twenty-five Wood-Engravings.—*London*, 1848. 8vo, pp. 132.

THE early appearance of this Supplement had been so frequently announced, our expectations being as frequently disappointed, that we had been disposed to look upon it in the same light with the Third Part of 'Quain's Anatomy,' the next number of the 'Cyclopædia of Anatomy,' and one or two other long-promised works, of whose completion during the present generation we had come to entertain a feeling nearly akin to despair. Its actual publication, therefore, has brought with it all the charm of an agreeable surprise; and our pleasure has not been diminished, but, on the contrary, increased, by our perusal of the volume. The nature of its contents forbids analysis; since it professedly consists of nothing else than a condensed summary of the researches upon certain departments of physiology, in which the last few years have been so wonderfully fertile. The volume is, in fact, a report upon these subjects, extending backwards to the period of the completion of Dr. Baly's translation of Professor Müller's 'Elements;' that is to say, about six years since. We shall, therefore, confine ourselves to a simple enumeration of its principal topics, and to a general account of the mode in which they have been treated.

Under the head *motion*, we have in the first place some additional details on ciliary action; next some important and conclusive evidence regarding the muscularity of the arteries and capillaries; and thirdly, a summary of the latest researches on the structure and properties of muscular fibre. A short but interesting notice of some new observations on the voice then follows; and we next have an account of the recent advances in the physiology of the senses. These, however, are almost exclusively confined to the sense of vision; and have reference principally to the structure of the iris and retina, the adaptation of the eye to distance, the relative action of the retina and the sensorium in vision, and the theory of complementary colours. The whole of this first portion is furnished by Dr. Kirkes; and is executed in a manner very creditable to his judgment in the selection, and to his literary ability in the arrangement and exposition, of the materials at his command.

With the account of the recent advances in the physiology of generation, Dr. Baly's portion of the work commences; and we need scarcely say that it is executed with the same careful discrimination, comprehensive acquaintance with physiological science, and literary skill, as distinguished his translation of Professor Müller's 'Elements,' and the admirable notes which he appended to it. The greater part of it, he informs us, was written during the winter of the years 1845-6; and we might notice one or two points which have been more fully elucidated by later researches, —as, for example, the history of the evolution of the mammalian ovum from the Graafian vesicle given by M. Pouchet; and the observations on the menstrual fluid supplied by Mr. Whitehead. The subjects on which the principal additions have been made under this head, are as follows:—The unimpregnated ovum; the semen (which has been particularly studied by that industrious and accurate observer Professor Kölliker); the discharge of ova from the ovaries; the corpus luteum; the nature and purpose of menstruation; fecundation; and the changes which take place in the impregnated ovum, down to the completion of the cleaving process;—the most important researches on all of which topics are very clearly though concisely set forth and impartially estimated.

Dr. Kirkes then resumes the history; and gives us a comprehensive summary of all the newest and best researches,—first on the development of the embryo as a whole, with its peculiar appendages,—secondly, on the development of its various organs,—and thirdly, on the development of the animal tissues. We can find nothing wanting in this part of the work, which is everywhere brought on to the most advanced information on each part of the subject.

It will be apparent from this notice, that the volume before us is not only of value as a supplement to the *second* volume of Professor Müller's classical work, but that it will afford to every one who is interested in the progress of physiology, a complete *résumé* of the more recent and trustworthy researches in the departments of the science which its authors have thought it desirable to embrace. We can only regret that they did not include the topics discussed in the *first* volume of Professor Müller's 'Elements,' and thus give us a *complete* report of the recent progress of physiological science.

ART. X.—*A General Medical Catalogue of Modern Works with their Prices and Dates, corrected to January,* 1848. *With a Classified Index of Subjects and of the Authors who have written on them.* By SAMUEL HIGHLEY.—*London,* 1848. 12mo, pp. 86.

THE utility of such a catalogue is so obvious, that we need say nothing in its recommendation, further than to remark that we have reason to believe it to have been very carefully and judiciously compiled, and that we have not ourselves been able to detect in it any errors either of commission or of omission.

ART. XI.—*Vision in Health and Disease; the Value of Glasses for its Restoration, and the Mischief caused by their Abuse; being the substance of Lectures delivered at the Central London Ophthalmic Hospital.* By ALFRED SMEE, F.R.S., Surgeon to the Bank of England, &c. &c. Illustrated by One Plate, and numerous Woodcuts.—*London,* 1847. 8vo, pp. 64.

IF any one takes up this pamphlet with the expectation of finding in it a complete view of the physics and physiology of vision, he will be assuredly disappointed, as it contains very little that bears on the latter division of the subject. The optical laws, according to which the rays of light are made to form pictures on the retina, are explained very much as in elementary treatises on the subject; and the application of these laws to the correction of imperfect vision by convex or concave lenses, is shown with sufficient intelligibility. The remainder of the space is chiefly occupied by crotchets of Mr. Smee's own, which, as usual, are worth very little to other people. Thus we have an *optometer* for ascertaining the "optical properties of the eye, the defects of which we seek to remedy by optical contrivances;" in other words, for indicating the proper focus of spectacles; this is accompanied by a table which surpasses even a railroad time-table in complexity and unintelligibility; and after all, as it seems to us, the right glasses will be more certainly secured by the careful trial of two or three pairs, than by any such scientific investigation. The whole scheme reminds one of the ingenious but ponderous machine (devised, if our memory serves us, in Laputa) for drawing a cork.

ART. XII.—*Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fascic. III. With Four Plates.—*London,* 1848. Folio.

IT is rare, in literary matters, to find a man *better* than his word. Mr. Wilson only promised two parts a year; and we have a third within little more than six months after the first. This is a favorable indication of the past and augurs well for the future success of this truly meritorious publication. We shall only enumerate the subjects of the present part,—viz. Acne vulgaris, Erythema annulare palmare, Urticaria perstans, and Melanopathia syphilitica,—and state that its execution sustains the high character we have given to the preceding fasciculi.

PART THIRD.

Periscope.

ANATOMY AND PHYSIOLOGY.

Researches into the Comparative Structure of the Liver. By JOSEPH LEIDY, M.D.

THESE researches extend over the same ground as that previously trodden by Dr. T. Williams (Guy's Hospital Reports, vol. iv, N. S.), and by Dr. Handfield Jones, of whose inquiries we gave an account in our last Number. We shall confine ourselves, therefore, to a mention of the points which Dr. Leidy has been particularly successful in elucidating. The following is his account of the biliary tubuli of insects: "When more intimately examined, these tubes are found to consist of a delicate tube of clear, transparent, amorphous basement-membrane, the inner surface of which is covered with secreting cells. From the thinness of the tube, the cells often project, so as to give it a granulated appearance when viewed by the naked eye, as in the flesh-fly; and generally towards the free extremity the sides of the tubes are so irregular, that they appear as if merely folded upon the secreting cells to keep them together. The secreting cells are round, oval, or nearly cylindrical from elongation. Their average measurement is about ·09 millimetre. The contents are white, yellowish, or brownish, and consist of a finely granular matter, numerous fine oil-globules, a granular nucleus, and a transparent nucleolus. The cells in the extremity of the tubes are not more than half the size of those a little farther on, or nearer the termination, and contain less granular matter, and no oil-globules, so that they are more distinct, and the nucleus more apparent. Upon advancing a very little, the cells are found to be of an increased size, and full of granular matter, so as considerably to obscure the nucleus from view. A little further, we find the addition of fine oil-globules readily distinguishable by their thick black outline, when viewed in a certain focus. Sometimes the cells become so filled with oil as to be distended with it, rendering the granular matter and nucleus so transparent, as totally to destroy all appearance of the former, and the latter only is to be perceived in faint outline. The nucleus is generally central, globular, and pretty uniform in size in all the species, averaging in measurement about ·025 millimetre. Sometimes, where the cells are elongated, the nucleus is also irregularly so. The nucleolus is always transparent, and measures about ·006 of a millimetre. The central passage of the tubes, or separation of the cells in the middle line, is usually found filled with fine granules, and a great amount of oil-globules. The biliary tubes of insects are bathed in blood or the nutritive fluid, and the respiratory tracheæ are distributed to them with extreme minuteness, but are separated from the secreting cells through the intervention of the basement-membrane."

[The existence of this basement-membrane appears to us in many instances rather probable than demonstrable. The biliary tubes of insects frequently appear like strings of large rounded cells; and the basement-membrane, if present at all, must be of extreme tenuity, and must exactly follow the contour of the cells.]

A very similar account is given by Dr. Leidy of the biliary cæca in the Mollusca and Crustacea; in both instances, the cells at the blind extremity being about half the size of those near the mouth of the sac, and being much less charged with oil.

Dr. Leidy's observations thus confirm those of Professor Goodsir, as to the origin of the cells at or near the blind extremity, and their gradual development as they are pushed onwards towards the mouth, of the cæcal tube or follicle.

The most important part of Dr. Leidy's researches, however, relates to the liver of the higher vertebrata; the minute structure of which he seems to have been more successful than any previous observer in determining. In regard to the general arrangement of the lobules, and the distribution of the blood-vessels, he fully accords with Mr. Kiernan; and he has determined one point which was previously rather a matter of inference than of observation,—namely, the anastomosis of the capillaries of the hepatic artery with those of the vena portæ, within the lobules, so that both form one system of vessels, converging from the circumference towards the centre of each lobule, and giving origin to the hepatic vein. It is in regard to the ultimate distribution of the hepatic ducts, however, that Dr. Leidy's researches appear to be most satisfactory. They are in complete harmony with the sagacious prediction of Mr. Kiernan (founded on the distribution of the ducts in the left lateral ligament), that these canals terminate in anastomosing plexuses, which make up, with the blood-vessels, the entire substance of the lobules. "The lobules are composed of an intertexture of biliary tubes, and in the areolæ or interspaces of the network, the blood-vessels ramify, and form among themselves an intricate anastomosis, the whole being intimately connected together by a combination of the white fibrous and yellow elastic tissue. In structure the biliary tubes correspond with those of the invertebrata consisting of cylinders of basement-membrane containing numerous secreting cells; and the only difference exists in the arrangement, the free tubes in the vertebrata becoming anastomosed, or forming an intertexture. The tubuli vary in size in an unimportant degree in different animals, and also in the same animal; being generally from two to two and a half times the diameter of the secreting cells. The tubes of one lobule are distinct from those of the neighbouring lobuli, or only communicate indirectly by means of the trunks or hepatic ducts originating from the tubes and lying in the interspaces of the lobuli. The secreting cells are irregularly angular or polygonal in form, from mutual pressure, *and line the interior surface of the tubes*." The figures given by Dr. Leidy are so distinct, that we cannot but ask ourselves how this structure has escaped the notice of other anatomists, who, as is well known, have been unsuccessful in determining the relation of the hepatic cells to the biliary ducts. Dr. Leidy does not specify the mode in which his preparations have been made; but we understand that his plan is to *dry* a small portion of injected liver, then to take a very thin slice of it, and to examine this when restored to its original condition by being moistened. We hope that his observations will be speedily tested on this side of the Atlantic. If they prove correct, Dr. Leidy will be entitled to the merit of having solved one of the most interesting problems in Minute Anatomy; and we trust that this is but the first of many similar achievements.—*American Journal of the Medical Sciences*, Jan., 1848.

On the Contractility of the Smaller Arteries. By E. and E. H. WEBER.

THE recent experiments of Professors Weber on this subject have an important bearing upon certain questions adverted to in a former part of the present Number (p. 147). These experiments were made with the rotating electro-galvanic apparatus upon the arteries of the mesentery of frogs, between 1-7th and 1-17th of a Paris line in diameter. When vessels between these sizes were exposed to the electric stream, they did not instantaneously respond to the irritation; but in a few seconds they began to contract, so that their diameter, in from five to ten seconds, was diminished by a third, and their sectional area consequently reduced to about half. If the application of the stimulus was continued, the diminution of calibre continued also, until the diameter was reduced to one third or even one sixth of the original, so that only a single row of blood-corpuscles could pass along the tube at once; and at last the vessel became completely closed, and the

current of blood arrested, the original conditions being restored, however, soon after the withdrawal of the stimulus, unless the stream had been very powerful or long continued, in which case the vessel dilated beyond its former size, and lost for a time the power of contracting again. The effect of these changes upon the flow of blood is precisely what might have been anticipated upon hydraulic principles; *a marked acceleration of the current being observed in proportion to the narrowing of the tube*, and a retardation with its dilatation. When the electric stimulus was applied in like manner to the capillaries, it produced no change in their diameter; but it operated in a remarkable manner on the blood. The blood-corpuscles seem to have an unusual adhesion to each other, and to the walls of the vessels, so as to produce a great amount of friction and a consequent retardation; and the continual arrival of new corpuscles produces an accumulation that completely fills the vessels of the part, and thus occasions a total stagnation. When the stimulus is withdrawn, the blood-corpuscles disperse again after a time, and the current is renewed. When the larger arteries were experimented on in a similar manner, no evidence of contraction could be obtained.

These experiments are in full accordance with the observations of Hunter, in regard to the presence of a proper contractile tissue in the smaller arteries; and are more satisfactory than any chemical or microscopical evidence yet adduced. —*Müller's Archiv*, 1847, No. 2.

On the Independent Contractility of Muscular Fibre. By Dr. E. Harless.

A new set of experiments performed on this subject, upon animals rendered completely insensible by ethereal inhalation, confirms the opinion that the irritability of muscle is a property inherent in the tissue itself, and not in any way derived from the nervous system. Dr. Harless found that even when the nervous system had been rendered, by the action of ether, utterly incapable of conveying a galvanic stimulus, applied either to the nervous centres or the nerve-trunks, the same stimulus, applied directly to the muscles, would immediately throw them into powerful contraction.—*Müller's Archiv*, 1847, No. 2.

These results are in harmony with those obtained by Dr. Madden some years since, and communicated to the British Association at its meeting in Edinburgh, on the agency of narcotics in destroying the power of nervous conduction, without diminishing muscular contractility in an equal degree. We hope that they will tend to convince Professor Müller and other German physiologists, of the untenability of the doctrines they have upheld as to the dependence of muscular contractility upon nervous agency.

On the Sarcina of Goodsir.

The sarcina has recently attracted considerable attention in Germany. Virchow, in a memoir on this subject, criticises the different theories that have been promulgated regarding it. Schlossberger (Würtemb. Correspondenzbl., 1846, No. 26), who seems to have been the first continental observer who noticed the observations of our countrymen Goodsir and Busk, regarded the sarcina as nothing more than minute fragments of disintegrated muscular fibrillæ. The microscopic measurements of Hasse and others at once demonstrate the incorrectness of this view; the breadth of the sarcina being nearly double that of the primitive muscular fibrillæ. Further, if the sarcina and fragments of muscle are simultaneously examined under the microscope, it will be seen that, on the addition of acetic acid, the latter almost entirely disappear, while the former, if at all affected, is only rendered a little paler. Other proofs of their difference might also be adduced. The sarcina has been found in the fluid contents of the stomachs of persons who have altogether abstained from an animal diet, and in the stomachs, hepatic ducts, and gall-bladders of rabbits. It has likewise been found in the lungs of the human

subject. Its presence in the stomach gives rise to no peculiar symptom, and the occurrence of vomiting in some cases seems only accidental.

He concludes with the following *résumé*:

1. The sarcina is not proved to be a product of decomposition.

2. It stands in no definite relation to the process of fermentation, or indeed to any marked process.

3. Its cellular nature is not proved; although it appears probable that it should be classed with the lower kinds of vegetable forms.—*Archiv für Pathologische Anatomie und Physiologie*, Ersten Bande, Zweites Heft, p. 264.

On the Digestion of Alcoholic Fluids, and their Office in Nutrition. By MM. BOUCHARDAT and SANDRAS.

AFTER detailing several experiments and observations made upon men and animals (especially granivorous birds, on account of the facility with which they can be got to take alcoholised substances), the authors of this paper arrive at the following conclusions:

As in fatty bodies, the first stage of digestion properly so called, viz. solution, is wanting; alcohol undergoing no other change than some degree of dilution by the gastric, buccal, and other fluids of the alimentary canal. As already shown by M. Magendie, they are absorbed through the medium of the veins. Such absorption takes place especially in the stomach, although when these drinks are given in great excess, or mixed with sugar, it may also take place throughout the whole intestinal canal. The chyliferous vessels take no part in the absorption, and if the drinks have been given with fatty matters, chyle may be abundantly collected, but exhibits no appreciable trace of alcohol. When the alcoholic drinks have entered the current of the circulation, alcohol is eliminated by no part of the secretory apparatus—a small portion only evaporating by the lungs. If alcohol be introduced in too large quantities into the circulation, the arterial blood retains the venous colour, and all the phenomena of asphyxia may be produced. Alcohol, under the influence of the oxygen incessantly introduced into the economy during respiration, may be immediately converted into water and carbonic acid; but in some of the experiments, an intermediate product of its combustion, acetic acid, has been obtained; alcohol, and the products derived from it, disappear rapidly from the economy. When it is introduced simultaneously with glucose and dextrine, it is destroyed much more rapidly than these bodies.—*Annales de Chimie*, tom. xxi, pp. 448-57.

On the Changes that the Blood-Corpuscles undergo in the Spleen. By Profs. ECKER and KÖLLIKER.

DR. ECKER, of Basle, and Professor Kölliker have simultaneously been engaged in investigations on the nature of the cells which occur in the spleen of various animals, and which contain blood-corpuscles and yellow granules. Repeated observations convinced Dr. Ecker that the occurrence of these bodies was perfectly normal; and further, that the blood-corpuscles in the spleen undergo a normal disintegration. He found in the spleen of rabbits, dogs, sheep, and calves, besides the known cells and nuclei, other cells inclosing blood-corpuscles. Thus, for instance, in the calf, cells were observed of about 0·007 millim., containing *one* blood-corpuscle, and a perfectly pale and finely granular mass. On the addition of water, the cell burst, the blood-corpuscle escaped, and, after growing paler, disappeared. Other cells (of about 0·010 millim.) contained *two* blood-corpuscles, and besides these, some also contained a granular nucleus, exhibiting a perfect resemblance to the ordinary cells of the spleen; whilst others again inclosed only a fine granular mass, but no nucleus. Other cells contained *three* or *four*, or sometimes even *ten*, or more blood-corpuscles, of about 0·015 to 0·030 millim. The form of these cells is sometimes round, sometimes irregular; they sometimes contain a nucleus, some-

times not; the cellular membrane is generally very distinct. In some cells, instead of blood-corpuscles, there are only yellow, or brown, or blackish granules, which, as is evident from the stages of transition, are owing to a disintegration of the blood-corpuscles. Thus we find large cells (0·030 millim.) filled with shrivelled, yellow blood-corpuscles, which do not change in water, and are filled with yellow granules. In the free blood-corpuscles numerous transitions from the normal to the shrivelled state are observed, and nowhere are more important differences in the size of the blood-corpuscles exhibited than in those of the spleen. An investigation into the nature of the blood-corpuscles in the spleen of frogs and tritons, showed Dr. Ecker the occurrence of precisely similar changes; and in speaking of the signification of these alterations, he adduced two hypotheses, which had at first sight appeared to him to furnish an equally satisfactory elucidation of the subject. 1. That the blood-corpuscles, either singly or several together, become inclosed in cells, within which they are disintegrated; or already disintegrated blood-corpuscles become inclosed by cells. This fact is not without analogy, since in the brain and the thyroid gland similar changes of the blood occur under different pathological conditions, as in effusions. The destination of these cells is not known with certainty, but as Dr. Ecker found them in the blood of the splenic vein (in a calf), he conjectures that they reach the liver with the portal blood; and that a separation of the blood which has lost its vitality is effected in some way by the bile. 2. The second hypothesis, the validity of which Dr. Ecker has, however, found reason to doubt since his investigations have been solely directed to the changes observed in the spleen of mammalia, was as follows: That the cells which inclose one or more blood-corpuscles are converted into lymph-corpuscles, the lymph being changed by the addition of blood-constituents. The red colour of splenic lymph appeared at first sight to corroborate this view, but subsequent investigations showed that no such bodies were to be found in the splenic lymph of a calf. It would appear that the separation which occurs in the vesicles of the spleen serves the same end as the vesicles of the other blood-preparing glands. Kölliker has extended his observations on these changes of the blood-corpuscles to so large a number of the classes of vertebrata, that we may consider them as of general occurrence in all these animals.—*Henle u. Pfeufer's Zeitschrift für rationelle Medizin*, Bd. vi, Heft 2.

On the First Process which takes place in the Impregnated Ovum of the Frog—Cleavage or Division, and its Products. By Dr. H. Cramer.

In ova that have been laid, the germinal vesicle has disappeared, and its cells are dispersed through the whole contents of the yelk. It would appear that the membrane of the vesicle becomes dissolved, as no fragments of it can be detected, which ought to have been the case if rupture had occurred. By carefully opening a fresh-laid ovum, and examining it under the microscope, there may be seen moving among the yelk-corpuscles transparent specks (*flecken*), which are recognised as floating cells, as, in fact, the bodies which are developed in the germinal vesicle. Dr. Cramer then describes the composition of the yelk, when, by cleavage, it has acquired the blackberry-form noticed by Baer. He finds it contains a multitude of round or oval bodies of large size, filled with yelk-particles, and surrounded by an elastic, delicate membrane. These bodies likewise contain in their interior from two to four of the cells above mentioned, as developed within the germinal vesicle. These large bodies, by a process of subdivision, become smaller and smaller; and although at first each separates into two, subsequently one body may divide into three or four portions or cells, each still containing, like their parent, yelk-particles and cells derived from the germinal vesicle. This process has been noticed, more or less accurately, by other observers, and takes place, as is well known, equally in the vegetable and animal kingdoms. It is evidently one of the most important acts of development, inasmuch as the last set of corpuscles, produced by the process of cleavage, are transformed into the primitive tissue of the embryo, the first product being a delicate membrane, composed of a layer of

united polygonal cells. The author proposes to call these minute formative corpuscles "embryonic cells," and the larger bodies above described "yelk-bodies or globules."

On the development of blood-corpuscles. The first blood-corpuscles which are seen circulating in the branchiæ of the tadpole are, according to Dr. Cramer, embryonic cells; thus he found, on slitting up and turning back the delicate membrane forming the walls of the heart, a collection of embryonic cells, which it cannot be doubted are the particles first seen circulating. In the beginning, these corpuscles are filled with germinal vesicle cells; subsequently these cells disappear, but their finely granular contents remain, and accumulate towards the centre of the corpuscle. With this change in their constitution an alteration of form takes place, the corpuscles, which were originally round and globular, becoming oval and flattened (this applies, it must be recollected, to the tadpole), but still presented a central projection, which is more distinct than it is in the fully formed blood-corpuscle, in consequence of the central granular mass being larger than the future nucleus. The cells or corpuscles now begin to be coloured slightly, their central granular contents more and more disappear, till there only remains a small mass of a fine powder, which is transformed into a nucleus, presenting more definite borders than in the complete corpuscle, because of the comparative absence of colour. The appearance of the corpuscle in this stage, as depicted by the author, somewhat resembles that of a blood-disc of the frog when treated with acetic acid. He also describes and represents by figures the development of red corpuscles in the frog, from the lymph-corpuscles.—*Müller's Archiv,* 1848, p. 20.

In his recent lectures on the blood at the College of Surgeons, Mr. Paget stated, as the result of observations carried on by himself in conjunction with Dr. Kirkes, that the *first* blood-corpuscles are directly formed from the embryonic cells, in the warm-blooded vertebrata as well as in reptiles; but that these are afterwards superseded by another series formed from the lymph-corpuscles. The period at which this change takes place in the tadpole corresponds with the disappearance of the external branchial apparatus; and if this stage of the metamorphosis be retarded by cold and darkness, the introduction of the new corpuscles is retarded also. The same change takes place in the embryos of higher animals, at the time of the closure of the branchial fissures in the neck.

ORGANIC CHEMISTRY.

On the Blood of the Splenic Veins and of the Vena Portæ. By M. BECLARD.

AFTER long reflection upon the functions of the spleen, it occurred to M. Beclard, that an examination of the blood in the splenic veins might throw light upon this; and, in a letter addressed to M. Dumas, he furnishes an account of the results invariably arrived at in eight experiments performed on dogs.

1. The blood returning by the splenic vein, prior to its junction with the vena portæ and veins of the stomach, contains a smaller proportion of globules than arterial or even venous blood in general. 2. The quantity of albumen is proportionably increased—and, indeed, the disappearance of the globules can be conceived of with difficulty, without their principal constituent element being thus found to be present. 3. I have sought for the constant source of the renewal of the globules, which appeared to be thus incessantly destroyed in the spleen. I first thought of the lungs, and, as all the blood of the organism traverses them, the entire question resolved itself into this,—whether a notable difference exists between arterial and venous blood, in respect to amount of globules. A larger proportion does exist in arterial blood, but to so slight an extent, as to be explicable by other causes; so that I abandoned the idea of the lungs being the exclusive organ for the formation of globules. 4. The arterial blood examined at different points of the circulation, is found to present an identical composition in the same animal; while the venous

blood differs in respect to its proportion of elements, according as it is examined in different regions. The blood of the splenic veins is an example; but to my surprise I found that the amount of globules in that of the vena portæ, prior to its junction with these, so far from being diminished, was considerably increased; and so abundant is the supply of globules thus brought from its radicles to its trunk, that intestinal absorption at once presents itself as an explanation of the accumulation. The following table exhibits the differences as observed in four successive bleedings performed on the same dog:

	Ext. Jugular.	Mammary Artery.	Splenic Vein.	Vena Portæ.
Water	778·9	750·6	746·3	702·3
Albumen	79·4	89·5	124.4	70·6
Globules and Fibrine .	141·72	159·9	128·9	227·1

Annales de Chimie et de Physique, 1847, tom. xxi, p. 506-8.

At the sitting of the *Académie des Sciences*, Jan. 17, 1848, M. Beclard made an additional communication upon this subject, and after referring to the above experiments, added, that he had since made 36 comparative analyses between the blood of the splenic and the external jugular veins; selecting the latter as the most convenient representative of the mean composition of venous blood. These also showed a less proportion of globules, a larger proportion of albumen, and a somewhat larger one of fibrine, in the blood returning from the spleen. "So that the spleen, so far from being an organ for the formation of globules, would appear to be the place of their destruction." To prevent error, the blood of the vena portæ was examined in its intestinal branch, (sup. mesenteric) and the proportion of its elements was found to undergo an extensive variation. Thus, 1st, during the early period of the digestive absorption, the quantity of albumen is considerably increased; and 2d, the same occurs at a later period with respect to the globules; while 3d, if the animal has been subjected to long abstinence, the proportion of constituents is the same as that found in the general venous system. "It results from these experiments on the blood of the vena portæ, that the globules commence and finish in the same system. While, in fact, the intestinal branch of the vena portæ brings the new globules to the common trunk, the splenic branch transmits to it the vestiges of those which have been destroyed in its interior. The blood of the vena portæ not containing a larger proportion of fatty matters than the venous blood in general, and chyle differing from lymph especially by the presence of fatty matters, it is, if not demonstrated, at least very probable, that albuminoid matters enter the blood by one sole channel, the vena portæ. On the other hand, these same matters enter the blood under one form alone, that of albumen. Finally, as the transformation of albumen into globules takes place in the vena portæ, and this only some hours after the commencement of digestive absorption, it results that the blood circulating in the portal system is not subjected to the general laws of the circulation. I shall examine this important physiological point in my next Memoir."—*Comptes Rendus*, tom. xxvi, pp. 122-4.

On the Constitution of Taurine. By Prof. REDTENBACHER.

WHEN Redtenbacher announced the presence of sulphur in taurine, he observed that on oxidizing it with caustic potash, and decomposing the residue with dilute hydrochloric acid, sulphuretted hydrogen and sulphurous acid escape, while sulphur is deposited in just the same manner as if pure sulphur had been treated with potash. He now observes that this reaction gives us an insight into the constitution of taurine. Heated cautiously with potash, the taurine parts with sulphur in the form of sulphurous acid; this proves that it is contained as sulphurous acid in taurine; for if it existed as unoxidized sulphur, or as hyposulphurous acid, sulphur would be eliminated in decomposing the potash-compound; if it existed as sulphuric acid, no sulphurous acid should be disengaged; and the presence of hyposulphuric acid is impossible from the small number of atoms of oxygen.

Taurine contains, therefore, two atoms of sulphurous acid; and deducting them we have $C_4\,N\,H_7\,O_6\,S_2 - O_4\,S_2$, or $C_4\,N\,H_7\,O_2$, which exactly contains the elements of aldehyde-ammonia, $C_4\,H_3\,O + N\,H_3 + HO$. In the reaction the ammonia escapes as gas, and the aldehyde is oxidized, and combines with the potash as acetic acid. Taurine is therefore *bisulphite of aldehyde-ammonia*, in about the same stage of condensation as cyanate of ammonia in urea. Redtenbacher concludes by showing that such a salt may be readily formed, although in a less condensed state than in taurine, and gives analyses showing its isomorphism with taurine. He failed, however, in condensing it into the last-named substance.—*Liebig in Wöhler's Annalen*, Jan. 1848.

On the Action of Diuretics. By Professor KRAHMER, of Halle.

FROM a series of experiments, the results of which are recorded in the following table, Professor Krahmer seems to have established, beyond all doubt, that the ordinary so-called diuretics are totally devoid of any physiological action.

Tabular view of the average daily amount of the urine and its constituents (expressed in grammes).

	Urine.	Solid residue.	Combustible portion.	Ashes.	Urea.	Uric acid.
Mean of all the observations (103)	2029·3	73·13	38·39	34·69	19·46	0·35
Do. of those when no diuretic was taken (62)...	2084·6	74·01	39·65	35·24	19·64	0·36
Do. after the use of distilled water (1).........	4920·7	70·70		34·69		
Do. after the use of diuretics (40)..............	1871·2	71·77	37·93	33·83		
Pulv. bacc. junip. (4)	1759·1	65·88	29·32	36·56		
Terebinth. (4)	1277·2	60·37	34·52	25·85		
Pulv. rad. scill. (6)	1760·4	69·96	32·38	37·58	15·82	
Pulv. fol. digitalis (11)......	2103·1	76·16	39·34	36·82	19·71	0·23
Pulv. rad. rhei (6)......	1899·4	73·56	41·85	31·71	19·46	0·35
Resin. guaiac. (5)...........	2114·0	75·39	42·77	32·62	22·74	0·29
Tinct. sem. colchici (5)......	1756·5	71·99	42·26	29·73	22·84	0·69

Referring to Becquerel's tables as a standard of comparison, we find the corresponding figures 1319·8 36·8 27·6 9·6 16·5 0·5 which are most of them considerably lower than the above numbers of Krahmer. The subject evidently requires further investigation.—*Journ. für. prakt. Chemie*, Bd. xli, pp. 1-7.

Observations on the Relation of Acids to the Animal Economy. By M. MIALHE.

IN a former article upon the abuse of *alkaline* medicines, M. Mialhe laid down the following propositions:—1. That the greater part of the humours of the economy being normally alkaline, the abuse of alkalies does not rapidly produce serious accidents, as they do not change the nature of the medium in which the chemical changes are effected. 2. The normal alkalescence in animals contrasts them with vegetables, whose constitution is naturally acid. 3. The alkalescence of the fluids is necessary for the decomposition and assimilation of hydro-carbonated amylaceous substances. 4. The employment of alkalies, generally useful, is only dangerous for those whose vital fluids are already supersaturated with them, as is the case in certain pathological conditions, and among the inhabitants of the country, who, during their laborious occupations, throw off much acid with their perspiration, or who live upon an entirely vegetable regimen.

From the above statement it follows, that if the naturally alkaline fluids become neutralized, and still more if they become acid, functional disorders will

result. The vital fluids whose composition becomes thus perverted are no longer fitted to produce the necessary interstitial changes and intimate modifications. The economy, at first, struggles against this condition, but ere long yields to it; and debility, wasting, and serious diseases, as pyrosis, gravel, gout, scorbutus, or diabetes (diseases far more nearly connected together than is generally supposed), may result, and prove impossible of relief as long as the acidity prevails.

A superabundance of acid may arise in the economy from various sources. 1. During the heats of summer, many persons make an immoderate use of acidulated beverages. 2. The diet may be too azotized. Meats, by reason of their albuminoid matters, contain much sulphur and phosphorus; and these bodies, during their combustion in the economy, give rise to large quantities of sulphuric and phosphoric acid. 3. The most important of all the sources is bodily inactivity, in consequence of which a deficiency in the excretion of acids by means of the perspiration exists. Thus we find uric acid, gout, and diabetes especially prevailing among those classes whose diet is too animalized, and bodily exertion defective. This has long been recognised to be the case in gout and gravel, and is no less true as regards diabetes, a disease of far more common occurrence than is suspected. The glucose derived from feculent matters cannot be decomposed or assimilated in the absence of alkalies, and diabetes is a result. Herbivorous animals, whose fluids are still more alkaline than our own, never suffer from this disease.

The peasant in the country, sweating enormously, and at the same time increasing the alkaline state of the humours by his vegetable regimen, can bear large quantities of acid drinks. So, too, the inhabitants of hot countries eat with impunity enormous quantities of acid fruits. The acids of fruits, however, always are less dangerous than the free acids, as they are in part united with alkalies, and their salts undergo changes in the blood, which transform them into carbonate of potass. Wherever inactivity or cold diminishes the cutaneous secretion, the ingestion of acids becomes especially dangerous; and thus gout or diabetes, which had disappeared, may be reproduced by a low temperature alone.

If certain waters are more favorable than others to health, a fact which did not escape the Romans, this is due to the presence of a marked proportion of bicarbonate of lime. Such waters immediately saturate superfluous acidity, facilitate the decomposition of glucose, and are especially adjuvatory to the formation of bone and the prevention of rickets.

A question has of late much occupied chemists, viz., why the organic acids should pass into the urine and the mineral acids not. That portion of the former of these which remains after saturating the alkalies, may pass at once off by the urine; but the mineral acids being endowed with the property of coagulating albumen, which the others are not, can never reach very far, since at every step they are absorbed by the tissues and the fluids which impregnate them. The resulting coagulum may, indeed, gradually yield its acid to the alkaline bases of the blood, and the acid so saturated may pass into the urine as a salt, but it can never reach the fluid in its free state. "So much do I rely upon this explanation, that I am certain that phosphoric acid, in spite of its mineral origin, would reach the urine in its free state; for, unlike the other mineral acids, it does not coagulate albumen, and in that respect is analagous to the organic acids."—*L'Union Médicale*, 1848, No. 22.

PATHOLOGY.

A Description of the Malignant Pustule as communicated from the Elephant. By Elijah Impey, Esq.

This is a very interesting paper, detailing the history of four cases of a peculiar local affection (complicated with constitutional disorder) resulting from participation in the dissection of an elephant. The disorder of which the animal died was apparently inflammation of the trachea, accompanied by extreme congestion of the

vessels of the neck, and by profuse serous effusion into the subcutaneous areolar tissue, and even amongst the muscles. The thorax and abdomen, however, were not examined; the swelling of the throat having been the prominent symptom during life. Five persons were concerned in the dissection, which was performed within six hours after death. The two who suffered least were employed in separating the head, and were literally up to their elbows in blood, but had no abrasion of the skin; the same was the case with a third, who amused himself by detaching a hind-leg; whilst the serjeant-major, who suffered most severely, was struck by a small splinter on the cheek, while hewing at the jaw-bone, to remove the grinders. The Moochee could not be induced to touch the animal without smearing his hands and arms with oil; and he escaped uninjured.

All the four, who had not taken this precaution, were speedily affected with local sores, the seat of which was on the hands or fore-arm, except in the last case, in which it was (as might be expected) the cheek that suffered. This sore first presented itself as "a vesicle, on rupturing which an ichorous watery fluid escaped; and the cuticle being removed, exposed beneath an ashy, dry, firm slough, the size of the base of the vesicle, or local irritation, also oval, and very adherent, and tenacious in its whole extent. The margins of the sores which bounded these were smooth, soft, and well defined, the discharge sanious and clear, but trifling, and the base, except in one instance, free from hardness or tumefaction. The surrounding integuments were either affected with erysipelas and inflammation of the lymphatic, whose course was marked by a broad riband of angry redness up to the axilla and neighbouring glands, or by an angry œdema. In the latter case, effusion into the interstitial cellular tissue took place very rapidly and extensively, reaching down from the right cheek to the mammary and hypochondriac regions, and into the reticular texture of the eyelids and lips, the apertures of which it almost totally closed; but this œdema never itself took an erysipelatous inflammation, or showed a disposition to vesicate. The method of ulceration was slightly phagedenic, but not rapidly so; the sore spreading in most cases seemingly by mere extension, as if from the ichor of its own discharge, the first slough not being detached, or any other change apparent at the next dressing beyond the enlargement of the sore, which retained its oval or circular form, though occasionally it was quite stationary. The powers of the parts in the vicinity were at all times languid, evincing no disposition to throw off the sloughs." . . . "The depth to which these sloughs extended was very remarkable; a small sore on the distal extremity of the first metacarpal bone of the right hand, not a quarter of an inch in diameter, having laid bare the tendons of the *extensor communis* and *extensor indicis*, as well as the bone underneath. The cavity and form of the sore, on the detachment of the slough, was always smooth and cup-shaped, as if the piece had been scooped out."

The constitutional disorder seemed divisible into three stages. "The 1st, febrile, occurring a few hours after the first perception of the local irritation, and ceasing on the rupture of the vesicles or detachment of the cuticle, occupying altogether from four to eight or nine hours. The 2d more prolonged, terminating at the separation of the eschar, between the eighth and fifteenth days; in this stage, secondary fever supervened irregularly, but for the principal part of the time the patients were free from it. The 3d stage was denoted by the healing-up of the sores, and was still more prolonged than the preceding; nearly three weeks elapsed before the smallest filled up, with the assistance of stimulants and dietetic measures, a plenitude of which the system markedly required, from the sallow phlegmatic countenance that attended the latter stages." All the patients being plethoric, antiphlogistic measures were adopted in the first instance, with the beneficial effect of subduing the febrile irritation; but we question whether the last stage may not have been prolonged by this lowering treatment. The local treatment consisted in the application of powerful escharotics. The sores were *circumscribed* with nitrate of silver, apparently with the good effect of preventing the extension of the vesication and sloughing; and potassa fusa, and in one instance nitric acid, were applied to the sloughing surface itself. All the cases did well in

the end, though the recovery was tedious.—*Bombay Medical and Physical Transactions*, No. viii.

This disease appears to us by no means identical with that to which the name of 'pustule maligne' has been given, though analogous to it in its character and course. The constitutional disturbance was much less than is generally observed in the case of animal poisons thus introduced into the blood. The character of the ulcerations is very similar to that described as resulting from the contact of the juice of the Manchineal tree (one of the Euphorbiaceæ); even the droppings of rain from its leaves are said to have the same result.

Notes on Smallpox in India. By C. MOREHEAD, M.D.

THIS paper is chiefly interesting as furnishing a valuable statistical contribution in regard to the dependence of the prevalence of epidemics upon *season*. The following tables show the number of *cases* of smallpox admitted each month into the Native General Hospital at Bombay, between the years 1829 and 1844; and the number of *deaths* which took place from smallpox at Calcutta during each month of the year 1844. Similar results were afforded by the epidemics of former years.

	Bombay. Cases.	Calcutta. Deaths.		Bombay. Cases.	Calcutta. Deaths.		Bombay. Cases.	Calcutta. Deaths.
Jan.	38	157	May.....	41	375	Sept. ...	2	6
Feb.	76	455	June....	27	150	Oct.	0	2
March..	143	963	July.....	38	45	Nov. ...	5	*
April ...	58	756	Aug.....	3	13	Dec.....	14	*

It would thus seem to be the law of epidemic smallpox in India, that its seasons of prevalence are the winter and spring, and those of absence the summer and autumn; and it is curious that the maximum in both tables should present itself in the month of March, thus corresponding with the observations of Sydenham and Huxham respecting the prevalence of smallpox epidemics about the vernal equinox. Our own Registration-Reports usually show a marked difference in the number of deaths between the first and second halves of the year; but this difference is far more strongly marked and more constant in India; and the indisposition to the propagation of smallpox in the hot months there manifests itself in a very remarkable manner,—namely, *in a difficulty in propagating the cow-pox* during that part of the year.—*Bombay Medical and Physical Transactions*, No. viii.

On the Morbid Anatomy of the Intestinal Mucous Membrane in Infants. By DRS. FRIEDLEBEN and FLESCH.

Drs. Friedleben and Flesch have contributed an able memoir on this subject, illustrated by numerous cases. They adopt the following arrangement:

A. *Congestion.*
  a. Congestion of the mucous membrane itself.
  b. Congestion of Peyer's glands.
B. *Acute inflammation.*
  a. Primary acute inflammation of Peyer's glands.
  b. Secondary acute inflammation of Peyer's glands.
C. *Chronic inflammation of Peyer's glands.*
D. *Ulceration of the solitary glands.*
  a. Primary ulceration.
  b. Secondary ulceration.
E. *Softening of the mucous membrane.*
  a. Red softening.
  b. White softening.

They conclude with the following *résumé:*

* For the months of November and December there was no return from Calcutta.

1. The changes in the intestinal mucous membrane in infants are very frequent; in fact, the most frequent of any necroscopic appearances.

2. These changes are partly chronic, and are the special causes of atrophy, and partly the controlling causes of the acute and exhausting diarrhœa, and other forms of diseases comprised under the head of softening of the stomach, and which are so frequently attended by important cerebral symptoms.

3. These changes, under the circumstances indicated, are more frequently met with in the dead body than those which are usually instanced as of common occurrence, viz. enlargement of the mesenteric glands, softening of the stomach, the formation of aphthæ in the intestinal canal, &c.

4. In a similar manner, these changes are far more frequent than those of the stomach, which, with the exception of softening of the cæcal extremity, are very rarely observed at this period of life.

5. Changes of the mucous membrane, excepting those of a secondary nature, are very strongly characterised.

6. Chronic inflammation of Peyer's glands is the most frequent change that we meet with; and it is likewise the most common anatomical cause of atrophy.

7. Next to this, but much less frequently, occur red and white softening, as causes giving rise to atrophy.

8. Red and white softening are only different stages of the same morbid process; simple and gelatinous softening constitute only a variety of form.

9. Chronic ulceration of the solitary glands of the small intestines is much less frequent in atrophy.

10. Acute inflammation of Peyer's glands is an extremely dangerous form of disease, to which sufficient attention has not hitherto been directed.

11. This disease is a true phlogosis, and is likewise specially induced by concomitant diseases (as, for instance, croupous lobular pneumonia).

12. Most authors appear to be ignorant of this disease, and the few who have noticed the changes to which it gives rise, erroneously class it with dothinenteritis, a disease which, however, does not occur in this early period of life.

13. Secondary acute inflammation of Peyer's glands and inflammation of the solitary glands, which are generally of simultaneous occurrence, constitute a symptom of tuberculosis. We have always found, in these cases, tuberculosis of the spleen, but never of the intestinal canal.

14. Colitis, on which French authors so strongly insist, is frequently observed, but always limited to small sections, and it is evidently an affection of much less importance than the simultaneously occurring changes of the mucous membrane of the small intestine.

15. The mesenteric glands, with the exception of a slight redness and tumefaction in some few cases, are generally in a normal condition; and even where disease exists, it is not very important or extensive. It is only in the case of general tuberculosis, that these glands are frequently found to be in a state of partial tubercular infiltration.

16. The peculiarity and frequency of the changes in the intestinal canal on the one hand, and the non-occurrence of many important diseases, as typhus abdominalis, intestinal tuberculosis, &c., constitute one of the most prominent characters of the pathology of the intestinal mucous membrane in infancy.—*Henle u. Pfeufer's Zeitschrift f. ration. Med.*, Vol. v, Heft 3.

On Allotriophagia, or Endemic Pica. By Dr. VOLPATO.

DURING the investigation which has been recently going on in Italy of the causes of Pellagra, it was discovered that large numbers of the children of the lower orders indulge in the habit of eating dirt, cinders, chalk, and the like—so that the propensity in some parts has become endemic. At the meeting of the *Scienziati* at Venice, Dr. Volpato read a paper upon the subject, under the designation of *Allotriophagia* (αλλοτριος improper) and the diseases it produces; in which he gives

some particulars of the cases which have been met with in certain fertile districts of Castelfranco, numbering 11,140 inhabitants. By the peasants, the children are regarded as the subjects of witchcraft, and superstitious practices rather than theurapeutical resources are resorted to by them. Of 226 cases, 111 were males, and 115 females, the great bulk belonging to the agricultural classes. In 124 cases, the parents are reported as healthy, in 20 as allotriophagi themselves, and in 75 as pellagrinous. In 192 cases, the previous habit of body was good. The epochs of life were from 6 to 12 months in 8 cases; from 1 to 2 years in 134; from 2 to 4 years in 41; and from 4 to 12 years, and upwards, in 43. The habit had been continued from 1 to 12 months, or more. The diseases which succeeded this habit were of a gastro-enteric nature in 193 cases, chlorotic in 205, pellagrinous in 105. When the habit has been long indulged in, the countenance assumes a peculiar expression of features, the skin becomes of a dirty white or yellow, digestion is impaired; and dyspnœa, palpitation, œdema, and impeded development follow.—*Gazetta Medica Lombardia*, 1848, No 5.

On Puerperal Fever. By Prof. MARTIN.

Professor Martin, of Jena, concludes an able memoir on this subject, with the following summary:

1. The term puerperal fever is one which deserves to be retained in our medical nomenclature, since all the febrile diseases incidental to childbed originate in one common cause.

2. This common cause may be traced to the peculiar character of the blood of women in childbed.

3. Besides this general cause, which is necessarily incidental to the puerperal condition, there is another special incidental cause, not of invariable, although of very frequent occurrence, which is connected with the simultaneous deviations in the character of the blood that are observed in many cases, and which appears to influence the nature of the febrile condition, and in part, likewise, the mode and locality of the deposition.

4. These incidental deviations in the character of the blood are the causes of the epidemic occurrence and difference of the puerperal fever; but they may also be induced in some women by individual circumstances, in which case, they affect the symptoms, course, and termination of the disease in the greater number of those affected at the same period, although there may not actually be any epidemic at the time.

5. Such sporadic cases of puerperal fever do not, however, necessarily presuppose a special disposition, since any morbid exciting cause may induce puerperal fever, where there is no other predisposing cause than that incidental to childbirth generally.

6. A distinction between the differences in the character of the fever is fully as important for the prognosis and therapeutics of the disease, as for the separation of the individual local affections, which, in their turn, influence the different forms of puerperal fever.

7. These local affections consist in the derangement of certain constituents of the blood, or in the transformation of the blood itself, and vary considerably, according to the epidemic or sporadic character of the blood on the occurrence of the disease, whence plastic depositions and softening of the tissues (in consequence of infiltration with serum) may equally occur.

8. Local affections are most frequently met with in the interior portion of the sexual organs, especially in the uterus; but they likewise often occur in remote parts of the body, without there being any evidence of a previous uterine derangement. These affections are not, therefore, of special importance with reference to puerperal fever.

As the principal objects for future inquiry into the nature of puerperal fever, the author mentions the following:

1. Chemico-microscopical examination of the blood, urine, sweat, &c., of women in childbed, pursued simultaneously in the case of many, and renewed at different times, with a comparison of the results of investigations carried on simultaneously with reference to the blood of healthy women not pregnant, and not immediately after childbirth.

2. Chemico-microscopical investigations of the blood, the secretions and excretions of women suffering under puerperal fever, carried on simultaneously in the case of many, or at different periods, and at different epochs of the disease, having constant and special reference to the symptoms, mode of treatment, and the termination of the disease.

3. A careful investigation of analogous dyscrasic processes of exudation, as for instance, of rapidly fatal termination of peritonitis exsudativa in scrofulous and gouty persons.

4. A more careful distinction between the different febrile characters, that is to say, between the peculiar varieties in the symptoms and exudations observed in the case of one and the same local affection, and a comparison of these with the individual character of the blood. And finally:—

5. A distinction between individual local affections, and between the different forms of puerperal fever.—*Henle u. Pfeufer's Zeitsch. für rat. Med.*, Vol. v, Heft 4.

PRACTICAL MEDICINE.

Practical Remarks on Croup. By Dr. H. ZERONI.

THE most ordinary form of croup is *Congestive Croup.* This form generally occurs at a time when there is a tendency in the weather to induce catarrhal affections, manifesting itself suddenly, generally in the night, and without any precursory symptoms; with the exception, perhaps, occasionally of a slight cold in the head. In this form of the disease, children wake from a quiet sleep with a sharply barking kind of interrupted cough, raise themselves in bed, and begin to cry in apparent distress, a piping hissing inspiration being occasionally heard, as well as in their coughing. The countenance is often flushed and turgescent, but the respiration is not hurried, and but little febrile excitement is perceived in the pulse. When the attack subsides, the child again falls asleep, and rests quietly till the morning; occasionally, however, the attack recurs, respiration becomes more noisy, rattling and hissing, and the child either vomits, or simply makes an effort to do so. This form of the disease requires little more than careful nursing for its cure; the child should be kept in bed, be made to drink copiously of warm drinks, partake freely of some oily emulsion, and have a sponge steeped in hot water laid on the neck. If, however, a hissing hurried respiration causes apprehension of a recurrence of the attack, the most effectual means is to give an emetic, continuing its use until the child has vomited several times. (This form of the disease appears to be induced by an hereditary and acquired predisposition.) To young children during the first year, Dr. Zeroni gives ¼ gr. cupr. sulph. every quarter of an hour.

The second form is *Inflammatory Laryngeal Croup.* This is far more serious in its nature, and *never* occurs without premonitory symptoms, or where it may not be referred to the action of some injurious influences. It may have been induced by the preoccurrence of the milder form, or owing to exposure to bad weather immediately after recovery from a former attack. The characteristic symptoms of this second form of croup are as follows: Broken, rough, whistling cough; the inspiration is quick, and has a sharp sound; the child is restless, moves the hands, bringing them frequently to the head and neck. The face is hot, red, or purple, the neck swollen, whilst the pulsations of the heart and arteries are rapid. When the attack subsides, the child becomes strikingly animated, enters into his customary sports, and evinces no desire to lie down or to go to sleep Respiration after the

first attack, and even after several attacks, is quiet and natural; it becomes, however, gradually more hurried and noisy, and a faint rattling is heard, which assumes by degrees a metallic sound. Hoarseness increases, and the voice becomes low and whistling. The attacks come on more frequently, and the child is more restless and irritable during the periods of intermission, whilst the pulse grows fainter and fainter. This uneasiness, however ceases. The child dozes continually, lies on its back, with its head thrown back and pressed into the pillow; the throat protrudes, the countenance is drawn, pale, and swollen, somewhat of a bluish or yellowish tinge. The eyes are sunk, and half shut; whatever is handed to the child is impatiently pushed aside, and nothing can induce it to drink. The respiration is loud and rattling, all the muscles of the neck act convulsively, the pulse is frequent and small. The child dies either in this state of sopor, with the symptoms of paralysis, or in the midst of convulsions induced by another choking attack of cough.

This form of disease requires prompt and energetic treatment. No time should be lost in abstracting blood, and no apparent amelioration of the symptoms should hinder the frequent application of leeches, in proportion to the age, until the child begins to evince an appearance of exhaustion from loss of blood. A second important means is cupr. sulph.; from 3 to 4 grains of which should at first be given in order to induce vomiting, and the dose should be then reduced to $\frac{1}{8}$ or $\frac{1}{4}$ of a grain, every half hour, or hour, until the disease assumes a favorable turn. Dr. Zeroni also speaks of the invaluable aid he has derived in some cases of *this* form of croup, but not in any other, from a combination of musk and opium.

We now proceed to notice the third form of croup.

Inflammatory Tracheal Croup.—This is likewise attended by premonitory symptoms, and induced by pre-existing or extremely injurious influences. It generally occurs in the months of February and March. Children catch cold, have a dry, somewhat rough, cough, which being often disregarded, they are frequently suffered to expose themselves to cold and damp; the hoarseness and cough gradually increase, but this state often continues for upwards of a week before the occurrence of the fit of choking, and before medical advice is sought. After the first attack, the child is often cheerful and even at times extremely merry. The voice is quite gone, the respiration somewhat hurried, and more or less rattling; the cough not frequent, short, rough, unattended by a whistling inspiration, no expectoration, or if any, merely a white frothy mucus interspersed with a few streaks of blood; the pulse quick, the skin warm, and the urine natural. If the little patients are able to speak, they complain of pain in the neck and the middle of the chest. By degrees the choking fits become more frequent, the respiration more hurried and difficult, and the tone accompanying it rougher and more croaking. Extreme hilarity and the most remarkable movements alternate with excessive lassitude, during which the child sinks down exhausted, falls asleep, exhibiting the most marked disinclination to be spoken to or touched. The cough becomes a noiseless suppressed expulsion of air, the attacks are accompanied by a violent noisy rattling sound, the muscles of the neck become powerfully convulsed, and the head is thrown far back. The pulse is small and quick, the skin drawn, the muscles extremely relapsed, the face swelled and puffy, the lips blue. The child dies in a state of sopor, as if from asphyxia. This form is more fatal to children under two years of age, than to those who are older. It is met with in children of six, or even occasionally, nine years of age.

The application of leeches is of the greatest importance, since on this depends the result of the whole treatment. If a sufficient number of leeches be early applied to the neck and chest, we may regard the termination of the disease as probably favorable. Emetics do not appear to have much influence here, although they occasionally relieve the respiration.

The fourth form, which is designated by Dr. Zeroni as *Aphthous Croup*, is the most dangerous, but fortunately also the most uncommon; it has only been observed in autumn, during a continuance of stormy, cold, and rainy weather. It

never occurs unattended by premonitory symptoms. The child is somewhat excited, occasionally flushed, and appearing from time to time to have transient febrile symptoms. As, however, it is cheerful, sleeps well, and has a good appetite, these symptoms are too often neglected, and the child is suffered to go out in the damp or cold, until at last it complains of pain on swallowing. On examining the throat, the tonsils are found to be somewhat swollen, reddish, and covered here and there by a yellowish white puriform investment. The submaxillary glands are swollen. The child continues, however, cheerful, and there is scarcely a trace of fever. The aphthous streaks or points now extend gradually more and more, approaching each other. On removing part of this investment from the tonsils, we find that the subjacent membrane is of a brownish red colour, but not dry. Deglutition becomes more painful, but still there is no fever, and it is not till the fourth or fifth day that the symptoms assume a more serious character. Hoarseness comes on, a low singular kind of cough is heard, and occasional oppressive sensations are experienced. The disease soon runs its fatal course, and the child, after several days of indescribable suffering, dies in a state of sopor, under circumstances similar to those of which we have already spoken. A prophylactic mode of treatment seems the only one that is of avail in this form of croup; and, considering the nature of the disease, too much stress cannot be laid on those means of prevention under the control of parents—such as prompt attention to any symptoms of indisposition manifested by young children, and care not to expose them to the open air until all morbid symptoms are entirely removed; since Dr. Zeroni mentions that where once this aphthous affection of the tonsils was established, he never yet succeeded in saving the child; leeches, tartar-emetic, and calomel being all without avail. The only means which he considers at all likely to produce a favorable result, are the external application of caustics, as suggested by Aretæus. Dr. Zeroni considers that this aphthous affection of the tonsils may occur in adults, although in their case he has never observed a fatal result. The disease may manifest itself alone, or conjointly with febrile diseases; but he has not found that in this latter case the local affection rendered any change necessary in the mode of treatment for the main disease.

The fifth form of croup, observed by Dr. Zeroni, is *Suppurative Croup*. This is invariably found to have been preceded by a fully developed catarrh, and usually occurs at the close of winter, and the beginning of spring. It begins with more or less fever, restlessness, insomnia; the cough that was previously loose, becomes dry, rough, and barking, without being attended by a whistling inspiration, or a metallic sound. The cough comes on by fits, during which the child tries to sit up, bends the head forward, and puts its hands to its ears, tongue or mouth. The attacks are not attended by choking, but cause distress by the continuance of the short broken cough. The child is hoarse from the beginning of the disease, but loses his voice entirely after a time; cases, however, occur, in which the cough is at first loose, and the voice clear, but where there is much fever at the beginning of the disease, and even strongly marked delirium occurring at night. Fever gradually increases, the child sleeps almost continually, actual suffocative fits at length come on, the respiration becomes hurried, gasping, and rattling. The child is pale and appears swollen; and finally torpor supervenes, with an extremely quick pulse and profuse perspiration, and the child not unfrequently dies in convulsions.

If the disease is neglected, it generally proves fatal to infants and very young children from the ninth to the eleventh day. In adults, it may be prolonged to the fourteenth or eighteenth day; in the latter case the attacks are much more violent. The suffocative attacks which generally supervene on the seventh day are most distressing; the child starts up with violence, tears, scratches, and bites everything it can lay hold of, often tearing its hair and biting its hands; it appears to be in most fearful struggle, and in the height of its agony, the hoarseness suddenly disappears, and it cries in a loud voice for help: the short cough becomes looser, and mucus is expectorated, the fever abates, and finally the dreadful sufferings of the little patient terminate in symptoms of paralysis.

This form, like the others, demands a prompt and early application of leeches, which must be repeated with a frequency proportionate to the age of the child and the violence of the fever; it is almost the only thing to which recourse can be had, but as soon as the cough becomes somewhat less distressing, and the fever abates, a favorable termination of the disease may be hoped for; occasionally, however, much service is derived from Cupr. sulph., given in sufficient doses to produce vomiting; this must be done when the cough and fever have abated, and the suffocative attacks have begun. The above forms of croup are only met with in children, and seldom after their sixth year. Dr. Zeroni scarcely attaches any faith to the opinion entertained by many, of the fatal nature of croup in adults. He says that he certainly has observed all the symptoms of croup most strikingly manifested in women, but these were found to depend on uterine derangement, and yielded to a mode of treatment adopted with reference to diseases of the latter kind; and he considers that where adults have sunk under croupous symptoms, they must be ascribed rather to œdema glottidis than to genuine croup.—*Henle u. Pfeufer's Zeitschrift für rat. Med.* 1847.

The Use of Ice as a means of arresting Hemorrhages. By M. Chassaignac.

From a paper read by M. Chassaignac on this subject, before the Academy of Medicine at Paris, we extract the following remarks:—Mucous hemorrhage often proves fatal in cases where its cause and mechanism are alike unknown. It would be easy in these cases, by means of a pair of forceps, to carry pieces of ice to the mucous cavities, place them exactly on the bleeding surface, and thus use them both as compressive and refrigerant means.

Ice may in this manner frequently be substituted with advantage for the actual cautery. In no case has its employment been followed by gangrene, nor have the points of the pieces, which become rounded in melting, ever wounded the parts to which they were applied. They leave an inoffensive residue, which enables the practitioner to dispense with those precautions which are absolutely necessary after the employment of caustic.—*Gazette Médicale*, 1848.

On Quinine in Acute Rheumatism. By Dr. Vinet.

Quinine has of late years been very freely used in France, in acute rheumatism, and Dr. Vinet states that he has arrived at the following conclusions, from observation of its employment in the various hospitals of Paris:—1. That given in quantities gradually increased from 15 to 45 grains in the 24 hours, in divided doses, it produces no notable accidents; but if the larger quantity be not sufficiently divided and taken at considerable intervals, vomiting, faintness, or cerebral disturbances occur. 2. The disturbances of the hearing, sight, and brain are usually the first in appearing, and are far more prominent than those relating to the digestive organs. They usually cease upon mere suspension of the medicine. 3. Quinine, given in the above doses, exerts a remarkable, and frequently a rapid, sedative power on the circulation and the pain, and an indubitable effect upon the general phenomena of the disease. In about half the cases submitted to it, these effects are prompt and durable, and in the other half slow, uncertain, and unstable; while in a few cases, no effect whatever follows. 4. The cases in which it proves most efficacious, are those in which the general and local symptoms are best marked. 5. In cases in which it procures a prompt cure, it may prevent the cardiac complication, and in those in which it is slower in its operation, it does not seem to favour the occurrence of this, which, however, when it does occur, requires the usual appropriate means for its removal. 6. The beneficial effects are generally the more promptly produced as the dose is large, such being often observed after a moderate amount of perturbation of the nervous system. 7. Given for a relapse of the disease, the effects are the same as when originally employed.—*L'Union Médicale*, No. 43.

M. Aoudouard, an old practitioner, in a paper in the 'Revue Médicale,' shows that bark has been recommended by several of the best writers on gout and acute rheumatism, and refers to a paper approving of it, which he himself published in

1808; but, he adds, the cures effected by him and his predecessors were accomplished by, at most, 2 oz. of bark daily, while now an equivalent of quinine, equal to 5 or 6 oz., is given. But then the bark cured without producing any ill effect on the brain, or any other bad consequences, which cannot be said of the quinine.—*Revue Médicale*, 1848, No. 1.

In No. 28 of 'L'Union Médicale,' M. Fauconneau-Dufresne adds two other instances to the now numerous ones on record, of death occurring from excessive doses of quinine. Some practitioners have given even a drachm and a half per diem; but death resulted in one of these cases from a scruple dose.

Metallic Quicksilver in Ileus and Obstructed Bowels.

Several cases of the utility of quicksilver in ileus have been recently published in the German journals. Dr. Schubert relates one of a man to whom every internal and external medicine had been given, until the incessant vomiting obliged the abandonment of all of the former. The constipation was most obstinate, and the abdomen much distended, but there were no signs of inflammation. The patient seemed at the last extremity, when Dr. Schubert remembering two similar cases he had seen so treated with success, ordered him 4 oz. of quicksilver every half hour. His death seeming inevitable, only two doses were given; but after two hours he had stools, and soon recovered.

Dr. Löwenhardt refers to a work published by him in 1838, in which he sets forth the advantages derivable from this substance, and in the paper before us details additional cases in illustration. The cases now added are those of volvulus, internal incarceration, spastic ileus; inflammatory ileus, after the inflammatory symptoms are removed; incarceration persisting after the operation for hernia, probably from the agglutination of the parietes of the canal by exuded mucus; and especially *very obstinate vomiting*. All the cases he adduces were not cures, and he gives the post-mortem examinations of some, from which it appears that the mercury sometimes passes through an intussusception, without removing it.—*Casper's Wochenschrift*, No. 9, *Medicinische Zeitung*, 12 and 13.

Cause of the Fatality of Inflammation of the Upper Lobe of the Right Lung. By M. Hervez de Chegoin.

M. Hervez de Chegoin has for some time had his attention drawn to the peculiarity of the symptoms, and the especial danger, of pneumonia attacking this part of the lung. Such patients speedily exhibit all the signs of exhaustion, a very small pulse, a death-like pallor of countenance, peculiar disturbance of the intellectual faculties, nausea, and diarrhœa. Some complain, too, of intense pain in the clavicular region; the expectoration and cough are very slight, and crepitation is speedily exchanged for a dull *souffle*. The general and local signs of the inflammation are in fact very slightly displayed, although it almost always proves fatal. An autopsy of such a patient, which recently occurred to him, he thinks throws some light upon the matter. Examining the organs exactly *in situ*, he found, although the patient had lived 13 days, the upper lobe was only in the first stage of hepatization, and that this now firm body exerted great compression upon the *vena cava superior*. This, he conceives, may explain the slow progress of the disease, the small pulse, the peculiar cerebral disturbance, &c. And if this view be correct, instead of being deterred from bleeding these patients by their apparently exhausted state, our measures should be only the more vigorous; as the only chance of relief is the removal of the engorgement of the lung, which, by compression of the vein, impedes the circulation.—*L'Union Médicale*, No. 63.

Retention of Urine in Cerebral Affections.

M. Von Den Broeck states that in these cases he has almost always found the application of large cupping-glasses to the upper and inner parts of the thighs give rise to a speedy voiding of the urine.—*Rev. Méd.-Chir.*, vol. iii, p. 38.

SURGERY.

On Gun-shot Wounds.

Although the French Revolution of February 1848 was far less bloody in its course and fatal in its consequences than that of July 1830, the number of wounded was still considerable; the total admitted up to the 21st of May into the various hospitals amounting to 643. Many excellent clinical observations have been made by the various hospital-surgeons, some of which may be advantageously reproduced.

M. Velpeau on Gangrene.—Of this, *two species* exist, *direct* and *indirect.* The first is observed as the direct result of the injury, and may occupy only a very thin layer of the wound, or extend to the bone itself: this last tissue, when affected, being usually so to a greater extent than the soft parts. The second form, which is that which is usually described by authors, is developed below the surface, and under the influence of the wound. An intermediate form is observed when the gangrene of a wound extends to its vicinity. Gangrene usually depends upon three *causes:* wounds of blood-vessels, that is when the large veins, as well as the arteries, are implicated; wounds of numerous nerves; or great crushing of the soft parts. The *prognosis* of gangrene following wounds from *fire-arms,* is more serious than that from *ordinary wounds,* but less so than in the case of *spontaneous* gangrene, which depends upon some general cause and organic lesion of the blood-vessels. As regards the gangrened part, it is lost; and the question is, whether we should perform *amputation,* or allow it to fall off. Formerly, a fear of hemorrhage led to the latter practice; but now, possessed of hæmostatic resources, and aware that a projecting bone and deformed stump remain, we amputate. If, however, the general health is much deteriorated, or severe and multiple lesions exist, by amputation we only add to the danger. Otherwise, although in spontaneous gangrene it is preferable to await the line of demarcation, in that arising from injury, we hasten to operate, and that for one of the reasons which have been advanced in favour of abstaining—the disposition of gangrene to propagate itself—the best preventive of which is the prompt removal of the gangrened part. Moreover, purulent infection is more likely to take place in gangrene than in a simple wound; and the reaction by which the surrounding parts seek to oppose the extension of the gangrene only adds to the exhaustion of the patient. Gangrene too makes rapid havoc in the economy of the patient.—(*Gaz. des Hôpitaux,* 1848, Nos. 28 and 29.)

M. Malgaigne on the Diet of the Wounded.—All those who have observed French after-treatment must feel convinced that it often errs in a prolonged starvation diet. M. Malgaigne has in this respect departed from the usual practice of his colleagues; and he was induced to do so by what he observed in Paris in 1814. Then, although the French wounded commanded the highest skill of the capital, and were placed under more favorable circumstances than the Russians, yet these latter recovered in far larger proportions. This seemed to be due to the large quantities of food, wine, and brandy they were allowed to take. It is true, these last killed several, but still the advantages of a good regimen was no less obvious in others. (*Gaz. des Hôp.* No. 27.)

Application of Cold to Wounds.—Upon the subject of the application of cold, much difference of opinion prevails among the best surgeons. M. Jobert advocates the continued application of pounded ice in bladders in the cure of *burns* and traumatic injuries; believing it of great importance to diminish suppuration as far as possible, thus husbanding the strength of the patient, and preventing the development of deep and deforming cicatrices. It is, however, in the wards of M. Baudens at *Val de Grace,* that the application of ice in traumatic injuries is carried to the greatest extent, as we noticed in a former Number. M. Velpeau, on the other hand, is little favorable to this practice; and remarks that after observing it in others, and trying it himself, he has come to the conclusion that it is attended with more evil than benefit. Although in wounds whose union by the first intention is sought, the

cold water may not always do harm, yet when the flaps of the wound are large, thin, or unconnected, it may easily induce their mortification—the thing here really to be feared being the want of that activity of circulation, which cold water diminishes. In contused wounds, certainly, cold water may diminish inflammation, but this is required up to a certain point for the elimination of mortified parts; the old maxim, that we must have inflammation, but not too much of it, being the difficulty. It is not even demonstrated that cold prevents or diminishes suppuration, though the pus may have a more diluted appearance; but it is well known that these applications have induced serious attacks of inflammation of the air-passages and rheumatism. In M. Velpeau's cases the amount of inflammation around the wound has never been a cause of uneasiness, nor has the pain been violent. (*Gaz. des Hôp.* Nos. 47 and 50.)

Removal of Foreign Bodies.—M. Jobert observed that owing to the persons being so near when shot in the Paris tumults, almost all the balls traversed the parts, so that the question of their extraction had not to be entered upon, and was limited to that of the wadding, portions of clothing forced into the wound, and fragments of broken bone. When any of these are near the surface, they should be at once removed; but this is rarely the case, as the balls usually take a circuitous course, or bury these bodies deeply in the soft parts. Any attempt at their removal increases the inflammation and does harm, and M. Jobert even forbids the examination of the course of the wounds by a probe, fearing the disturbance of a salutary coagulum or commencing adhesion. Smooth bodies, such as balls, when left are surrounded by cysts; and the others, by exciting suppuration, become afterwards much easier of removal. M. Velpeau limits interference for the removal of fragments of bone, &c. to cases wherein these are quite free, for otherwise subsequent suppuration will best loosen them. He objects to making any incision in quest of balls; for this course is very uncertain, and their presence is not attended with much danger, as they soon become isolated from other parts by a small cyst. M. Baudens, accustomed to practise in Algeria, whose climate is so favorable to the healing of wounds, recommends rather more active procedures than the above for the removal of fragments of fractured bone, sanctioning incisions for this purpose. But he raises a strong remonstrance against a very bad practice, which he says he found universally prevalent in the French army when he entered it, but which he has done much to abolish, viz., the making precautionary incisions (sometimes very long ones) or *debridements*, under the fallacious idea that by converting the gun-shot wound into a clean longitudinal one its more ready healing is obtained, fistulous openings and strangulations being prevented. He shows conclusively the fallacy of these views, and the ill effects of such meddlesome surgery, and only admits the removal of foreign bodies, or the taking up of vessels, as justifications of incision.—*Gaz. des Hôp.* Nos. 31, 33, 50.

Elevated Position of Wounded Parts.—Some persons, M. Velpeau observed, always place an injured limb in an elevated position; a practice that, however good at the commencement of an inflammation, can only tend to its propagation after the formation of pus has taken place; for in these cases the inflammation around the edges of the wound is not that which is to be feared, governable as it is by topical applications, but infiltrations and purulent sinuses; and it is far better to allow an inflammation to progress at the extremity of a wound, than to favour its advance in the other direction. In those cases where, from the nature of the wounds, as some of those on the knee, it becomes impossible to place the part in a dependent posture, great danger is present. The raised position, too, as applied to the lower limbs, has another inconvenience; for as these constitute a fifth of the total mass of the body, a derangement of the circulation, amounting almost to a stasis, takes place, and may give rise to various important lesions of the viscera, as has been the case in some persons so treated for ulcers of the legs.—*Gaz. des Hôp.* No. 39.

Blandin on Pain and Hemorrhage.—A shock rather than actual pain is felt at the time of the receipt of the injury; so that the flow of blood has often alone indicated what has occurred. In sword-wounds, the pain is ordinarily not very

severe, yet much more so than in those from fire-arms. A numbness of the parts is produced, all sensibility being temporarily lost; and this peculiar feeling is called *stupeur*. It may be only *local*, resulting from the action of a ball on the part itself; or it may be *general*, affecting the entire economy. In ordinary cases, when a part of the body is struck by a bullet, there is only a local effect produced, but when a ball carries away a part, a general shock or *stupeur* is produced. In these cases the pulse is small—a nervous pulse. Great engorgement of the wound is present, by reason of the non-resistance of the tissues, and this eventually gives rise to a more intense inflammation than after ordinary wounds. It is a common opinion that no *hemorrhage* takes place after gun-shot wounds, but it is a great error; for although these wounds may not be so easily followed by hemorrhage as others, yet numbers perish in battle from fatal bleeding alone. These hemorrhages are, however, certainly more easily arrested spontaneously than are others, they having in this respect much resemblance to lacerated wounds. In the February cases hemorrhage was readily arrested, although often proceeding from large vessels.—*Gaz. des Hôp.* No. 51.

Diagnosis. An observation made under this head by M. Blandin may be usefully borne in mind, as relating to medical jurisprudence. It is, that, while after two or three days, gun-shot wounds may present no appearance distinguishing them from any others, on the other hand, wounds resulting from no projectile body sometimes put on exactly the same appearances as gun-shot wounds do at first. M. Blandin referred to the case of a young man just admitted, in whose knee-joint there was so circular a wound, that, had he declared it to result from a ball, he must have been believed.—*Gaz. des Hôp.*, No. 51.

Immediate amputation. There are, after gun-shot wounds, three periods:—the first, that of *stupeur*, resulting from the shock endured by the nervous system, previously perhaps in a state of great excitement. It continues generally from twenty-four to thirty-six hours, and is followed by the second or stage of *inflammatory reaction;* this to be succeeded by that of *suppuration*. Immediate amputation should be performed during the period of *stupeur*, and not await that of inflammation, which would attack the stump. The case of *fracture of the thigh* is the most embarrassing, as there may seem to be an insufficient amount of injury to warrant amputation, while experience has too frequently shown the fatal consequences of attempting to save the limb. This fracture, from ordinary causes, complicated by a wound near the joint, often necessitates an operation; and it is absolutely essential if the wound has been produced by fire-arms. The consequences of secondary amputation are less serious than those of primary, because the patient has already passed through the dangers of inflammatory reaction; but the fear of finding an operation, at first possible, become impracticable by reason of local or general disorders, renders it necessary to have recourse in several cases to immediate amputation. Sword wounds hardly ever give rise to this necessity; but when a limb is struck by a large projectile, the injury is generally too great to allow of delay. When operating in the stage of stupor, we should not employ the chloroform.—*Velpeau, in L'Union Médicale*, 1848, No. 28.

M. Baudens advises us, if the upper extremity is fractured, to remove the fragments, and only employ consecutive amputation as a last resource. If the femur is comminutively fractured, to amputate immediately; if the tibia or fibula is separately fractured, to try to save the limb; while, if both bones are so, as a general rule, to amputate.—*Gaz. des Hôp.*, No. 42.

On the Employment of the Tartrate of Iron and Potassium for the Treatment of primary Phagedænic Syphilitic Sores. By M. RICORD.

PHAGEDÆNA is not a separate species of syphilitic sore, but may show itself as a consequence of some local or general cause of irritation. Thus œdema, inflammation, strangulation, and bad dressing, are local causes; while among those of a general nature, are debility, privations, excess, unhealthy habitations, scrofulous or

lymphatic temperament, long-continued mercurialization, &c. &c. It is usually, however, in the changed aspect of the sore that these conditions chiefly manifest their influence, and which gave rise to the use of the remedy in question. Some of the patients, however, suffer in their general health from the same causes which have induced the unhealthy aspect of the sore. Some of them are much enfeebled, suffer from headaches, palpitations, and gastralgia, their skin and mucous membranes being anormally pale; others have various kinds of eczematous eruptions form on the skin, and near the phagedænic ulcer; and if the suppuration from the sore is abundant, the patients become still more enfeebled.

The administration of iron is not new, but, before M. Ricord, practitioners gave it in only very small doses, such as less than a drachm per diem, whereas he commences with three drachms at once, and carries it on even till six. In three days, it will often already produce a modification in the appearance of the sore. The pultaceous matter which covers the bottom is first more easily detached, the discharge is less grumous, and does not coagulate so easily on the surface. The pale and transparent granulations become red, or rose-coloured; and the suppuration, which had been serous, and loaded with the detritus of the pultaceous substance, becomes homogeneous and thicker. The edges of the ulcer take on an ash colour, become rounded, and cease to project. The ulcer, in fact, becomes converted into a simple one, and, as such, may soon heal. After trying various local applications, M. Ricord dressed the sore with a solution of the tartrate of iron and potassium, which, to those not accustomed to it, gives the sore at first a very discouraging and dirty appearance; but local applications of any kind are only of very secondary importance. When the internal use of the medicine has been too soon suspended, the sore has soon returned to its foul state. The time it may be required is indefinite, M. Ricord continuing it, as a general rule, until cicatrization is complete; for if a portion is left unhealed no bigger than a pea, the sore will again rapidly extend, and involving the cicatrix, as it then sometimes does, it becomes cured with only additional difficulty. The shortest duration of such sores, counting from when the medicine is commenced, has been seventeen days, while in others between three and four months may be required, especially if complications requiring the suspension of the medicine intervene. Rarely have the pathogenetic effects of the medicine called for such suspension, and even the most enfeebled patients can take it without repugnance. At the end of three or four days, and sometimes much later, the skin and mucous membranes acquire a deeper colour, the capillary system acquires an increased development, the pulse becomes fuller and quicker; headache, and, if the dose be large, uneasiness of the stomach, or colic, are complained of. The appetite generally increases, and the fecal matters become very hard and black, sometimes loose. Frequently no other effect is observed than the amelioration of the phagedæna. The medicine is given in water in divided doses; so as to commence with from one to three drachms per diem, and in about ten days six drachms are reached. As adjuvants, M. Ricord gives bitter and antiscorbutic ptisans.—*Gazette des Hôpitaux,* 1847, No. 118.

Foreign Bodies in the Air-Passages. By Dr. Mason Warren.

Dr. Mason Warren relates three interesting cases, with the hope of contributing somewhat to the more accurate definition of the line of practice that should be followed in these emergencies. In the *first*, a garden bean entered the trachea while a girl, æt. 8, was laughing, and the eventual symptoms gave every reason to believe that it had reached the left bronchus. As the irritation produced was only occasional, an operation was delayed as long as possible, in hopes that the body would be spontaneously discharged; but two days after, symptoms of suffocation manifesting themselves, it was resorted to. It was rendered difficult by the tumefaction of the neck produced by venous congestion and the struggles of the child; and before the trachea was opened, a crack was heard as if a portion of the lung had given way, and a small tumour was forced up in front of the left side of

the trachea during each inspiration—the dyspnœa being henceforth mitigated. The bean, much swollen, was removed with some difficulty when it rose into the trachea. The child did well. The *second* case occurred in the person of a boy between 2 and 3 years old, who had swallowed a horse-shoe nail. For some days after, he suffered severe occasional suffocative paroxysms, apparently produced by the nail mounting up from the bronchus to the trachea. These became so imminent, and the child's strength was so rapidly failing, that it was resolved to operate. Shortly prior to the period determined upon for this, a violent suffocative cough, accompanied with much stringy mucus, occurred, after which all symptoms of the presence of a foreign body disappeared. It was believed, from subsequent symptoms, that this had entered the fauces enveloped in the mucus, and had then been swallowed. The *third* case was that of a young woman who had swallowed a pin. The patient's sensations indicated the left side of the larynx as the site of the foreign body. Attempts were made to dislodge it by the finger, and she was afterwards bled, to subdue the irritation caused by these and the constant cough. While awaiting a renewal of the attempts, the pin became spontaneously dislodged, entered the throat, and was swallowed.

These and similar cases on record lead to the following conclusions: 1, We should not leave any soft body liable to increase of bulk from heat and moisture to the chance of spontaneous expulsion, especially in children, in whom the larynx is comparatively small. 2. Metallic or other hard substances, when engaged in the larynx, or lodged in the lungs, may be left with more impunity. 3. If the substance is fixed in the bronchus, and the patient young, the prospect of seizing it by instruments carried through the wound is very small. Mr. Liston thus removed with difficulty a bone from the right bronchus of an adult. 4. Some doubts may, however, arise as to the propriety of leaving a foreign body in the lungs, without an effort to remove it; for although it may become detached, as it frequently does, after many months or years, yet fatal organic lesions are not unfrequently the result. The judgment of the surgeon must be determined therefore by the circumstances of the particular case.—*Boston Medical and Surgical Journal*, vol. xxxvii, pp. 389-96.

An easy Means of rendering the Ulnar Artery accessible to the Finger or even to the Eye. By M. MALGAIGNE.

THE means suggested by M. Malgaigne, the efficacy of which the reader may at once test upon his own person, will be found of great utility, when circumstances prevent our feeling the pulse at the radial artery, or when it is desired to take up the ulnar. He thus describes it.

"I have several times had occasion to place a ligature around the ulnar artery for lesions of this vessel; and when the cellular tissue is gorged with effused blood, and it is necessary to seek it at a considerable depth, I do not hesitate to term it a very difficult operation. Perhaps the means I am about to state will assist in removing the principal difficulty, which especially depends upon the depth of the artery. If the fingers and hand are turned forcibly backwards upon the dorsal aspect of the forearm, the relations of the ulnar artery become surprisingly changed. The deep-seated muscles upon which it lies are forcibly raised, and cause a sensible projection under the skin. The tendon of the flexor carpi ulnaris, on the contrary, retreats inwards and backwards; so that the artery, which, in the natural position of parts, is partly concealed by it, is now forced to a much more anterior plane, and lies four or five millimetres on the inner edge of the tendon. In many subjects it becomes more superficial than the radial, and it may be seen raising the skin at each pulsation.

"In traumatic lesions of the vessel, the same position brings the wounded extremity of the vessel towards the surface, and enables us to seize it; and in any case, when we wish to pass a ligature around the vessel, there will be no longer occasion to denude and draw away the tendon of the flexor."—*Revue Médico-Chirurgicale*, tom. ii, p. 160.

Rapid Recovery after Fracture of the Tibia.

DR. SCHWEICH relates the case of a peasant, æt. 40, whose tibia was transversely fractured at about its middle, producing obvious displacement. The case was treated by means of the starch bandage, and the reporter left off attending him on the sixth day. He was much surprised to hear afterwards that he had tried the experiment of walking in his room on the 12th day, and had gone to his employment on the 14th. On the 25th day he called upon the author, and exhibited a well-formed callus.—*Casper's Wochenschrift*, 1848, No. 7.

Cauterization of Irreducible Omentum. By M. BONNET.

M. BONNET, of Lyon, dissatisfied with the practices hitherto recommended by writers on hernia, conceived the idea of excising the projecting mass of the omentum, having first applied a ligature to its pedicle, and then cauterizing the wound with the chloride of zinc. Three cases have occurred to him, in which the adherent omentum was thus successfully treated, in two of which gangrene had already commenced before the caustic was applied. Equal parts of flour and the chloride of zinc were used, and applied daily, the portion cauterized the day before being first removed.—*Gaz. des Hôp.*, No. 67.

MIDWIFERY, &c.

A Statistical Inquiry into the Causes, Symptoms, Pathology, and Treatment of Rupture of the Uterus. By JAMES D. TRASK, A.M., M.D.

DR. TRASK, struck with the discrepancy of opinion prevailing among the most esteemed writers upon midwifery, in respect to the course of procedure to be adopted on the occurrence of rupture of the uterus, resolved to investigate the subject for himself, by examining into the results exhibited in published cases. To this end he has collected the leading particulars, if not of all, at least of by far the greater number of cases on record, and by an elaborate analysis of these, has endeavoured to educe rules of practice more satisfactory than those hitherto laid down. Only those who have been engaged in somewhat analogous investigations, can appreciate the amount of labour they entail, to say nothing of the constant disappointments resulting from the omission of essential particulars by the relators of cases. The art of narrating these at once succinctly and completely seems to be one of difficult attainment, if we may judge by the rarity of the accomplishment.

Of the 303 cases collected by Dr. Trask, only 38 are reported as happening *during pregnancy;* many cases of sudden death during this period doubtless occurring, without this cause having been, owing to the obscurity of the diagnosis prior to parturition, either discovered or suspected. Dr. Trask examines the question of the *causes* at considerable length. Among the predisposing, *contraction of the pelvis* holds a foremost place with most observers, especially when the rupture occurs at the cervix. A *diseased or softened condition* of the part of the uterus where the rupture occurred, is noted sufficiently often to confirm Dr. Murphy's opinion (Dub. Journ., vol. vii) of its frequency in these cases. Disproportionate *size of the fœtus, oblique positions of the head, unrectified transverse presentations, rigidity of the cervix, obliquity of the uterus,* &c., are other predisponents. The *immediate* causes are violent natural or excited *uterine action, external violence,* and *forcible attempts at delivery*. It is an error to suppose that when rupture takes place from spontaneous uterine action, it does so only in *violent or protracted labour*. A table is given, showing that it occurred at very variable periods of its duration, from one to sixty hours. Dr. Trask adduces several cases, in which the injudicious use of *ergot* seemed to have been the immediate cause, and strongly urges upon practitioners the necessity of abstaining from this drug, when there is

any disproportion between the head and the pelvis, when faulty presentation or other obstacle to delivery exists, and when the os uteri is not dilated completely, or perfectly dilatable. Three cases are quoted as proving that the *movements of the fœtus* may induce rupture of an organ probably already diseased; and three others, in illustration of the curious fact that the accident may follow intense *mental emotion.*

Passing over the author's observations upon the pathology, symptoms, diagnosis, and prognosis of the accident, which contain nothing very remarkable, we come to those upon the *treatment*. Most accoucheurs formerly, and several even now, recommend the leaving those cases to nature. Unfortunately, many fatal cases, however treated, have never been published; and where the assistance of art has been resorted to, it has often been so but at the last extremity. The author carefully guards against the supposition, that the results he has collected indicate the exact proportion of deaths or recoveries after the various modes of treatment; considering it of great importance if he can even point out that course which seems generally preferable. Of 154 cases delivered artificially, 97 died, and 57 survived; of 89 abandoned undelivered, 65 died, and 24 survived; of 6, in whom artificial delivery was tried and failed, all died. As far as these figures go, then, they show that 37 per cent. of those delivered by art, and 27 per cent. of those left undelivered, are saved. Even where life has not been saved, its prolongation after delivery has averaged twenty-two hours, as compared to nine hours in the undelivered woman. Dr. Trask, after showing the superiority of gastrotomy to any other mode of delivery, when the child has passed entirely into the abdomen, passes in review the various circumstances which should modify our procedures.

1. *Rupture occurring in women having a well-formed pelvis.* If the head is still in the pelvis, and the child alive, the forceps should be used; but if the child is dead, perforation should be resorted to. 2. *Escape of the child into the abdomen, the pelvis being ample.* Version is preferable to gastrotomy only on one condition, viz. that the edges of the wound through which the child has passed have not contracted. Unless it is merely the upper part of the vagina that is perforated, they will be found almost invariably to be so contracted, as to render their dilatation exceedingly painful and dangerous from the risk of hemorrhage. 3. *Impaction of the head in the pelvis* calls for perforation. 4. *In contraction of the brim of the pelvis,* gastrotomy is indicated, as exposing the woman to far less suffering and risk than perforation; and a general rule may be laid down, that, when we are led to expect, from whatever cause, *a protracted and difficult delivery by the natural passages, gastrotomy will afford the best chance of recovery.* Nearly one half the instances in which both mother and child were saved, were those in which gastrotomy was performed, notwithstanding that the number of cases in which this operation was resorted to is very small, compared to those of natural and other means of artificial delivery. 5. Another important rule is, that *delivery should be effected as speedily as possible after the rupture has occurred,* the non-success of interference being doubtless often due to its too long postponement. 6. When the mother has ceased to live, *gastrotomy* still offers the best chance for *saving the child.—American Journ. Med. Sciences,* vol. xv, pp. 104-46, and 383-415.

[Our analysis of a paper, itself so analytical, is necessarily brief, but it will suffice to direct attention to so important a contribution to obstetrical science.]

Colour of the Vagina in Pregnancy. By Dr. Albert.

Dr. Albert states that of all the signs of pregnancy hitherto known, that derivable from the observation of the dark red colour of the vagina is the best, seen as it is by the aid of a speculum at so early a period of pregnancy, and proceeding progressively with the development of the uterus. He has tested its utility in about 30 cases, and has besides examined, with the same success, a great number of animals at various periods of gestation.—*Zeitschrift für Geburtskunde,* vol. xxiii, p. 449.

Voluminous Enterocele through the Fundus Uteri. By M. LE CHAPTOIS.

THIS case occurred in a woman 60 years of age, the mother of seven children, she having long suffered from a vaginal hernia, for which she had not sought advice. Her disease and consequent suffering constantly increasing, M. Le Chaptois was sent for, and found that an enormous mass of intestines had descended externally through an aperture in the uterus. The uterus, entirely inverted, hung down upon the thighs, pyriform in shape, brown in colour, and larger than the fist. The walls of its fundus were thin and parchment-like, as if deprived of all circulation. An aperture near the right tubular angle had given passage to the intestine. The parts, by means of methodical endeavours, were returned within the pelvic cavity, where they were retained by a sponge and suitable apparatus. The patient had a small pulse, cold sweats, frequent syncope, and hiccough, prior to the reduction, and repeated vomiting and purging, with frightful convulsions after it. She, however, rallied, and convalescence proceeded satisfactorily until the third month, when she was carried off by a peripneumonia. No autopsy was allowed.—*Gazette Médicale*, 1847, No. 51.

On Examination of the Uterus by the Rectum. By M. CHOMEL.

M. CHOMEL urges that the exploration of the uterus by the rectum in cases of its malversation, is by far too much neglected; although information so derived, both as respects the position and diseases of the organ, is oftentimes quite conclusive. In exploring in this way, however, it must be remembered that on the posterior surface of the uterus in the retroverted state, a kind of furrow, separating the cervix from the body of the organ, is perceived, and that, in almost all women, pressure here excites intense pain, which many surgeons have mistaken for an indication of inflammatory action, of which it is no sign whatever.—*Gaz. des Hôp.*, No. 28.

On the Erysipelas of New-born Infants. By M. TROUSSEAU.

THIS disease, whatever class of society the patient belong to, whether in hospital or private practice, is almost always fatal; so that, in nine years, M. Trousseau has only seen three recoveries. A slight swelling and redness are first pointed out by the mother upon the vulva or penis, and, as the child seems well enough, the practitioner may disregard them. But the inflammation gradually extends to the abdomen and thighs, and the child becomes sleepless and remarkably pallid, sucking greedily on account of thirst, but suffering from no purging or vomiting. About the fourth or fifth day the erysipelas commonly becomes complicated with peritonitis, which kills the child within twenty-four hours; so that it is rare for it to live beyond the sixth or seventh day. There is also not unfrequently umbilical phlebitis present. When puerperal fever prevails, infantile erysipelas is not uncommon, it then proceeding from ulceration which ensues upon the detachment of the funis.—*Gazette des Hôpitaux*, 1848, No. 1.

Epidemic Abortion. By Dr. SCHWEICH.

DR. SCHWEICH observes that a great number of abortions occurred at one time in his practice very near to each other, the women not having been heretofore liable to the accident, nor yet exposed to any exciting cause of its production. He is disposed to refer them to atmospheric causes, such as are capable of inducing epidemics. The winter had been warm and wet, and the spring dry, thus offering a confirmation of the aphorism of Hippocrates, "*At si hiems austrina, et valde pluvia, et placida fuerit, ver autem plus justo siccum et aquilonium, mulieres quidem, quibus partus ad ver imminet, ex quâvis causâ abortiunt.*" Witte, too, states that, under the same circumstances, numerous abortions occurred at Jena in 1686.—*Casper's Wochenschrift*, No. 7.

On some of the Affections of the Os Uteri. By M. Chomel.

M. Chomel observes that the word *ulceration* is often most unadvisedly used in relation to this part; for in perhaps a thousand cases of granulation of the cervix uteri which he may have seen during the last 20 years, he has met with only two or three examples of *non-venereal ulcer*. In some women, the mucous membrane of the os uteri is raised by a number of *white spots*, dispersed over the cervix, presenting a slight relief, and persisting sometimes for an indefinite time. Their contents appear like tuberculous or caseous matter, but M. Guenau de Mussy opened one recently with the point of a bistoury, and fluid pus flowed out; and probably, when they have existed a long time, this fluid concretes. At other times, we find at the cervix uteri *small red tumours*, at most as large as a pear or quince pip, some being sessile, and others having a delicate or even a filiform pedicle, which may become so elongated, as to allow the tumour to project beyond the vulva. They are met with both in young and old, are of no kind of consequence, producing only a little sanguineous discharge, and may be snipped off with scissors, applying the *nitr. arg.* to the point of implantation.—*Gaz. des Hôp.*, No. 19.

Prognosis furnished by the Tears of Children. By M. Trousseau.

M. Trousseau states that it may be laid down as an aphorism, as seldom liable to exceptions as those of Hippocrates, that when a child sheds tears a favorable prognosis may be delivered, however menacing the symptoms; while, when this is not the case, in painful diseases, and especially if the eyes are dry and sunken in the orbits, great danger to life exists. The observation applies almost invariably to children less than two years of age, and particularly to those less than one, but may frequently be verified even until seven. In no one of his operations for croup has he ever seen a child shed tears; and he has always felt much pleased if they did so some days after, as, when they did not, the unfavorable prognosis given was almost always verified.—*Gazette des Hôp.*, 1848, No. 14.

MATERIA MEDICA AND PHARMACY.

On the Action, Uses, and Preparation of Indian Hemp.

Numerous observers have described Indian hemp as producing in the nations of the East, who familiarly use it instead of intoxicating spirits, sometimes a heavy lazy state of agreeable reverie, from which the individual may be easily roused to discharge any simple duty—sometimes a cheerful, active state of inebriation, causing him to dance, sing, and laugh, provoking the venereal appetite, and increasing the desire for food—and sometimes a quarrelsome drunkenness, leading to acts of violence. During this condition, pain is assuaged and spasm arrested. Sleep usually supervenes in three hours; and when this passes off, no nausea, loss of appetite, or constipation follows; no other symptom but slight vertigo. The frequent use of it, however, brutalises the intellect. On trying Mr. Robertson's extract once for toothache, Dr. Christison found that about four grains taken at 3 a.m. caused in an hour cessation of pain, a pleasant numbness in the limbs, giddiness, a rapid succession of unassociated ideas, and impossibility to follow a train of thought, frequent intervals of sleep, and slight increase in the force of the pulse. Next morning there was an ordinary appetite, much torpidity, great defect and shortness of memory, extreme apparent protraction of time, but no peculiarity of articulation, or other effect; and these symptoms lasted till two p.m., when they ceased entirely in a few minutes after taking lemonade. In some instances, a condition like catalepsy has been produced, in others, a state resembling delirium tremens. In his own professional experience it has produced sleep, relieved pain, and arrested spasm; and he has never observed any disagreeable effect during or after its action,

except that in one instance it excited, as its operation commenced, an alarming sense of occasional succussions in the brain. Dr. O'Shaughnessy found that carnivorous animals were easily intoxicated by it, but that graminivorous animals were affected with greater difficulty. No inquiry has yet been made on the effects of large doses. The hemp grown in Europe is thought to be inert, and, as its leaves are not varnished with resin, it is probably feeble compared with the Indian plant; but the only careful experiments with it which have been detailed, those of Parent-Duchatelet, are not at all conclusive.

"Indian hemp (he adds) has been used as an antispasmodic in hydrophobia, tetanus, malignant cholera, and infantile convulsions, with marked relief in repeated instances. Some cases of tetanus appear to have been cured in the East Indies by it; favorable cases have been reported also in this country; and my colleague, Professor Miller, had a case under his care, which proved successful without any other important remedy. It has also been employed with success as an anodyne in chronic rheumatism, toothache, and other varieties of neuralgia; I have used it a good deal, and with decided success, in diseases at large, to obtain sleep. On the whole, it is a remedy which deserves a more extensive inquiry than any hitherto instituted.

"The dose of the alcoholic extract is from 2 to 5 grains, as a hypnotic, anodyne, and antispasmodic. On account of its insolubility, it should be given either in the form of emulsion, or in that of tincture dropped into water immediately before administration. An emulsion may be made by triturating 5 grains in a warm mortar, with 15 minims of olive oil, and then adding gradually a fluid drachm of mucilage, and 2 fluid ounces of water (Bromfield). The tincture of the extract, dispensed by the Edinburgh druggists, is given in a dose varying from 15 to 40 drops, with a little water, sometimes sweetened with sugar. The pure resin of Messrs. Smith appeared to them to be very active in the dose of two thirds of a grain, though made from old gunjah."

M. Gastinel, of Cairo, has communicated the result of some researches which he has made on this substance, with the object of combining the greatest quantity of the active principle in the smallest volume. His residence at Cairo, where the plant of the hachisch is collected, enabled him to procure a large quantity of the plants at the time of their maturity, and to make numerous experiments, the result of which has been to obtain an alcoholic extract, which answered well when given in a dose varying from 5 to 6 grains. M. Gastinel pursued his investigations, and, after many experiments, succeeded in isolating the alcaloid, which he hoped would be found to contain all the active principle. But by a remarkable anomaly, it appears that it is not the vegetable alkali which possesses the well-known properties of the hachisch, but a resinous principle contained in the plant at the period of its maturity, and this also M. Gastinel has succeeded in isolating. By experiments made on himself, he found that its action was manifested in a dose of 2 grains.

At a subsequent meeting of the Academy of Medicine at Paris (May 1848), M. Edmond de Courtine presented an extract of a memoir on the hachisch, of which the following are the main results:

1. The active principle of the *Cannabis indica* brought from Algeria, is a resin 0·05 of which produces the same effect as 2 grammes of pure extract of hachisch, or rather 15 or 20 grammes *dawamesc*, an oriental electuary considered to be pure, and containing Indian hemp, condiments, and aromatic substances.

2. *C. indica*, grown in France, yields a less active resin than the above, and in smaller quantities.

3. *C. sativa*, grown in France, yields an analogous but much less active resin.

4. *C. sativa*, grown in France from seeds that have been gathered in Italy, yields a more active resin than the above.

5. The active principle of *Cannabis* is seated principally in the leaves of the plant.

6. *Cannabis indica* and *Cannabis sativa* do not exhibit sufficiently well-marked botanical characters to constitute two distinct species.

7. The resin of *Cannabis indica*, or *Cannabine*, may be considered a valuable addition to our therapeutics.

The physical and chemical properties of the resin obtained by the author from *C. indica*, sent from Algeria, are as follows. It is of a dark brownish-green, of an aromatic and nauseating odour, of a peppery and acrid flavour, soluble in the cold in strong alcohol, in ether, fixed and volatile oils, fatty bodies; insoluble in water and weak alcohol; although tolerably homogeneous, it presents when spread out in a porcelain capsule, and when still warm, small agglomerations ending in a point, which appear to announce the presence of a fatty body. From the numerous, experiments made by M. Courtine on himself as well as on others, and on animals, to determine the physiological actions of *C. indica* on the nervous system, he thinks himself justified in believing that cannabine may be very useful in medicine as a narcotic and stupefying agent in the treatment of neuroses generally, and in the last stages of cancerous affections. Cannabine produces, although in a less degree than dawamesc, mæljoun, &c., tetanic effects; and in certain periods of its action it appears to enter into the class of general, exciting stimulants, such as strychnine, electricity, &c. Hachisch induces sanguineous congestion of the lungs, as M. Aubert-Roche has shown in persons suffering from the plague,—and as the author has found to be the case. This pulmonary congestion, which is not always induced by the use of hachisch may, however, be counteracted by blood-letting; and its occasional occurrence is not, in the author's opinion, a reason for rejecting this remedy, since it has been used with some advantage in some cases of hooping-cough and bronchial catarrh. The author also agrees with M. Moreau in thinking that cannabine may be found useful in the treatment of mental diseases.—*Christison's Dispensatory*, (new edit.) *and Gazette Médicale*, 1848.

On the Counteraction of the Ill Effects of Mercury by Dulcamara. By M. BRETONNEAU.

M. BRETONNEAU, from long observation, has convinced himself that the too prolonged use of mercury gives rise to symptoms, besides those due to its peculiar action on the nervous system, quite analogous to those of secondary syphilis. He has also been led to form a very high opinion of the power which the *solanum dulcamara* possesses as a preventive or as a curative of these. He lays great stress, however, upon its mode of administration. Two drachms (prepared as decoction) are to be given daily for the first week, four for the second week; and so on, adding weekly two drachms until ten are attained, which brings the patient to the sixth week, at which time the dose is to be gradually diminished, until the two drachms are again attained, when the medicine is to be discontinued.—*Rev. Méd.-Chir.* vol. iii, p. 40.

On the Use of Adansonia Digitata as a Substitute for Sulphate of Quinine. By M. DUCHASSAING.

M. DUCHASSAING, of Guadaloupe, having been led by the high price of sulphate of quinine to seek some other remedy against intermittent fevers, conceived the idea of employing the bark of Adansonia digitata. The result of a series of numerous experiments has tended to confirm the efficacy of this remedial agent, which is cheap, of an agreeable taste, exercises no action on the nervous system, but is favorable to the functions of digestion. M. Duchassaing has found these means succeed in cases where the strongest doses of sulphate of quinine had failed. One ounce of this bark, boiled in a litre of water till it was reduced one third, generally sufficed to cure these kinds of fever.—*Gazette Médicale*, May, 1848.

New Method of preparing Copaiva. By M. LOBEL-ANDRÉ.

M. LOBEL-ANDRÉ has submitted to the Academy of Medicine at Paris, a new mode of preparing copaiva, which guarantees the purity of the substance, and offers a more certain mode of cure. This method consists in the saponification of the resin of the balsam of copaiva. The resin, in itself indigestible, becomes saponified, and holds in solution the volatile oil. By this method, none of the constituents of

the balsam are eliminated, but are all rendered completely assimilable. The experiments made by M. Lobel-André himself, and several other practitioners, leave no doubt of the relative advantages of this method, by which, moreover, all pains in the epigastrium, malaise, eructation, and vomitings are avoided.—*Gazette Médicale.*

On the Efficacy of the Seeds of Phellandrium Aquaticum in Affections of the Respiratory Organs. By Dr. Michéa.

As Dr. Michéa freely admits, the use of phellandrium is no new discovery in medicine; but, notwithstanding the incontestable energy of its action, it is but little used in the present day. It is much more frequently employed in veterinary than in ordinary medicine; and its febrifuge, tonic, and even sedative properties, which were so highly appreciated by the practitioners of the last century, have been almost wholly neglected by those of our own day; we therefore owe a debt of gratitude to Dr. Michéa for having replaced this medicine among our therapeutic agents.

The following remarks give a brief exposition of its action, and of the facts advanced by Dr. Michéa in support of its employment. It is especially in bronchitis, chronic catarrhs, pulmonary phthisis, and asthma, that its use has been attended by the most favorable results. These seeds are at once stimulating and sedative, calm the cough, diminish the oppression, or wholly remove it by facilitating expectoration; exercising so remarkable an influence on the organs of respiration, as almost to verify the words of Lange, who said that these seeds put a stop to spitting of blood, arrested the development of pulmonary tubercles, by opposing their softening, whilst they contributed to the cicatrization of the cavities.

They may be taken twice a day, reduced to powder, in doses of 5 decigrammes, mixed with sugar; this medicine is, however, more agreeable, and appears to act with more certainty and rapidity, when given as a syrup. From two to four spoonfuls should be given without intermission for six weeks or two months; as it is only after that length of time that its salutary effects are appreciable.

Amongst other observations drawn from his own practice, M. Michéa cites three cases: one of incipient pulmonary phthisis, another of chronic bronchitis, which had resisted several other modes of treatment, and the third of nervous asthma.

Without entering into a detailed report of facts, the symptoms described leave no room for doubting the diagnosis established by Dr. Michéa; and whilst we take into consideration the mode of treatment previously adopted, the time, and the regimen prescribed conjointly with this medicine, we cannot deny the evidently favorable action which it exercised.

It should also be remarked, in support of the practical facts recorded by Dr. Michéa, that M. Recamier, more than twenty years since, frequently employed the seeds of phellandrium with undoubted success at the Hôtel-Dieu, for cases analogous to those above described.—*Bulletin de Thérapeutique.*

FORENSIC MEDICINE.

Medical Jurisprudence of Insanity.

The medical witness in this country oftentimes finds himself in a painful position, owing to the incapacity of judge and jury for the appreciation of the teaching of the sciences of which he is the interpreter; and in this way persons are subjected to all the penalties of crime of which their calamity should render them irresponsible, while others are allowed to endanger the peace of society, because their aberrations are not grossly palpable. Something of the same kind, it seems, prevails across the Channel; and the 'Gazette Médicale' takes the case of Marshal Mortier as an occasion for making a few remarks upon the subject. Having attempted the murder of his children, he was consigned to a lunatic asylum, but appealed to the

tribunals for his liberation on the ground of his sanity. He conducted his case with so much sagacity, ingenuity, and plausibility, that his counsel was enabled to make a powerful appeal in favour of the improbability of a man suffering under any aberration being able so to act. It required the tact and discrimination of medical men to be able to pronounce him partially insane, as evidenced by his groundless belief that he was hated by his friends, and his consequent desire of revenge.

An erroneous opinion, observes the Gazette, extensively prevails, especially among the members of the long robe, that every one is fitted to form a judgment on the question of moral irresponsibility; it being sufficient to be possessed of one's own reason, to decide whether that of another is perverted or not. And yet it should seem that the physician, occupied in observing the cerebral mechanism, both in its normal and perturbed states, and aided in such study by the confirmations or corrections derivable from these varied conditions, should be especially enabled to determine the state of this mechanism in a given case. There are two circumstances especially liable to lead those into error, who have not devoted prolonged consideration to the subject. The first is the *partial* form of certain alienations, a form far from being a rare one, however problematical the existence of a pure monomania, confined to a single idea or sentiment, may be. Sometimes the disturbance is purely instinctive, being rather a *want* or *impulse*, than a delirious *conception*, and the subject may be invincibly driven to murder or rob, without any apparent motive. Sometimes certain of the ideas or sentiments are disordered, and a perversion of these ensues. Except for such disorder, as well as certain peculiarities of character, great volubility of language, &c., the person may appear quite reasonable, and is often clever, cunning, and intellectual; and setting out from his false conceptions, he may pursue a perfectly logical chain of reasoning or actions. Demand of such a one how he came to kill his imaginary enemy; and a person not deeply versed in mental pathology might easily suppose he had acted knowingly, of his free-will, and with perfect self-consciousness. The lunatic may even favour this interpretation by displaying his motives and the proofs of his premeditation; but the instructed physician does not stop at these facts, but ascends to the source, and finding this to be a true alienation, he removes from the patient a terrible responsibility.

Another circumstance which renders the problem even yet more difficult for the inexperienced, is the fact that this partial insanity exhibits itself far more readily in regard to the *affections* than the *intellect*. Beings formerly the most dear to him excite beyond all others his aversion or distrust; and the lunatic need not offer any sign of intellectual disturbance properly so called. He is, according to the expression of Esquirol, a reasoning madman, but also an irresponsible one. The facilities for a misappreciation are here obvious. A patient who speaks at random, who is totally unable to reason, and is the victim of the strangest hallucinations, is at once easily recognised as a madman; but the lunatic of whom we are speaking resembles in so many points the generality of men, that the physician is perhaps alone enabled to recognise the sad affliction of which he is the victim.—*Gazette Médicale*, 1848, No. 8.

On the Influence of the Silent System in the Production of Insanity. By M. JORET.

M. JORET, the medical superintendent of the female prison at Vannes, recently forwarded a paper to the Académie de Médecine, giving an account of the great increase of insanity in that establishment since the adoption of the system of working and sleeping in common, but in utter silence, under pain of severe punishment for the slightest infraction of this. He stated, that of 872 admissions in 1842-4, 414 had been liberated, 19 pardoned, 8 removed to other prisons, 81 had died, and 25 proved insane; and this large mortality and number of insane he refers to the system adopted. In this opinion M. Collineau, the reporter on the paper, agrees. Himself superintendent of female prisons in Paris during 40 years, he has had full opportunity of proving that mere detention, if the prisoners are employed, will not give rise to insanity. M. Nacquart stated that, however true

M. Joret's observations may be as regards Vannes, it is certain they will not apply to many other prisons he has visited, at which the same system has been adopted. M. Ferrus, Inspector-General of Lunatic Asylums, explained that the intention of the government had been, that at the central prisons the inmates should work, eat, and take exercise in common and in silence, and sleep in separate cells; but that the crowded state of most of these prisons, and the fact of their not having been originally constructed for detention, prevented this from being carried into effect. This crowded state of the establishments has rendered the adoption of the silent system a matter of great difficulty, entailing numerous and severe punishments for its enforcement, which, conjoined with an insufficient diet and exercise, have produced a bad effect upon the moral and physical condition of the prisoners. The number of insane that these establishments produce is, however, much less considerable than M. Joret's statement would lead us to expect; the proportion in his establishment so far exceeding that of others, as to demand inquiry into the cause. Moreover, there the cases have been chiefly examples of general or partial maniacal delirium, while in the other prisons we find more of intellectual debility, giving rise to imbecility and dementia. "Indeed, most of these unfortunate beings, even on their arrival at the prisons, give tokens of these conditions; and long observation authorizes me to say, that if those of the accused whose mental condition appeared doubtful or very obtuse, were submitted to a careful medical examination, a certain number would escape condemnation, as being unable to appreciate the morality of their actions." M. Londe observed, that to carry this system out, the number of punishments rendered necessary on account of infraction of silence is truly enormous; and that not only does physiology point out the danger of suppressing the manifestations of our impressions, but statistics has repeatedly recorded the fatal results of the attempt.—*Bulletin de l'Académie*, t. xiii, pp. 489-505.

[While upon this subject, we may direct the attention of our readers to a highly interesting paper by M. Lelut, in the 'Gazette Médicale' for 1846. It details the results of a tour of inspection of the cellular prisons of France he had just returned from making, by desire of the government. He speaks unfavorably of the silent system; but of the *separate* or *cellular* system, adopted in about twenty-four or twenty-five of the principal prisons of France, he reports in warm terms of approbation. He found governors, chaplains, and medical attendants unanimous in their approval of the change which had been thus wrought in the condition of the prisoners, who, indeed, themselves fully appreciated the advantages of the system. Under its adoption the amount of mortality had become reduced a half or a third in different prisons; while insanity, which, among men at liberty, prevails in about the proportion of 2 per 1000, and which, upon the old prison system in France, averaged 10 per 1000, now ranges from 2 to 5 per 1000 in the various prisons, some not having had cases of its occurrence for very many months. He justly observes that, in reasoning upon this subject, people take no care to inform themselves of what are the true facts, and compare the results with those observed in man at liberty, instead of with those derivable from different forms of imprisonment.]

On the Time required to produce Death by Hydrocyanic Acid. By Dr. Sewell.

Dr. Sewell adds another case to the several now on record, proving that death by no means necessarily follows so speedily as once it was believed to do. In the present instance, seven drachms of medicinal acid were swallowed, after which the man was enabled to perform several deliberate acts, such as unlocking a door, calling out for aid, and returning to lie down on a sofa. He moreover had no convulsions, although these have been held by some to be always present when a longer interval than usual before death elapses.—*Boston Med. and Surg. Journ.*, No. 1031.

INTELLIGENCE.

ON THE EFFECTS OF CHLOROFORM.

We greatly regret having to place before our readers another case, in which the poisonous effects of chloroform have produced a fatal result. We cannot conceive that, by any ingenuity whatever, this result can be otherwise explained in the present instance; and it appears to us that a most serious liability is now thrown on all those, who encourage the use of chloroform as a means of escaping the pain of operations not themselves attended with even a remote danger to life. We still remain of the opinion expressed in our last Number, that in severe operations the probable benefit is far greater than the probable risk; but no one is justified, as it seems to us, in running such a risk for a mere avoidance of the pain of the extraction of a tooth. We have reason to know that, in the practice of a dentist who has most largely employed it, and whose success is much quoted as an argument in favour of its exhibition for this purpose, troublesome and long-continued disorders of the nervous system have followed its use in several instances.—The communication with which Mr. Nunneley has favored us will be read with interest, as suggesting a new and perhaps more advantageous mode of employing anæsthetic agents.

Report of the Principal Facts connected with a Fatal Case of Chloroform Inhalation which occurred in Cincinnati.

The subject of the following report, Mrs. Martha G. Simmons, was at the time of her decease thirty-five years and ten months old. Her husband states that she generally enjoyed excellent health; sometimes she was "nervous," and suffered occasionally with neuralgic pains about the face and pain in the ear, apparently arising from decayed teeth. She also suffered at times from "sick headache."

She was the mother of six children, five of whom are still living; her last accouchement occurred eight weeks previous to her death. Nothing unusual occurred, either at the time of parturition, or subsequently; her health remained good, and the ordinary quantity of milk was secreted.

On the 23d of February, she dined at a quarter past 12 o'clock, and after dinner walked to a dentist's, a distance of about three fourths of a mile, for the purpose of having some roots of teeth extracted. She arrived at the dentist's 15 minutes before 3 o'clock, appeared slightly flushed from the exercise of walking, but exhibited no alarm on account of inhaling the chloroform.

At 3 o'clock, 15 minutes after her arrival, Mrs. S. commenced inhaling chloroform; Mrs. Pearson and Mrs. Cross, two female friends, were present, and report the following as the events which occurred. The respiratory movements appeared to be free—chest heaving. While inhaling, *the face became pale.* At the expiration of about *one minute*, the instruments were applied, and four roots of teeth extracted. The patient groaned, and manifested what they regarded as evidences of pain, while the teeth were being extracted, although she did not speak, or exhibit any other sign of consciousness. As the last root came out—which was about two minutes from the beginning of the inhalation—patient's head turned to one side, arms became slightly rigid, body drawn somewhat backwards, with a tendency to slide from the operating chair. At this instant, Mrs. Pearson states that she placed her finger upon the patient's pulse, observed that it was feeble and immediately ceased to beat; respiration also ceased *about* the same time. The face, which was previously pale, now became livid, as also did the finger nails; the lower jaw dropped, and the tongue projected a little at one corner of the mouth, and the arms were perfectly relaxed. The females regarded her as being then quite dead. Efforts were made to resuscitate the patient—ammonia was applied to the nostrils, cold water dashed in the face, mustard, brandy, &c., applied. The patient was now removed from the operating chair and laid on a sofa; but she did not breathe, nor exhibit any sign of life, after being placed in the recumbent position.

Messrs. Meredith and Sexton, the dentists who operated in the above case, make the following statement: The patient took the chloroform vapour from Morton's inhaler; it contained a sponge (perhaps one third filling the glass globe of 4½ inches diameter) saturated with the liquid; to this 25 drops more were added when the patient began inhaling. Breathing at first slow; inhaled 12 or 15 times, occupying from a minute to 75 seconds. One of the dentists thinks she remained about *ten* minutes in the operating chair, and that life was not extinct until the end of that time; the other estimates the time at *five* minutes. One says he does not know whether she breathed after being laid on the sofa or not; the other thinks she did not. . . . .

After the patient was laid on the sofa, medical aid was sought, and Dr. A. H. Baker was the first physician who arrived; this was probably thirty minutes after respiration had ceased. He immediately pronounced her dead, but proceeded to employ vigorous measures for resuscitation. The principal means employed consisted in artificial respiration, electro-magnetism, and external stimulants. Professor Locke applied electro-magnetism, which caused active muscular contraction, but no evident effect on the heart. About an hour after the accident, Profrs. Mussey and Lawson arrived, and aided in the further employment of the means above specified. Not the slightest sign of life was manifested after the arrival of Dr Baker; the heart did not respond to the electricity, and the only change produced was some slight removal of the lividity of the countenance by the artificial respiration.

The *post-mortem examination* was made twenty-six hours after death. Present—Drs. Mussey, Lawson, Baker, and Mulford.

Examination by Dr. Lawson. Record by Dr. Mussey.

External appearances.—Lips livid, but face pale; bloody froth issuing from the mouth. Anterior surface of body and limbs free from discoloration, but posteriorly the skin presented a deep livid hue. Cornea dull and flaccid, and a dull-red horizontal belt extended across each eye, corresponding to the part which was unprotected by the lids; this belt was one tenth of an inch in diameter, and made its appearance a few hours after death. Limbs quite rigid. Abdomen distended with gas. Patient rather muscular; weight probably from 140 to 150 pounds; hair dark; eyes dark brown; temperament sanguineo-bilious.

Brain.—Integuments contained but little blood. On removing the upper part of the skull, a larger quantity of blood than usual flowed from the vessels of the dura mater. Superficial vessels of the brain moderately distended; two or three ounces of fluid blood, intermixed with bubbles of air, flowed from the sinuses of the dura mater. General aspect, colour, and consistence of the brain normal.

Lungs.—Considerably but not intensely congested; crepitated freely at all points; no extravasation. Lining membrane of bronchi slightly congested, apparently the result of recent catarrh; deeply stained by the blood. Pleura at all points highly injected; six drachms of bloody serum in the right, and two ounces in the left chest.

Heart and large Blood-vessels.—Pericardium contained six drachms of bloody serum. Heart flaccid, *and all its cavities entirely empty;* inner surface of both ventricles and auricles deeply stained. Aorta and pulmonary artery empty; no blood in the cava within the chest, and a very small quantity in the part which lies within the abdomen; indeed, so small was the amount that it could not be appreciated until the vessel was opened. Lining membrane of all the blood-vessels deeply stained.

Abdomen.—One ounce and a half of bloody serum in the right hypochondrium. Stomach and intestines distended with gas. Partially digested aliment, amounting to about three gills, was found in the stomach. Liver paler than natural, arising from the absence of blood; kidney considerably engorged. No marks of previous disease in any of the abdominal organs. Uterus and bladder normal; the former exhibited the usual condition of the organ two months after delivery.

Blood.—Fluid as water in every part of the body; not a coagulum was seen in any vessel. Examined with the microscope, the globules appeared altered some-

what in form; some were irregular in shape, and they seemed generally distended and more globular than is normal; they were also somewhat fragmentary, a part apparently having been ruptured; their number seemed somewhat diminished. The colour, in every part of the system, was that of dark venous blood.

Sympathetic nerve.—The sympathetic nerve, together with its larger ganglia, including the semilunar ganglion, presented a natural colour.

The Chloroform used.—The specific gravity of the chloroform employed was found to be 1·3. It contained some alcohol, but upon the whole is regarded as a fair article; it was the same which the dentists had previously used in numerous cases without any unpleasant results.—*The Western Lancet and Hospital Reporter*, March, 1848; and *American Journal of the Medical Sciences*, April, 1848.

On the Physiological Local Effects of Anæsthetic Agents. By T. Nunneley, Esq.

Some very important and novel views on this subject were brought before the branch meeting of the Provincial Association at Leeds, on June 7th, 1848, by Mr. Nunneley of that town. This gentleman stated that for many months he had been engaged in making experimental researches on those agents, with a view to ascertain, as far as possible, the modus operandi, the doses which may be borne with impunity, and the different modes of application; as well as, in case of an overdose, the best means to be adopted to counteract it. His experiments have not merely extended to the common anæsthetic agents employed, such as ether and chloroform, but he has been endeavouring to ascertain whether or not there may be some others, which may either be more safely administered, or may possess still greater advantages than the usual agents employed: this being an inquiry which he thought to be quite worth pursuing, inasmuch as it appeared to him that the agents now used were so rather as the result of accident, so to speak, than from having been proved by extended experimental research to be the best existing. He stated that he believed it not improbable that it would ultimately be found that all those preparations, which have a *radical basis*, (in the language of modern chemistry), such as acetic ether, bisulphuret of carbon, aldehyde, and many others of an analogous character, upon some of which he has made extensive experiments, would be found to possess similar properties on the animal economy. He did not intend to enter on this important question generally, which he reserved for another occasion.

This much Mr. Nunneley was prepared to state, *that chloroform appeared to be most deleterious to life*, to require the greatest care in its administration, and that the boundary up to a fatal dose is by no means well marked—that of two animals, in apparently the same condition, the same dose being given in precisely the same way to both, the one may speedily die, while the other will bear it with impunity, —that from the effects observed, he has reason to think the ultimate effects are in some respects not dissimilar to those produced by prussic acid,—that to some animals, as for instance the newt, the frog, the toad, some fish, slugs, snails, and some insects, the effects are *more* rapidly fatal than prussic acid of Scheele's strength; and that, even in higher animals, when under the influence of an incomplete dose, or recovering from the effects of a large dose of either chloroform or prussic acid, the phenomena are in many respects very similar; and further, that the numerous post-mortem examinations which he has made, fully corroborated this opinion. He stated that acetic ether, with which he had made numerous experiments, possessed very considerable anæsthetic powers,—that bisulphuret of carbon does also, to some extent, possess similar power, and so far as his experiments go, it is very important to add, that this power is of a safe character, the animal speedily recovering.

But of all these remedies, he believes that sulphuric ether will be found the safest and least noxious to life. On these points Mr. Nunneley intends hereafter to lay his experiments, already very numerous and varied, before the profession. His chief object on the present occasion, was to call the attention of the profession to experiments proving, as he thinks, the value and safety of a *new mode* of administering these agents.

His object on the present occasion was to show that the action of all or most of these agents might be produced *locally by local application*, the sensorium being unaffected, consciousness being retained, and the limbs not subjected to their influence being unaffected. He stated that either by *immersion* in a small quantity, or by the *vapour applied* merely for a limited period, a limb may be rendered *perfectly motionless and senseless*, and, what may be an additional advantage, *fixed in any desired position*. He stated that he had immersed his finger in these fluids for about half an hour, and an hour, and at the end of this period the finger was nearly powerless and insensible, it being forty-eight hours before the effects entirely disappeared, a sensation of heat and discomfort extending along the track of the nerves to the axilla; that before operating on a difficult case for artificial pupil, he had applied for twenty minutes a small portion of the vapour of chloroform to the eye, by means of a small jar which accurately fitted the orbit, with the effect of rendering the parts nearly insensible. The first effect of these agents when locally applied is to produce redness, heat, and smarting, which subside, followed by swelling and redness of the integuments, which remain for some time. Mr. N. stated that he could completely paralyse any limb of frogs or toads by immersion or exposure to the vapour in about five minutes or less; and he mentioned, as a curious fact, that if the exposure to the influence were continued longer than was sufficient to produce a local effect, this influence extended to the corresponding limb of the other side: thus, for instance, if one hind-leg became *too much* influenced, the other hind leg partook of the same effect—if the fore leg were too much affected, then the other fore-leg became so likewise, and subsequently the whole body—a result which Mr. Nunneley mentioned as strongly corroborative of his experiments with prussic acid, as detailed in the last volume of the Provincial Transactions, and strongly supporting the opinions of Dr. Marshall Hall on "reflex action." These views were illustrated by a series of interesting experiments before a highly respectable audience of medical men, on frogs and toads, in which, after immersion for a few minutes, the limbs became insensible, and were amputated in repeated portions without any symptoms of pain whatever.

The experiments which Mr. Nunneley performed before the meeting were perfectly successful and satisfactory; and if those views should prove to be correct, which we think very probable, they will give a new impulse to the use of those agents, and enable the most cautious practitioner to use them without the danger which may attend their internal administration.

He stated that by this new mode of application to the hind-legs of rabbits, he had been enabled to amputate the toes without the least indication of feeling—that he was not prepared to state what was the best mode of applying it, or the exact quantity to be used, which obviously can only be determined by a very lengthy series of experiments on different animals, which he is at present zealously pursuing, his principal object being to make known the important *physiological local effects* of anæsthetic agents generally, which we believe have not hitherto been announced.

THE LATE DR. HOWARD, OF MANCHESTER.

It is for the interests of humanity that a proper tribute should be paid to the memory of those, whose virtues have contributed to the welfare of mankind. It is also desirable that discrimination should be exercised towards merit according as it is deserved, and that mere popularity or meretricious pretension be distinguished from real worth and solid acquirements. It is with peculiar pleasure, therefore, that we call attention to the character of a late physician, whose virtues were too quiet and unostentatious, and whose professional opinions were too honest and too much in keeping with the modesty of truth, to be readily appreciated by the public mind.

Richard Baron Howard, whose recent death has given occasion to the following remarks, was born on the 18th of October, 1807, at Melbourne Farm, in the East Riding of Yorkshire. He was the sixth son of Mr. Charles Howard, of Hull, and

Mary Baron, of Manchester, who were married on the 26th of March, in the year 1800. Of Dr. Howard's early life we are not in possession of many details, nor is there, perhaps, anything of particular public interest in the quiet and studious manner in which his youth was spent. To trace the formation of his tastes and habits would offer a more pleasing occupation, if it were possible to arrive at the real influences which determined the direction of his pursuits, or to offer more than mere conjectures as to the circumstances which had so happy an effect on his future dispositions. We may, however, state that Dr. Howard had the advantage of the best parental influences. In July 1817, Dr. Howard was placed at a school in North-allerton, under the care of a Mr. York; where he passed creditably through the usual routine of elementary education. At the completion of his studies, after a period of about six years, he first evinced a desire to be educated as a medical practitioner; and he accordingly removed to Edinburgh, where, after the usual period of apprenticeship, which he spent with Messrs. Scott and Orr, of that city, he obtained a surgeon's diploma from the Edinburgh College. In 1829 he became a licentiate of the Apothecaries' Company in London. About this period, being desirous of accepting the appointment of surgeon to an Indiaman, and finding the degree of M.D., a necessary preliminary, he returned to Edinburgh and obtained this further qualification; his thesis "De Hydrocephalo acuto" being printed, with a dedication to his father. A vacancy, however, now arose in the situation of physician's clerk to the Manchester Infirmary, and the post was thought peculiarly eligible for him, as many of his maternal friends and relations were resident in Manchester. After an active canvass, he was fortunately elected, although it is probable that exception would have been taken to his appointment, if it had been known that he was then in possession of a physician's diploma. The situation of physician's clerk to the Manchester Infirmary is one of great responsibility; and to those unacquainted with the regulations of the establishment, its appellation will not fairly represent its character. The Infirmary at Manchester not only serves as an hospital for the reception of the sick, but sends out visitors to attend the poor at their own dwellings, in all parts of that extensive and densely-populated town: it is chiefly required of the clerks to attend to this latter department; and it will be easily imagined, that the cases which come under their care are both considerable in number and various and important in their nature. From this source, therefore, in the very first instance, Dr. Howard had a large field for practical study; and he availed himself of the advantages which it afforded with no common assiduity. These labours were unfortunately interrupted by a severe attack of fever, with which he was seized in February 1831. During the period of his illness he was under the kind and attentive superintendence of Dr. Lyon, of Manchester; and, we understand, became the inmate of an institution which was afterwards eminently the scene of his future labours. The Manchester Fever Hospital, to which we allude, though existing as a separate building, is connected in all that relates to the care of the sick with the general hospital or infirmary; and as Dr. Howard was a resident officer of the latter institution, it was probably thought desirable that he should have a temporary accommodation in the former. The circumstance, trivial as it is in itself, is at least deserving of mention, inasmuch as Dr. Howard subsequently gave special attention to the subject of fever, and showed a peculiar interest in the management of the fever wards.

In July 1832, Dr. Howard tendered his resignation as clerk to the physicians, having completed the term of his engagement, which was a period of three years. No other candidate, however, coming forward, he was unanimously requested by the weekly board of trustees to continue another year in the situation; it was not, therefore, until July 1833, that his resignation was finally accepted. On the 31st of October, 1833, Dr. Howard was elected resident medical officer to the Manchester workhouse. In this establishment he had still further opportunities of pursuing his profession, and particularly of becoming practically acquainted with the physiognomy of disease, and of rendering himself familiar with the baneful effects of poverty and distress. In April 1834, his hospital duties were again interrupted by ill health; an attack of rheumatic fever obliged him to suspend his

labours for several months; and when he returned to his appointment, he was still in a state of imperfect health. In February 1838 he resigned his situation, having resolved to commence practice in Manchester as a physician; but previously to taking up his new position, he spent a short time in visiting the hospitals of London and Paris. In April 1839, Dr. Howard was elected physician to the Ardwick and Ancoats Dispensary; an institution which, though unpretending in its exterior, ministers largely to the relief of the poor in a district where such relief is particularly needed; and more actual service is certainly required from the medical officers holding this appointment, than from many of those who are associated with institutions of greater name.

During this year Dr. Howard first appeared as an author, in a small publication 'On the Morbid Effects of Deficiency of Food.' It will have been seen that Dr. Howard had for many years been diligently employed in professional ministrations to the poor; and he could not but feel how intimately their diseases, and even their whole moral and physical condition, are connected with the actual privations which their circumstances impose. This, therefore, was the subject which first engaged his pen, and which always continued seriously to occupy his mind. His known interest in these inquiries afterwards induced the Poor Law Commissioners to request his aid in drawing up a report as to the extent to which contagious diseases prevail amongst the labouring classes in Manchester. This report appeared in June 1840, in the 'Sanitary Inquiry in England;' and at a later period, a communication from Dr. Howard on a kindred subject, was inserted in Mr. Adshead's pamphlet on the distressed state of the labouring classes in Manchester. In March 1842, on the death of Dr. Pendlebury, Dr. Howard was enabled to obtain the appointment of physician to the Infirmary, for which he had previously been an unsuccessful competitor. In March 1844, contrary to his own wishes, but by the advice of his friends, he accepted the office of physician to the Haydock Lodge Lunatic Asylum. He had afterwards reason to regret his connexion with this establishment; for as his visits were made of necessity at considerable intervals—though always regularly, and in accordance with the agreement entered into—he found it impossible properly to superintend the care of the inmates; and not approving of the manner in which affairs were conducted, and yet feeling his responsibility, he was led to resign after much mental uneasiness. In the autumn of this year, Dr. Howard was appointed lecturer on the practice of physic, to the Manchester Royal School of Medicine and Surgery; and in the following year he commenced a supplementary course on the subject of auscultation.

Having thus traced Dr. Howard's career, until he had placed himself in the highest public situation which a medical man could well attain in a large provincial town, we shall pause to consider his position as a practitioner, and his character and habits in private and domestic life.

The mere duration of a medical man's connexion with public charities is no just criterion either of the obligation which the public owe to him, or of the amount of actual experience which he possesses; happily, however, the assiduous attention and untiring patience which Dr. Howard paid to the sick, were combined with an enlightened spirit of inquiry which is altogether superior to mere routine. Nor was Dr. Howard to be classed with that school of practitioners, whose zeal shows itself in a somewhat reckless pursuit of fame, and which aims more at reputation than the well-being or safety of the patient. He always viewed his hospital engagements as duties which he had voluntarily undertaken, and which were therefore equally sacred with every other in which he could be employed. His labours for the good of mankind, which were honest and unremitting, rendered him indeed worthy of a name (Howard) which has already distinguished a great benefactor of his race.

Perhaps if one thing more than another characterised the temper of Dr. Howard's mind, it was his love of sincerity and his perfect freedom from the common cant of artificial life. This operated no doubt in some degree against his immediate or rapid success; but it secured to him a feeling of internal honesty and truthfulness, which reacted beneficially on his thoughts and actions. It was

laying the foundation of a reputation, both with the profession and the public, which would ultimately have placed him in the highest scale in general estimation. He was far from being a sanguine man; probably he wanted more of what has been called the *poetic temperament*. He had no great powers of imagination, and certainly no keen relish for the fine arts. He was somewhat indifferent to the charms of music, and at least not more than ordinarily sensitive to poetry. More warmth of temper would, in our estimation, have given expression to a character otherwise almost perfect. But could we have added this, without destroying that calm complacency and cool judgment, which we so much admired in this amiable physician? Could we have found in him the same refuge for the troubles of less tranquil spirits? Could we have had that same quiet equanimity, in which confidence could always be equally reposed? Too often we expect qualities which are not easily compatible, if not actually contrary in their nature; and we should at least admit that the warmth of the poet may betray itself in eccentricities and irregularities, "which make knowledge useless, wit ridiculous, and genius contemptible."

Let it not, however, be understood that Dr. Howard was deficient in sensibility. His attachments were strong and lasting; and his sense of propriety delicate even to fastidiousness. In manners, Dr. Howard was so unassuming, and his opinions were so quietly and unostentatiously given, that those who consulted him might almost be led to suppose that his suggestions were self-evident, and not the result of mature study and reflection. His tone of voice was generally low and kind, with that friendly, familiar sort of modulation, which always wins upon the confidence. To the poor he invariably adopted the same manner. But in connexion with this gentle persuasiveness of conduct, he united a firmness of purpose, which a consciousness of the importance of his advice and of the sacred duties of his profession never failed to inspire. His kindliness never degenerated into weakness, nor stooped to a familiarity undeserving of his position. He was always the real friend of his patients, desirous rather to promote their welfare than to gain their good opinion, and not only considering their interests in his casual and professional interviews with them, but carrying his solicitude even to his own fireside. There were few of his patients who did not consider themselves as his personal friends;—they felt that in putting themselves under his care, they had come within the reach of his interest and sympathy;—they felt that to be known to Dr. Howard was to have secured the notice of one who was as humane as he was enlightened, and who did not regulate his life by a sordid scale of interest, but extended his generous sympathies to all classes and conditions of life, where suffering, poverty, or distress had claims on his ministrations. Nor was there anything fanatical or eccentric in his benevolence; he could not even be called one of those whom conventional language has somewhat arbitrarily named *evangelical*. His religious feelings were manifested chiefly in the impulse they gave to his practical exertions for the benefit of others. This is not the place to enter fully into the subject of religious convictions or belief; but we think it only right—lest the omission should be wrongly interpreted—to state that he was neither inattentive himself to the cultivation of piety, nor did he underrate the consolations it affords.

Considered as a physician, Dr. Howard's true value was chiefly apparent in the immediate practice of his profession. His freedom from prejudice, the caution with which he deduced his conclusions, the equal distribution of his knowledge, and, above all, his great experience, contributed to a common result. Dr. Howard had a rational conviction of the importance of medical treatment, and of the efficacy of remedial means; but in many instances he acknowledged its utter impotency to achieve particular indications. His treatment was sufficiently bold and decisive in acute diseases; but was mild, temporising, and what is called *expectant* in the more chronic affections. He was not of that apothecary-school of practitioners, who consider medicines as carrying in themselves the elements of cure, independent of a nice and just application of them to the circumstances in which they are required.

When we consider how many failures are necessary to success, how many incorrect notions can only be slowly and reluctantly abandoned, even by thinking and conscientious men, we must, indeed, regret that so much practical knowledge,—the result of so much careful thought, and of so many hours of mental discipline and patient investigation,—is lost to the world. It is a common and trite remark that the ways of Providence are mysterious and inscrutable; and it *does* seem, in a human point of view, a misfortune of great moment, that the harvest of a well-spent life should be thus lost after the toil and burden of the day.

Dr. Howard's writings, though comparatively few, are valuable as the record of the impressions which his large experience had communicated to his mind. They partook of that practical and useful character to which the bias of his mind and the direction of his thoughts might naturally have been supposed to lead. His propositions were advanced with caution and after mature consideration; and were of that common-sense kind which does not affect novelty nor pretend to theoretical ingenuity. His little treatise on the 'Morbid Effects of Deficiency of Food' perhaps claims as much attention from the fact of its bringing an important subject before the public, as it does from any great freshness of matter or originality of composition. But yet the views which Dr. Howard set forth, if not new to the scientific public, could not be said to be sufficiently known; and certainly his remarks on the diagnosis of starvation, drawn as they are from a painful acquaintance with the miseries of the poor, are full of value and deserving of publication. It is, perhaps, not possible to make evident in writing those fine shades of colour which give complexion to our diagnostic opinions, and enable us to recognise, with almost intuitive rapidity, the lineaments of disease and the countenance of wretchedness and want. But to direct attention to the prosecution of such observations is manifestly conducive to such an end, and the remarks offered must aid, if well considered, in saving many from a lamentable and untimely end. It is with pleasure that we everywhere find, throughout the work, traces of that kind and charitable spirit which always animated the author. A treatise which does not much exceed the limits of a pamphlet rarely continues to be largely read after the period of its first publication. It is to be regretted, therefore, that Dr. Howard did not live to extend his publication, and enlarge his views on a subject which is neither ephemeral in its interest, nor fairly represented in our permanent literature. We have, truly, "the poor always with us," and with poverty we have the miseries and hardships with which it is inseparably linked; and we have also the duties which our relation to the poor entails upon us, and for which our Christian profession makes us responsible.

Dr. Howard gave a very close attention to the subject of fever, both in his attendance at the fever wards in Manchester, and also in the direction of his studies. The consideration of fever was indeed with him a favorite subject of professional research. From the importance which he attached to the writings of Dr. Alison on this subject, these may probably be considered as a fair expression of his own views. We know that he laid much stress on the ill effects arising from the overcrowding of the inferior lodging-houses; and the influence of starvation appeared to him at least equally potent in the propagation of contagion, when the febrile poison was once established. We regret that he has not left us the full result of his generalizations on this subject; but we may gather much valuable information from the sanitary reports to which we have already alluded. Dr. Howard did not seem to us to excel as a *lecturer*, using the term in relation to the *vivâ voce* communication of knowledge. His lectures were certainly excellent in themselves, prepared with great care and labour, well digested, well arranged, and fairly in accordance with the recent doctrines of the day. But more is needed in a lecturer than this accuracy and system; there is the mode of delivery, the look, the gesture, the happy seizing on the principal features of the subject, which give that sort of intelligence between the pupils and the lecturer, which may be said, not altogether figuratively, "to speak volumes." But if Dr. Howard did not possess that happy and half colloquial mode of delivery, which makes the knowledge

it imparts as impressive as it is entertaining, he possessed those sterling qualities which must always demand respect and secure attention. He was so regular in his attendance, so courteous in his manners, so anxious to promote the interests of his pupils, and so full and accurate in his preparations for the class, that it was impossible not to attach the greatest value to his teachings. As a clinical lecturer, Dr. Howard appeared to greater advantage. The correct idea which he entertained of the requirements of the clinical teacher, is evident from an Introductory Lecture which he published on first entering upon these duties. Besides the general course of lectures on the practice of physic, and the clinical lectures delivered at the Infirmary, Dr. Howard gave a separate course upon auscultation; a subject to which he had given especial attention, and on which his opinions possessed a standard value.

Though Dr. Howard could not be regarded as an original scientific investigator, he was always desirous of keeping pace with the advancement of the day in those departments of natural knowledge which are collateral to medicine; and he had connected himself with all the principal institutions in the town, which are designed to promote the extension of science. Amongst these we may mention the Literary and Philosophical Society, the Geological Society, the Medical Society, the Medico-Ethical Society, &c. &c. Dr. Howard was also far from being inattentive to the cultivation of language and literature, the study of which seemed to relieve him from severer or more anxious occupations, as well as to enlarge his ideas and adorn his mind.

Dr. Howard's position then, considered thus as a lecturer, as a practitioner, as a scholar, and as a philanthropist, embodies all we could well expect or desire to complete the character of a physician. But knowledge passes away; and the proudest success even of good men is not of long duration. At the period of his greatest usefulness, he became the subject of an [illegible] to contemplate his own death, when he had just come within the reach of all that a sensible man can well desire to attain. He bore his sufferings with quiet resolution. They were great and long continued; he never desponded, however, but was willing to be encouraged against hope, and always received with attention and confidence the professional services of his friends, Dr. Lyon and Mr. Beever. At length life became burdensome; he sought change of place; and after spending a short period with his valued friend Mr. Gaskell, at Lancaster, he went to York, where his parents now reside, and expired on the 9th of April, 1848. He was buried at the cemetery near that city.

Those who enjoyed the advantages of intimacy with a friend so sincere and intelligent, will feel his loss long, as well as severely. They will feel the need of the sympathy and ennobling influence of a mind, which tended to elevate and refine those who came within its sphere. May they emulate his virtues, and seek to carry out their benevolent intentions with the same honesty of purpose, the same suavity of disposition, and the same intelligence and judgment.

BOOKS RECEIVED FOR REVIEW.

On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the Connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By William Prout, M.D., F.R.S., &c. Fifth Edition, revised. London, 1848. 8vo, pp. 595.

Will be reviewed in our next.

Clinical Observations on the Pathology and Treatment of Continued Fever. By Edward Latham Ormerod, M.B. London, 1848. 8vo, pp. 244.

Will be reviewed in our next.

Copy of Dr. King's Report on the Fever at Boà Vista. Folio, pp. 16.

Dr. M'William's Remarks on Dr. King's Report. Folio, pp. 15.

Hindu Medicine: a Commentary upon a Commentary. By Fred. J. Mouat, M.D., F.R.C.S. (From the Calcutta Review.) Calcutta. 8vo, pp. 55.

Fifth Annual Report of the Managers of the State Lunatic Asylum, made to the Legislature, Jan. 19, 1848. Albany (New York), 1848. 8vo, pp. 72.

Physician's Annual Report to the Managers of the Royal Edinburgh Asylum, for the year 1847. Edinburgh. 8vo, pp. 25.

On the Habitual Use of Opium at Singapore. By R. Little, Surgeon. (From the Journal of the Indian Archipelago.) Singapore. 8vo, pp. 79.

Of this remarkable inquiry we shall give a summary in our next.

On Functional Diseases of the Liver, associated with Uterine Derangement; embracing the consideration of Special Physiological and Pathological relations hitherto unnoticed. By Butler Lane, M.D., M.R.C.S.E. London, 1848. 8vo, pp. 32.

A Treatise on the Gout: its Nature and Treatment; being the Substance of a Paper read before the Medical Society of Guy's Hospital, Jan. 1848. By H. B. C. Hillier, M.D. London, 1848. 8vo, pp. 23.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. Fasciculus III. Four Plates. London, 1848. Folio.

On Wounds and Injuries of the Chest; being the Third Part of Lectures on some of the more important points of Surgery. By G. J. Guthrie, F.R.S. London, 1848. 8vo, pp. 109.

Lectures on Diseases of the Eye. By John Morgan, F.L.S., late Surgeon to Guy's Hospital, &c. Second Edition, carefully revised and enlarged, with Notes, by John F. France, Surgeon of the Eye Infirmary and Lecturer on Ophthalmic Surgery at Guy's Hospital. London, 1848. 8vo, pp. 222.

[illegible] of the best Modes of Operating, Continental and British. By Hugh Neill, Surgeon to the Liverpool Eye and Ear Infirmary. London, 1848. 8vo, pp. 224.

The two preceding works will be reviewed in our next.

A New and Improved Synoptical Table of the Diseases of the Human Ear, with their Symptoms, Causes, and Treatment. By William Harvey, M.R.C.S.E., and Thomas Buchanan, Surgeon to the Hull Dispensary for Diseases of the Eye and Ear. London, 1848.

Will be noticed in our next.

Gray's Supplement to the Pharmacopœia: being a Concise but Comprehensive Dispensatory and Manual of Facts and Formulæ, for the Chemist and Druggist and Medical Practitioner. By Theophilus Redwood, Professor of Chemistry and Pharmacy to the Pharmaceutical Society of Great Britain. Second Edition. London, 1848. 8vo, pp. 1070.

Will be noticed in our next.

A Dispensatory, or Commentary on the Pharmacopœias of Great Britain; comprising the Natural History, Description, Chemistry, Pharmacy, Actions, Uses, and Doses of the Articles of the Materia Medica. By Robert Christison, M.D., V.P.R.S.E., Professor of Materia Medica in the University of Edinburgh, &c. &c. Second Edition, revised and improved, with a Supplement, containing the most important new Remedies. Edinburgh, 1848. 8vo, pp. 1004.

Will be reviewed in an early number.

Treatise on the Falsifications of Food, and the Chemical Means employed to detect them. By John Mitchell, M.C.S., &c. London, 1848. 12mo, pp. 334.

Hospital Elections and Medical Reform, addressed to the Governors of St. George's Hospital and to the Profession. By Edwin Lee. London, 1848. 8vo, pp. 44.

Remarks of this character would obtain more attention, if proceeding from any other source than a disappointed candidate.

Recent Advances in the Physiology of Motion, the Senses, Generation, and Development; being a Supplement to the Second Volume of Prof. Müller's 'Elements of Physiology.' By William Baly, M.D., F.R.S., &c. &c., and William Senhouse Kirkes, M.D. London, 1848. 8vo, pp. 132.

Observations on the Cultivation of Organic Science, being the Hunterian Oration for 1848. By Richard D. Grainger, F.R.S., &c. London, 1848. 8vo, pp. 60.

Record of Cases treated in the Mesmeric Hospital, from June to December, 1847. With Reports of the Official Visitors. Calcutta, 1848. 8vo, pp. 183.

Will be noticed in our next.

The Plant; a Biography. In a Series of Popular Lectures. By M. J. Schleiden, M.D., Professor of Botany to the University of Jena. Translated by Arthur Henfrey, F.L.S., &c., Lecturer on Botany at St. George's Hospital. With Five coloured Plates and Thirteen Wood-Engravings. London, 1848. 8vo, pp. 365.

Will be reviewed in our next.

A Treatise on Diet and Regimen. By William H. Robertson, M.D., &c. Fourth Edition, re-[illegible] and much enlarged. Vol. II. (completing the work). London, 1848. Small 8vo, pp. 362.

Will be reviewed in an early number.

The Microscopic Anatomy of the Human Body. By Arthur H. Hassall, F.L.S., &c. Part XIII.

Prison Discipline; and the Advantages of the Separate System of Imprisonment: with a detailed Account of the Discipline now pursued in the New County Gaol at Reading. By the Rev. J. Field, M.A., Chaplain. London, 1848. Two vols., 8vo, pp. 900.

Will be reviewed in an early number.

Popular Lectures on the Prevailing Diseases of Towns; their Effects, Causes, and the Means of Prevention. By William H. Kebbell, M.D. Brighton, 1848. 12mo, pp. 196.

Sanitary Ramblings: being Sketches and Illustrations of Bethnal Green. A Type of the Condition of the Metropolis and other Large Towns. By Hector Gavin, M.D., F.R.C.S.E. London, 1848. 8vo, pp. 118.

The two preceding works will be noticed in our next.

The Ethnological Journal; being a Magazine of Ethnography, Phrenology, and Archæology, considered as Elements of the Science of Races; with the Applications of this Science to Education, Legislation, and Social Progress. By Luke Burke. No. I, June, 1848. pp. 48.

Chemie und Mikroskop am Krankenbette. Ein Beitrag zur Medezinischen Diagnostik, mit besonderer Rucksicht auf das Bedürfniss des practischen Arztes bearbeitet, von Dr. Mark-Aurel Hoefle, prackt. Arzte und Dozenten der Medizin an der Universitat Heidelberg. Mit Holzschnitten und drei Steindructafeln. Erlangen, 1848. 8vo, pp. 683.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1848.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *Discourses on Medical Education, and on the Medical Profession.* By John Ware, M.D., Hersey Professor of the Theory and Practice of Physic in the University of Cambridge (New England).—*Boston (N.E.)*, 1847. 8vo, pp. 113.
2. *Medicine an Art, and its Truths to be attained. Being an Address, read Jan.* 31, 1848, *at the Opening Meeting of the "Library of the Exeter Dispensary" and the "Devon and Exeter Pathological Society."* By Thomas Shapter, M.D., Physician to the Devon and Exeter Hospital, &c. &c.—*London*, 1848. 8vo, pp. 32.
3. *On the Aims and Philosophic Method of Pathological Research. An Inaugural Address, delivered at St. Thomas's Hospital, Dec.* 15, 1847. By John Simon, F.R.S.—*London*, 1848. 8vo, pp. 52.
4. *Outlines of Medical Proof.* By Thomas Mayo, M.D., F.R.S.—*London*, 1848. 8vo, pp. 48.

Although the four Discourses, whose titles we have placed at the head of this article, differ widely from each other as to their subjects, they have nevertheless one common object; namely, to contribute towards the elevation of the character of the medical profession, by setting up a high standard of attainment, and by indicating the right means of reaching it. It is our present purpose to follow up the remarks which we offered in our last Number upon the ethical relations of medical men, with some observations upon their intellectual position,—upon the demands which the public, the profession, and the interests of truth, make upon their powers of reasoning and judgment,—and upon the means by which those demands may be most satisfactorily met, in other words, the kind of preparatory training which the medical student should undergo. In following out our plan, we shall freely avail ourselves of the materials supplied by the authors before us; each of whom has supplied us with thoughts appropriate to our purpose.

The first of Dr. Ware's discourses was delivered in May, 1847, before the Massachusetts Medical Society; an association which is intended to include all the regularly educated physicians and surgeons of the state, and which possesses within itself the power of regulating all matters relating to medical education and practice within its limits. It may not be known to our readers, that numerous as are our examining and licensing boards, those of the United States are far more numerous; nearly every one of the principal states having at least two colleges possessing or assuming the power to grant medical degrees; and the license to practise being obtained from a State Medical Society, in cases where the possession of a degree does not itself confer the privilege. These state societies, like the colleges, are entirely independent of each other, every one making such regulations as it may deem most expedient; and the consequence is, as might be anticipated, that the standard of education and of information varies very widely in the different states. In some of them, indeed, free trade in medicine is carried to its fullest extent; any man being permitted to set up as a doctor, to prescribe, and to receive fees, without either diploma or license. As might be expected from the superior intellectual tone of New England, the standard of medical education is higher there than in most of the other states; and the powers enjoyed by the medical society in regard to the prevention of unlicensed practice, tend to keep quackeries of various sorts at a lower level than they attain in other parts of the Union, where they carry their heads at no small elevation. Still we infer from Dr. Ware's address, and from other information we have received, that quackeries of various kinds obtain more of the sympathy of the public, even in New England, than they do in our own country; and that the "free and enlightened citizens" of the United States, consider it a hardship for any man to be prevented from exercising any calling which he may select, and think it an undue check upon public liberty for any individual to be prevented from consulting an unqualified practitioner if he choose to do so, the "regulars" having no right to assert a monopoly of medical practice, but being merely competitors with the unlicensed, with the advantage of a certain *prestige* in their favour. It is to this state of things that Dr. Ware addresses himself, in that discriminating, independent, and philosophical tone, which is characteristic of all that he has given to the public. And the counsels of such a man, in regard to the character and relations of the medical profession, cannot but be of interest and value to his professional brethren on this side of the Atlantic.

"I cannot but think it singular," he says, "that society takes so little interest in a subject which every one, on reflection, must admit to be of the utmost consequence. In his own individual case, every one feels and acknowledges the great importance of the character and acquirements of his medical attendant; yet, as a whole, mankind seem profoundly indifferent as to all provisions for the education and the formation of the character of those who are to sustain this relation to them. My object will be answered if I shall have contributed to lessen in any degree this indifference, and to produce the conviction that medical education and the formation of the medical character, are not merely an affair of the profession, but one in which the community also have a deep interest; and whilst, on the one hand, I would have the members of our profession entertain high views of the duty they owe to society, I hope, on the other hand, that society will learn to feel that there is a duty also on their side which they have not always performed in regard to us." (p. vi.)

After some introductory observations on the peculiar position of the

members of the medical profession in a republican community, of whose aristocracy they constitute an important section, and on the character and condition of society in the state whose medical association he is addressing, he passes on to compare the present position of the profession in public estimation with that which it held in past times, and is led by this comparison to utter grave and significant warnings, to which, as the counsels of a sober and sagacious friend, we shall do well to give heed.

"We are not to conceal from ourselves the fact that this position is different from that which we once held. Formerly, though often the subject of ridicule and satire, medicine was looked upon by the mass of mankind with a veneration almost superstitious, as it is still among savage nations. In times long since past, there was supposed to be something recondite, mysterious, far above the apprehension of the vulgar, in the knowledge of physicians. The oracular air and the dictatorial authority which they assumed, were submitted to as rightfully belonging to those who possessed secrets of nature and art of an almost supernatural character. And, more recently, although the excess of this feeling has passed away, there still remained a *prestige* around the profession, which gave its members a sort of authority over the minds of men in their peculiar vocation, resembling that possessed by ecclesiastics at the confessional. But this has nearly ceased. Indiscriminate reliance on authority no longer exists. To assume it would expose us to derision. The confidence of mankind, as a mass, in the regular profession, has changed its character, and has probably much diminished. So far as it remains, it depends more on personal regard, and reliance on the individual, than upon any general high appreciation of medical knowledge. How ready are people of all classes to trust themselves to men of imperfect education, if not of equivocal character! How ready are those even of good education and intelligence, to set themselves up as final judges upon questions with respect to which men of the largest capacities, trained from youth to the study of such subjects, and grown gray in watching disease, find it very difficult to get at the truth!

"So much is this the case, that some among us have at times entertained fears with regard to the stability of our profession as at present constituted; and, rendered timid by the signs of the times, have seriously apprehended that we are to be sooner or later supplanted by some new medical dynasty, if I may so call it. At the same time, many out of the profession, the proselytes of some recent sect, have almost exultingly prophesied, that at no very distant period the new system, to which they have given their adhesion, will establish itself upon the ruins of the old.

"But, while I have not the slightest apprehension of this result, and, on the contrary, entertain the most unlimited confidence in our stability and permanence, yet, since it is impossible not to admit the reality of the change to which I have adverted, we are, I think, imperatively called upon to consider seriously our present condition, and to call up for reflection the principles of conduct, which will contribute most certainly to establish the profession in future upon an honorable and enduring foundation." (p. 13.)

Dr. Ware then proceeds to point out that the present age being eminently distinguished by its progressive tendency, there is a rapidly decreasing tendency to reliance upon prescription and authority. Everything is presumed to be capable of improvement; and, in consequence, everything is made subject to careful scrutiny, its foundations are examined, its truth is tested; and if in any subject there exists, or there is fancied to exist, on the part of those connected with it, a disposition to oppose the operation of the principle of progress,—an inclination to cling too closely to the past,—there will result, as a natural consequence, distrust and want of confidence. It is not surprising that there should be a special

tendency of this kind, in regard to the public estimation of the state of medical knowledge; since the amount of discordance amongst the authorities of any one time,—still more, the complete opposition between the principles and modes of practice prevalent in different ages,—together with that uncertainty of much of our knowledge as to the nature, causes, and treatment of disease, which the best informed among us are the most ready to admit,—may not unreasonably impress the inquirer with the conviction that there can be no truth and no certain knowledge, where there are so much disagreement and so much doubt upon what seem to be essentials. It is not strange, then, while this is found to be the case among those who were once looked up to as oracular and infallible, that distrust should creep in, and that the actual amount of certain knowledge should come to be undervalued.

We quite agree with Dr. Ware in the conviction that "Medicine will bear a comparison in its history, and in its present condition, not only as to the spirit of truth and humanity with which it has been pursued, but also as to the results which have been attained, with any branch of human inquiry, when the difficulties which surround its investigations, and the circumstances under which the comparison is made, are taken into consideration." There is much truth, too, in the following argument; especially if we adopt Dr. Ware's very comprehensive definition of medicine:

"In comparing medicine with other sciences, it seems always taken for granted that it presents a field about as extensive as each of the others with which it is compared. But this is a most erroneous view. Medicine embraces the whole science of living bodies. The phenomena of living bodies are much more various than those of inorganic bodies,—and so are the laws according to which they take place. The collection of the physical sciences, then, which concern inorganic matter, are *all together* to be placed in comparison with the science which relates to the laws of living matter. The chemistry of life, for example, is as extensive and complicated a subject as the chemistry of the inorganic world; and so of the infinite motions which take place in a living body, and of the infinite relations in which they are maintained by means of the nervous system,—they are more numerous, more obscure, more complicated, than those existing among the inorganic bodies, which are the subjects of natural philosophy and astronomy. Now when we compare the amount of labour which has been bestowed on mechanics, or astronomy, or chemistry, individually, with that which has been bestowed on medicine individually, these sciences are found in advance. But compare the *whole* of the labour which has fallen to the lot of astronomy, mathematics, chemistry, natural philosophy, geography, and geology, on the one hand, with that which has been bestowed on medicine as a science, on the other, and the progress it has made is rather a subject for pride than for humiliation." (p. 17.)

Whatever estimate we form of our own merits, however, we must not expect society at large to view them in the same light; until some established system of medical philosophy can be presented, which shall have, in its definiteness, its completeness, and in the universality of its applications, the same kind of claim to general assent as that which is conceded on all hands to the Newtonian physics. The present is a sort of transition-period in our history. The old claims of authority,—which have held by us, and by which we have ourselves held, much longer than many of us are aware of,—are rapidly losing their influence; and far be it from us to attempt to revive them. But the profession has not yet

established, with the progressive portion of the public, that relation which ought to subsist between the two. There are vast numbers among the latter, possessed of sufficient clearness of head to discern much that is prejudiced and unphilosophical in the proceedings of the "regulars," who are led astray by the desire of novelty to yield to the artful seductions of a more specious but really nonsensical quackery. And we continually see, in the train of the most absurd empiricism, individuals whose judgment we should highly value on any of the ordinary concerns of life, and whose example is peculiarly calculated to influence others who seek a justification for the transfer of their allegiance. Looking to the vast importance, in such an age of convulsive upheaval and restless movement, of securing the firmest possible stand-point for the exertion of our influence when the existing ferment shall have in some degree subsided, and the constitution of society shall be settling down into something like a permanent condition, it surely becomes us to inquire carefully and fearlessly what there has been in our professional character to weaken our old influence over the public, and in what manner a new and more solid appreciation of our worth may be best created in the minds of those, whose good opinion it is our interest, as it should be our unselfish happiness, to obtain. We are certain that, in the following remarks on this point, Dr. Ware has (to use a somewhat undignified but very apposite expression,) hit the right nail on the head.

"The following consideration, I cannot but think, has much weight, in enabling us to judge of the probable future destiny of our profession; namely, that whatever may be the currency of particular opinions, or the reputation of particular bodies of practitioners, the public will confide, habitually and mainly, in that body, or that succession, of men, who show themselves to be devoted to medicine, not merely as a means of getting a livelihood, or even as a means of treating disease and relieving suffering by the common routine of practice; but who pursue it as a great subject, all the relations and bearings of which it is their duty to investigate; who regard it as a science which they are deputed to build up and perfect; and who do all this as diligent, earnest, and disinterested inquirers after truth. It is this class of men, who, when they are understood, will receive the permanent confidence of mankind; and such, I undertake to assert, has been, and is, the essential character of our profession." (p. 20.)

In justification of this assertion, Dr. Ware points to the services rendered by the profession, not merely to the sciences purely medical, but to botany and zoology, to comparative anatomy and chemistry, to mineralogy and geology. "Strip these sciences of what has been contributed to them by physicians, or by those who have had the discipline of a medical education, and a chasm is left, which it would be difficult to fill." And in those more directly practical labours which contribute to the alleviation of human suffering, where are more earnest or more faithful ministers of Providence to be found, than those whom no toil or danger restrains from attending to the simple call of professional duty, alike in the hovel as in the palace, in the squalid habitation of penury as in the abode in which wealth contributes its utmost to mitigate the discomforts of the sufferer? We have a claim, too, on the confidence of our fellow-men (as Dr. Ware justly remarks), not only for what has been done, but for the spirit in which it has been done.

"These services of the medical profession, upon the whole, have been among

the most disinterested ever rendered to mankind. They have been distinctly governed by a desire of acquiring that knowledge which will confer practical benefits on society, without reference to the amount of reward, or to any fame except that of doing good. There is no better proof of this than the fact already implied, that the aim of those who are most prominent in medical history, has been more to investigate the laws of disease, and thus to prevent it, and raise the standard of health, than to acquire reputation or wealth by what is far easier and more lucrative,—attention to the ordinary details of a medical practice. And, if it were necessary to introduce a more striking example of what is to be regarded as the governing spirit of those who are the true index of professional character, we have but to name the discovery and gift of vaccination to mankind by the illustrious Jenner." (p. 23.)

We should be most glad to believe that such instances have their legitimate influence on the character of the profession at large, and their due weight in public estimation. But we fear that they must be considered as the exception rather than the rule; and that they are too often theoretically acknowledged to be examples deserving of respect and imitation, instead of being practically made so. The stoutest upholders of the dignity of the profession must freely concede that it includes a large class, who merely regard it as a means of earning their daily bread, or of acquiring property and consideration.

"To them, the practice of medicine is like the practice of any other occupation, selected and followed almost mechanically. Hence, there are always to be found physicians of sordid minds and purely selfish views, who are yet high in professional rank and emolument. Such men, naturally enough, but unhappily for our good name, have often been the most prominent to the public eye, and have been the chief recipients of favour and patronage, just as it has happened in all other departments; and they have been thus sometimes taken as exponents of the character of the whole profession. But its true representatives are they, to whose lives and labours I have just referred; and from these men the treasures of knowledge, which they accumulated in years of faithful and unceasing labour, have descended to us as our rich inheritance; possessed of which we should feel safe as to our future destiny, whatever may be the accidental and temporary alienation of portions of that public whom we serve, and whose confidence is so necessary for our usefulness. If we are faithful to the true character of our profession; if we go forward with honesty and fidelity in the path of our predecessors, governed by the same desire of knowledge and of usefulness, we need not fear but that the present movement of opinion will be transient, and that our position will become more durable than before." (p. 24.)

But it will be only by keeping pace with the advancing requirements of the age, that we can hope to maintain our ground; and only by anticipating them, that we can hope to improve our position. The adoption of an improved and improving standard of attainment for those who enter the profession, will do much for the coming generation, and must necessarily exert a favorable reaction on the present. To this subject we shall direct the attention of our readers in the latter part of the present article; but shall now follow Dr. Ware in his examination of the means by which the public estimation of the existing body of professional men may be raised. It is in vain to expect that this can be accomplished by any plan of medical reform, or any alteration in the existing constitution of our governing bodies; by anything, in short, but by the habits and conduct of the *individuals* of which the professional aggregate is composed. As reasonable would it be to expect that an Act of Parliament

can bring up the social condition of Ireland to a level with that of England, without the enlightenment and self-discipline of its people, as to anticipate any elevation in the *status* of the medical profession without a well-directed and combined upward movement of our own. What should be our standards and examples in such an effort, Dr. Ware has already shown us. There is no lack of noble names, honoured alike by the man of science and the philanthropist, whose place in the history of our race and in the world's esteem, even if too high for our own ambition, should excite our desire to tread at an humble distance in their footsteps; satisfied if we can thus contribute, however feebly, to redeem the general character of the profession from the distrust with which it is regarded by too large a section of the public, and to cause such names to be estimated,—not as those of individuals who have distinguished themselves by their philanthropy amongst the selfish, by their knowledge among the ignorant, by their philosophy amongst the irrational,—but as those of "bright particular stars," that have shone out with superior lustre from a firmament illuminated by the diffused radiance of crowds of less brilliant luminaries. That such may be the case, we must, each for himself, adopt in good earnest the apostolic precept; "forgetting those things that are behind" (not of course literally, but thinking nought of them as of progress made), and not pausing in the race "as though we had already attained, either were already perfect," we must "reach unto those things that are before," pressing on toward the mark for that prize of our high calling, which consists not in wealth acquired or dignity obtained, but in the happy consciousness of duty discharged, in the earnest gratitude of the sufferer restored by our means to ease and vigour, in the blessing of him that was ready to perish but for our intervention.

But as we cannot exercise our benign vocation without the confidence of the public, we are urged alike by philanthropy and by self-interest to adopt all legitimate means of securing and increasing this; and nothing is so likely to promote the object we have in view, as a readiness to examine, with a fair and candid spirit, "*all and any suggestions, however they may arise, and from whatsoever quarter they may come,*" which hold out a fair prospect of increasing our means of professional usefulness. On this head Dr. Ware speaks out strongly and boldly; but not, in our opinion, one whit too strongly; for we are convinced that the fault to which he alludes is at least as prevalent on this side of the Atlantic as in the American Union, and that it has done more than anything else to lower the character of the profession with the public, and to drive some of the most intelligent of the latter into the ranks of our opponents.

"I must be permitted to say, that the character of our profession has often suffered in the opinion of mankind, and confidence in the real value of our knowledge has been lessened, by the spirit which has been frequently exhibited on this subject. There has sometimes been a want of liberality, a reluctance which must be called narrow-minded, not merely to admit, but even to look at the evidence of any new truths, and especially new modes of treatment, which present themselves out of the beaten track of medical observation. It has even gone farther than this; and the same disposition has shown itself with regard to new things which have come up in the profession itself, especially if there happened to exist any of those petty jealousies, growing out of individual or local rivalry, which we so often suffer to blind and mislead us.

"Now this disposition arises out of a species of caution, good in itself and

useful, growing out of a large experience of the fact, that, of new things, a great proportion are false, and that the evidence upon which most men rely as sufficient for proof, especially on medical subjects, is totally inadequate. But still, new things are sometimes true, and we can only select these by an examination of all. And even were we capable of determining, as we sometimes are *a priori* without examination, that certain things are false, we cannot place mankind in that position for observation which we occupy, we cannot impart to them those elements for judgment which we possess. We expect them to take our opinions on authority; but, in order to secure submission to authority, we must satisfy them that we are candid inquirers and impartial judges; we must be free from the suspicion of professional prejudice or jealousy. In order, then, to lead the opinion of the public in the matters which concern our occupation, we must be content, not merely to examine the evidence for the things which *we* think offer a fair show of probability, but for those which *they* think offer such a show. What may seem very unlikely to us, may seem very likely to them. What we even *know* to be impossible, they may be very ready to believe; and this, even among those who in their own pursuits are intelligent and well-informed. To secure their reliance on our opinions, we must, in making up a decision, put ourselves in their place, instead of expecting them to put themselves in ours. We must allow much for the circumstances under which they judge; much for their prejudices. Let us be sure that we do not yield to our own. We must be content to sift the chaff for their satisfaction, even when we feel confident that there is not amongst it even a single grain of wheat to reward our search." (p. 32.)

The past history of medicine is fertile in examples of this disposition. The discovery of the circulation of the blood, inoculation, vaccination, and auscultation have all been opposed or undervalued at first by the most aristocratic and influential section of the profession; and their admission into the circle of established doctrine and practice has been due, not so much to a spirit of inquiry and an openness to conviction on the part of the general body of our seniors, as to the zeal of a few, generally among the younger aspirants for professional distinction, who had sufficient sagacity to perceive their value, and sufficient perseverance to force the recognition of it upon others. The *final* judgment, we are inclined to believe, has been almost invariably right; but we are not sure whether, in more than one instance, this would have been the case, had not the pressure from without urged forwards an inquiry which would otherwise have been avoided; the public having unequivocally manifested their expectation that every new system, which has a *primâ facie* case in its favour, should be examined and investigated by those who claim to themselves the regulation of professional opinion. We have no desire to see a tendency to that too-ready credulity, which goes to the opposite extreme of seizing and appropriating every novelty that is suggested; on the contrary, we think with Dr. Ware, that it would be difficult to say whether this, or the incredulity which decidedly rejects every novelty at once, without examination or inquiry, would be in the end most dangerous to medicine. But there is no occasion to brand, as the offspring either of folly or of imposture, systems or doctrines from which we think it right to withhold our assent; nor, whilst refusing to recognise them *in toto*, should we blind ourselves to the elements of good which they may contain. We so thoroughly accord with Dr. Ware's remarks on this part of our subject, and think them entitled, for the reasons we have already stated, to so much attention, that we shall lay them before our readers as fully as our limits permit; and we trust that the source from which they proceed

may give them a weight which they might not possess if they were simply put forth as an expression of our own opinions.

"The relation which we maintain towards systems which we believe to be false, should be determined by a principle which is most important in its bearing; namely, that there is no form of error, from the careful observation of whose origin and progress important inferences may not be drawn for the establishment of truth. Every kind of error has of course some truth, to which it is, as it were, the negative pole; the knowledge of one is necessary to the knowledge of the other. This is as applicable to medicine as to any other subject; perhaps more so."

Dr. Ware then applies this view to one of the systems most in vogue at the present day, namely, Homœopathy; of which, as a theory, he justly remarks that it contains nothing more at variance with sound medical knowledge than may be found in the doctrines of Boerhaave, or Cullen, or Brown; whilst to many of its supporters he gives the credit of having formed their belief with perfect honesty, and after what they regard as a fair comparison. The virtues attributed to infinitesimal doses, absurd as they may seem to us, are not found by experience to be more opposed to the belief of intelligent and truth-seeking men, than the belief in spells and amulets cherished by Lord Bacon, or in the remedial value of the hand of a man who had been gibbeted and of the heart of a mule which had been ripped open alive, entertained by Sir Thomas Mayerne, the physician of three kings. It is the general belief, however, of the mass of our profession, including nearly every one who has gained any reputation as a man of scientific acquirements, philosophic habits of thought, or practical skill, that this method of treatment is entirely negative; and that patients thus treated are practically left, so far as medicines are concerned, to the resources of nature. But are we, then, whilst opposing ourselves to Homœopathy as a system, to derive no benefit from the great experiment which the public appreciation of it is carrying on under our observation? Hear Dr. Ware:

"This being the case, it affords us a means of observing, on a large scale, the natural history of disease, as it goes through its course uninfluenced by the interference of art. The want of such an opportunity has been one of the greatest obstacles to the advancement of the practical department of our profession. In its elementary branches, our science has improved, slowly, perhaps, but with more uncertain steps; whilst in therapeutics it has constantly struggled with the want of some standard of comparison. How can we judge what is the efficacy of any given method of treatment, unless we know what course disease will take without treatment? The want of this knowledge—the knowledge of the natural history of disease—has been the cause of almost all the uncertainty, the opposition, the vacillation in the management of disease.

"This want it has been extremely difficult to supply in the ordinary course of practice. Approaches have been made to it with regard to a few diseases, and on a small scale; but, in order to supply it thoroughly, it needs a long experiment on a large scale. Now, believing as we do in a greater or less amount of efficacy from our method of treatment, it has been impossible for us conscientiously to institute such an experiment; and the world would not support us in doing it, if we were disposed. But the homœopathists are performing, as we believe, this very experiment; and, fortunately, such is their confidence in their system, that they do it boldly, and can keep up, in this way, that reliance of their patients on their remedies, which is necessary to the success of any treatment. It is only to be regretted that this same undiscriminating confidence on their part prevents the experiment from having its full value; since those who conduct it, and therefore might best observe it, do not scrutinise its results as philosophers, but merely gather them up as partisans. It is, nevertheless, our duty, without any feeling of rivalry or ill-will,

to watch its progress as best we may, to avail ourselves of the fruits of what we regard as the errors of others,—as we ought to do of our own,—to enrich ourselves, as it were, with their spoils.

"In a kindred spirit should we keep up a constant observation of the various attempts which are constantly making, sometimes by individuals and sometimes by bodies of men, to devise new methods of treating disease; and with a similar purpose of appropriating whatever may present itself which promises advantage. Everything which happens to those labouring under disease,—everything which is made in any way to act upon them,—is capable, when accurately observed, of illustrating the laws of life, or of disease, or those according to which recovery from disease takes place. The more varied are the conditions under which the observations are made, the more rich will be the results; and, considered in this point of view, there is no mode of treating disease, however regular, and in hands however ignorant, no delusion in regard to remedies, however strange, from such an observation of which useful knowledge may not be derived, either with regard to the history and treatment of disease, or, at least, with regard to the influence of imagination and prejudice upon its character and progress. All are experiments, which, though performed for no such purpose, should really be made to operate for our benefit. We are to consider that we cannot make them ourselves. With our knowledge of the accurate relations of disease, of the uncertainty of remedies, of the great powers of nature, and with that delicate responsibility to our patients, which imposes upon us the observation of the great first law in therapeutics, '*to do no hurt,*' we cannot run the sort of risk which such experiments imply; they require a recklessness of the result to which we cannot consent. When others choose to incur the responsibility of making them, it is right that we should reap the fruits.

"In this way, I am convinced, we may, from time to time, learn much to aid us on the difficult subject of therapeutics. We may find that some modes of treatment which have been highly prized, have less connexion with recovery from disease than has been usually imagined; and, on the other hand, that certain other measures may have been undervalued and overlooked. But, if this inquiry be always made in the calm and rational frame which should characterise the medical observer,—free from that turbulent spirit of animosity towards rival sects which we are too prone to indulge,—looking neither to the right nor to the left, to discover what the effect will be on our profession considered as a trade,—we shall be constantly adding to our stock of practical truth." (pp. 37-40.)

We have a firm faith that such will ultimately be the case with regard to all the systems of quackery (as we are accustomed to term them) on which the attention of the public is at present fixed; for experience teaches that such *has been* the course of all innovations upon the received doctrines of medicine. The truth, however doubted and ridiculed, will maintain its place, and will come out purer and brighter from the ordeal to which it is subjected; whilst errors, however tenaciously clung to, will be progressively discarded, not without leaving behind them some materials from which lessons of value may be educed. But we are desirous of hastening, so far as may be, this desirable consummation. We would have the decisions of the profession pronounced, not feebly and hesitatingly, nor yet hastily and dogmatically; but with the sagacity resulting from adequate mental training, with the impartiality produced by a complete dismissal of selfish interests, and with the earnest desire to comprehend the whole truth of the case, which will necessarily result from a due appreciation of the dignity of the inquiry and of the vast interests involved in it. In the present state of medical science, we feel well assured that the only true system is the absence of all system. No premature attempt to generalise

can have more than a temporary success. Be it ours to seek for light wherever it shall break in; to amass the treasures of knowledge, even if we have to pick them up from the mire; to draw wisdom from the errors and follies of our rivals, without disdaining to profit by their success. And then, as Dr. Ware beautifully remarks, "other systems will pass away,—ours will be permanent; nourished, indeed, to some extent by the very elements which come from their decay, as the eternal oak flourishes and grows green for ages from the decomposition of the transient vegetation, of which generations are springing up and perishing around it."

We shall presently return to Dr. Ware's Discourses, when discussing the subject of medical education; but we shall first offer a few additional remarks on the mental habits which it becomes those to cultivate, who desire to contribute to the advancement of professional knowledge, whilst actively engaged in practice.—Dr. Shapter aims to dispossess the minds of those to whom he addresses himself, of certain prevalent fallacies, which have proved very detrimental to the true interests of medical science: of these the chief are, "that medicine is a conjectural art," and "that it is dependent for its truths solely on observation without the aid of induction." Those who affirm the first of these propositions do in effect state, that the best practitioner is he who guesses best. But the whole process by which an enlightened practitioner directs his treatment,—the examination of the symptoms, the comparison of the state of disease with that of health, the determination of the nature of the disordered condition, and of the probable course it will follow, together with the selection of the remedies indicated,—implies knowledge guided by reason and science; while conjecture implies ignorance, its suggestions having no better foundation than is afforded by that blind faith which begins where reason ends. All legitimate medicine is a practical art, arising out of the two sciences of pathology and therapeutics; and in proportion as these sciences are perfected, will the rules of the art founded upon them be simplified and rendered definite and certain. Doubtless, says Dr. Shapter, many who designate medicine by the term conjectural, if they were to analyse their own thoughts and views, would find that these really are in accordance with the strict definitions of art, which cannot in its very nature be conjectural, but must be founded on fixed principles; and the term is merely used by them to indicate that many of its truths have not been attained.

"It is, however, of the utmost importance to establish *clearly* to the minds of all, that, inasmuch as disease is the consequence of natural causes, so must its alleviation be guided by natural principles, and consequently that the art of medicine is only true and certain as it is in accordance with these principles. This being ascertained, the opinion can no longer prevail (which assuredly it might, and justly so, if conjecture were our only guide) that the accumulation of facts is useless, and the generalizations of reason thrown away. Viewed as a certain art, we possess the assurance that our labours are not in vain, and that though from our blindness we may not always perceive truth, yet we are surrounded by it, and that, by seeking it, it may be found; for mental eyesight is like that of the body, its cultivation makes evident that which otherwise is confusion and darkness." (p. 14.)

Dr. Shapter then addresses himself to the second fallacy, that "the art of medicine is dependent solely and exclusively on experience."

"So strongly does this fallacy prevail, that we find the art thus appreciated not only by the general public, but likewise by a numerous class of the members of the

profession,—so much so, that the boast is not unfrequent of being a 'practical man,' meaning thereby a special regard for experience, whether general or personal, to the exclusion of all inductive philosophy. In the vulgar appropriation of this term, its definition is, however, still further narrowed, and the 'practical man' is he who, adopting personal experience, excludes that which is general.

"Those holding this view are ever boastful of their 'facts.' 'Fact' is their watchword. The sentiment, *quod vidi credo,* if not often expressed, is, at any rate, freely applied,—meaning thereby a belief in all that the senses, uncorrected by reason or philosophy, convey to the mind, and a disbelief in all that is not so presented to them. An examination of this position will undoubtedly show that such a dogma is not only diametrically opposite to the principles on which the sources of truth are founded, but leads to a belief in much that is not true, and to a disbelief in much that is." (p. 14.)

A very little scrutiny into most of our so-called "facts" is sufficient to prove that they are in reality generalizations founded upon the colligation of a number of individual instances, and that their true rank is therefore that of "theories." Thus the simple ultimate truth left to us by Sir George Baker, that the Devonshire colic is produced by the absorption of lead into the system, was deduced by him from the combined consideration of the following separate classes of phenomena, each of them including a distinct group of individual instances. In the first place, he found that those who were attacked by this disease, were cider drinkers; secondly, that the cider was then very generally made in leaden presses; thirdly, that these presses were rapidly dissolved by the action of the cider; fourthly, that the cider contained a very appreciable amount of lead; and fifthly, that the symptoms of the disease were the same with those of the colic known to be produced by the absorption of lead. The inference in question, simple and evident as we now deem it, was really deduced from an extensive survey of phenomena, by a regular process of philosophical comparison and induction. And the same is true of almost every one of those facts on which the most thoroughly "practical" man is accustomed to place his firmest reliance. Any conclusion which falls short of the certainty of such an induction, is merely hypothesis or coincidence; nevertheless nothing is more common, as Dr. Shapter truly remarks, than to hear quoted as a recognised fact, the crudest hypothesis, and as established truth, the most naked *non sequitur;* and this by those very "practical" men, who profess to discard all theory, and to be guided solely by experience,—as if the application of the experience of one case to the treatment of another were not in itself an acknowledgment of that belief in the uniformity of nature, which is the basis of all scientific inquiry. Let us follow Dr. Shapter in his exposure of one of the most common of the fallacies of the class to which he is referring.

"It is often stated that a given disease is cured by the administration of a particular medicine. Here four facts are implied: 1st, the fact of a disease; 2dly, the administration of a particular medicine; 3dly, the cure of the disease; and 4thly, upon these three facts are colligated another, viz., that the third is dependent on the second. For the sake of argument, we will allow the first three positions, and we may then examine as to the fourth, and we shall perchance find, that this so frequent fact is resolvable into mere coincidence or hypothesis; for example, when it has simply occurred that there was a disease, a medicine was given, and then a cure of the disease; now here the third is at once seen to be merely a concurrent term, and may be entirely independent of the second; therefore from such data it may be erroneous to conclude that the cure was dependent on the adminis-

tration of the medicine. Hence it is not a fact, but merely a coincidence. Again, the medicine has been given in many cases of disease, and in all recovery ensued; but inasmuch as it is not shown that in other cases in which the medicine was not given, recovery did not ensue, it cannot be stated as a fact, that the medicine cured the disease. It certainly amounts, however, to more than a coincidence, it is an hypothesis. Again, cure ensued in all the cases of disease in which the medicine was given, and in no case in which it was not given; here there is so conclusive a chain of circumstantial evidence, that it passes beyond hypothesis, and if worked out by induction, assumes the position of a fact." (p. 17.)

It is obvious, then, that something more than observation and experience are necessary for the establishment of even those simple facts of medicine, which the "practical" man cherishes as his most valuable truths; and that nothing but a correct apprehension of the process by which they are attained, and such an amount of logical training as shall enable him to distinguish true from false reasoning, can save him from blind reliance upon a set of assertions to which not the slightest value can be legitimately attached. And if this be conceded in regard to those truths of limited generality, upon which our higher and more comprehensive principles are based, much more must it be admitted with regard to the latter; since it is their function, not merely to express what *is* in all known cases, but also to predict what *will be* in cases that may hereafter arise. For it is a well-established principle in philosophy, that no amount of mere experience can establish a necessary or universal fact.

"A proposition may have been found to have been true in any given number of instances,—it does not, however, follow that the next instance which shall occur may not be an exception to this rule. Thus, five hundred cases of fever may not have died, the next case may; so that the experience of the five hundred cases does not show that any subsequent one may not prove fatal. Moreover, experience must be limited, and can bear no proportion to the cases in which it has not been made; and, therefore, however similar and circumstantial it may have been in the instances observed, it cannot affirm its propositions to be necessary. For example, we know by experience that ague may be produced by residing in a marshy country; but we do not thereby know that it will necessarily be so produced; here experience entirely fails us, and notwithstanding the number of cases of ague that have been observed, nothing but an hypothesis is eliminated." (p. 19.)

This discrimination between propositions that are necessary, and those extracted solely by experience, is one of vital importance in our scientific inquiries. It is from the neglect of it that an undue importance has become attached to the "numerical method," which, as Dr. Shapter justly remarks, however valuable it may be, as enabling us to affirm *positively* with regard to the proportion of instances that which was previously only stated generally, is entirely wanting in a main element necessary to evolve the higher truths of the science of medicine; its own numerical statement being, in fact, the only thing which it establishes. The application of the numerical method to purposes for which it is altogether unfitted, has further tended to confirm that previously too exclusive attention to symptoms, without investigating and determining their origin or cause, which has constituted on the part of many, the sole aim and process of inquiry.

"So largely and extensively has this error prevailed, that in its pursuit much valuable labour has been uselessly expended in vain repetitions of the same observations. Hence, while huge treatises have been written on many of the prevailing and most interesting diseases, nothing beyond the first hasty description is really

known of them. Within this category may be included the Asiatic cholera, the influenza, &c.

"Another error is the extravagant appreciation of the revelations of morbid anatomy. There are those who, largely falling into this error, have recognised in the appearances after death, the cause, not only of the death itself, but of all preceding disorder; while, probably, the preceding disorder has been the cause of the appearances thus laid bare. The longest established and most serious illustration of this, is the too familiar one of consumption; the extreme condition of the lung has absorbed attention, to the exclusion of the constitutional disorder which has preceded it.

"Another, and not unfrequent error of pathological observation, has been to consider as belonging and essential to the history of a disease, all the lesions which may have been observed in all the various and differing cases of such disease as may have occurred; thus, that which is only concomitant is mixed up with, or even mistaken for, that which is essential, and consequently is assumed to be characteristic while it is only accidental; thus the pathological condition of a fever has been made to comprise almost every lesion to which the human frame is liable, instead of excluding all lesions but such as were universally or generally observed." (p. 22.)

Thus the study of medicine is beset with various fallacies and errors, tending to clothe it with an uncertainty which does not inherently belong to it. "This great principle may, however, be relied upon, that in the art of medicine there is a right and a wrong way;—this, to be avoided, as rendering it useless, or enduing it with danger;—that, to be embraced, as resolving it into a real and certain utility." It is of paramount importance, then to determine what is the right and what the wrong method of pursuing medical study; and we believe that we speak the opinion of those best informed upon the subject, when we say that the right way is to be found in cautious induction, and sparing use of hypothesis except for the sake of directing inquiry; whilst the wrong way lies in hasty generalization and too confident theorising. All are now agreed as to the importance of a primary foundation of *facts;* it is in the *use made* of these facts, that we trace the difference between the man who contributes towards the real progress of medicine, and him who, whilst professing and even himself believing that he is aiding in the same good work, is really bringing about a serious retardation in the erection of the edifice, building up that which will require shortly to be pulled down at the expense of labour and delay. And when we consider how large a proportion of the medical novelties of the day fall into the latter category, we cannot but deeply regret the low state of the logical faculty in the great mass of the profession. Many, we believe, have no other notion of induction, than the process of heaping up facts, and of forming some general expression of them, approaching as nearly as possible to numerical precision. This is not induction, but empirical generalization; and though such expressions may chance to be scientifically accurate, they do not take the rank of laws until they combine the "idea" which lies at the basis of all the phenomena, with the general expression of the phenomena themselves. Thus when Kepler developed the relation between the times of the planetary revolutions and the distances of these bodies from the sun, he merely stated a fact common to all the planets with which he was acquainted; and there was no certainty that other planets then known would hold to the same formula, until Newton showed that the coincidence was the necessary result of the operation of those grand, simple, and

universal principles embodied in his laws of gravitation and motion. We have lately had an instance of the failure of an empirical formula of much the same kind. A certain relation was pointed out by Bode between the distances of the successive planets, which led to the anticipation of a planet between Mars and Jupiter, that was verified by the discovery of the asteroids: and MM. Adams and Leverrier were in part guided by the same formula in those investigations which led to the discovery of the planet Neptune. Yet it now appears that the distance of Neptune from the sun is really so much less than was reckoned on, that the formula cannot be any longer trusted to; being a mere numerical representation of certain facts, and altogether destitute of even an approximation to the "idea" that colligates them. It would be easy to adduce examples of the same kind from the history of medicine; but it must for the present suffice us to observe that the "numerical method" is a typical example of the process of empirical generalization; whilst the "cell-theory" may serve as a "pregnant instance" of a real inductive process, involving not merely a comprehensive expression of facts, but a fundamental "idea," of which the phenomena are (so to speak) the external signs or exponents. It is by the possession of that mental insight which enables them to discern the idea really colligating the facts, that those men have been distinguished, who have done the most for the advancement of medical science; and those who are not gifted with it would be discharging a much more useful and honorable office, in patiently collecting materials which the skilful architect may combine and arrange according to his idea of their appropriate relations, than in attempting to build up some little fragmentary construction of his own, which will need to be pulled down ere the complete design can be carried out.

We are glad to find Dr. Shapter as hopeful as ourselves, with regard to the benefit that would accrue to medical science and art from a more logical method of study.

"May be some, who now hear me, may think this Utopian; they may probably deem it next to impossible to apply principles so accurate to so complicated a series of ideas as are originated by the various phenomena of disease; that from amidst them it would be vain to look for general, much less for universal truths. Has not the same been thought and said in the infancy of all science? How much mysterious ignorance was entertained before the simple truth that the earth moves round the sun, was ascertained; and now that this is generally known, how little can we participate in the difficulties that occurred before it was proved? Its now apparent simplicity only creates wonder that it should ever have been doubted. But amongst all the difficulties attendant on the art of medicine, are we without truths? Most assuredly not; some simple truths have been ascertained; and they are beautiful and conclusive examples of the success of an application of the inductive process of reasoning." (p. 26.)

Dr. Shapter cites the various steps of progress made in the pathology of renal disease, as good illustrations of his argument; and we may especially notice the simple truth, now so familiar to us,—that the presence of an undue amount of urea in the circulating blood acts as a poisonous agent on the nervous system,—as one which no mere generalization of similar phenomena could have evolved; the facts on which it is based, and to which it is universally applicable, having apparently little or no relation to each other, and needing the colligation of the "idea."

We here take our leave of Dr. Shapter, cordially thanking him for the assistance he has given us, and of which we have gladly and freely availed ourselves, in setting before the medical profession the imperfections of its present intellectual system. Mr. Simon's Inaugural Address next comes before us; and we shall find that the views set forth in it are essentially the same. We must first stop to notice it, however, as a favorable sign of the times, that the governors of St. Thomas's Hospital should have established in their school a chair of surgical pathology in connexion with clinical instruction; and we congratulate them on having secured the services of a gentleman so well qualified by powers of observation and of reasoning, as well as by zeal and industry, for the discharge of its duties. Let us hint to him, however, that a simpler style will better become his subject and his position; grandiloquence is generally misplaced in a medical teacher; and a mode of expression which it is bearable, or even pleasurable, to listen to in a senior who has earned the right to soar high by the full development and vigorous use of his wings, becomes something akin to ridiculous in an aspirant who is making his first attempts at an independent flight. We have noticed the tendency to which we advert in Mr. Simon's earlier writings; but it is here so much exaggerated, in accordance, we presume, with the author's sense of the dignity of the occasion, as to call for this friendly warning. The passages we shall select are not those which most strikingly exhibit this fault, but those whose genuine philosophy and sound sense recommend them as most appropriate to our purpose.

Mr. Simon, like Dr. Shapter, is desirous of impressing upon the minds of those whom he addresses, that whilst observation and experiment are the basis of all scientific knowledge, the restriction of the attention to these, and to the subordinate generalizing processes that instinctively connect themselves with them, is inconsistent with those higher operations of the intellect by which latent causes are brought to light, and the secret springs of natural phenomena discovered. He then treats of the present condition of pathological inquiry under the three following heads:—1, Interpretation of phenomena;—2, Doctrine of Causes;—3, Principles of Classification.

In discussing the first of these subjects, Mr. Simon justly remarks that the interpretation of the phenomena of disease must found itself upon the analogies of the healthy condition; the pathologist being constantly obliged to translate the phenomena into the language of physiology, and to express them in terms denoting their similitude or contrast to healthy processes. That a sound pathology must be based on a sound physiology, seems a proposition almost self-evident; and yet the necessity is continually disregarded in practice, to the great detriment of science. For it is obviously necessary that there should be a standard of comparison in regard to action as well as to structure; for only by observing the former can we really understand the latter, many a structural change of which the morbid anatomist takes cognisance, being really the result of a process of growth in itself taking place according to normal or physiological laws.

"The simplest illustration I can give of this, is in the common use of the words 'hypertrophy' and 'atrophy,' expressing disease as mere *more* or *less* of the *natural process of growth*. Yet, even here, mark the use of the physiological formula;—

when we speak of a big heart as hypertrophied in respect of its muscular substance, we are led directly to the cause; we know physiologically that a muscle grows in proportion to the stimulus of exercise; and in looking to the valves of the diseased heart, we find, in their defective condition, sufficient evidence that the heart must have done double work,—sufficient explanation of its muscular hypertrophy. Similarly, when we speak of the characteristic flux of diabetes as an essentially normal solution *plus* sugar and its consequences;—when we describe an encysted tumour of the scalp as an accumulated sebaceous secretion;—when we say of an enlarged liver, that it is hypertrophied simply in respect of the fat of its endothelium;—when we speak of tetanus as a functional excitement of the spinal cord; we adopt the method and phrases of analogical interpretation. And in doing so, we express the several diseases in a form which, to the physiologist, half intimates their origin, or at least suggests the direction in which their causes may be sought for." (Inaugural Address, p. 14.)

Mr. Simon gives an appropriate warning, not merely against wrong methods of analogical interpretation, but against the application of right methods to imperfect data;—we must "give credit to the pathological theory, only in proportion as its physiological prototype is complete and trustworthy." And he truly remarks, that "the enunciation of a physiological law, or of any great fact in structure or function, is a fructifying principle to the pathologist, suggesting to him new elucidations of disease, and the only rational criterion of treatment;" whilst, on the other hand, the defects which are most striking in the present aspect of pathological science, the directions in which the phenomena of disease still lie uninterpreted, are those in which physiological knowledge is as yet most incomplete.

Next in order to the process of determining the nature and signification of phenomena, is that of *discovering their causes*. This, as Mr. Simon justly observes, "is the function of the philosopher, to which he himself attaches the highest importance; and it is that, too, by which the laity are most apt to measure him." There is perhaps no department of medicine which has made more rapid advance of late years, than that of Etiology; and none in which the beneficial effects of increased knowledge are capable of being more extensively and rapidly developed. For his conviction of the efficiency of a supposed cause, the intelligent practitioner no longer rests satisfied with the *post hoc, ergo propter hoc*, as his inductive formula; but he seeks to gain a definite acquaintance with its *modus operandi*, and to trace its agency through the successive phenomena of the disease which it has engendered. In prosecuting this search we must steadily rely on the conviction of the uniformity of the operations of nature; and we should also keep in view two leading principles, the first of which is thus set forth by Mr. Simon:

"First, we know that *causes choose their organs of manifestation*, with as decided, and sometimes as exclusive a preference, as governs the phenomena of inorganic chemical affinity. This we may make matter of experiment; if we introduce various noxious agents into the stream of circulating blood, all organs are equally exposed to their influence;—but how differently are they affected. Inject opium, and the brain suffers; arsenic, and the stomach inflames; strychnia, and the cord is acted on; cantharides, and the kidneys are irritated; and all this so definitely, that the attraction evinced is equal to a chemical demonstration of the agent employed. When the gums swell with mercury, or (as Dr. Burton has shown us) become blackened by lead, we have no more hesitation in naming the cause, than if we had the minerals precipitated in a test-tube. Equally precise is the elective

affinity of morbid poisons, which indeed we recognise and distinguish only by their specific attractions. In the abdominal flooding of cholera, in the coughing and sneezing and snivelling of influenza, in the eruption of the exanthematous fevers, we see the human body yielding uniform local phenomena to the excitement of specific causes, with just as much constancy as is found in the reactions of brute matter; and the evidence, that various diseases have their specific ranges of affinity, is just as clear as that demonstration of a chemical attraction which we find in the precipitation of sulphate of baryta, or the combustion of phosphorus." (p. 23.)

We do not think Mr. Simon equally happy in his statement of the second principle, in which he aims to express "the contrast between the variety of causes and the simplicity of their organic effects; the singleness of life and living reaction, opposed to the multiplicity of exterior contact." For he asserts that—

"Whatever be the cause of disease, the affected organ has but a single method of suffering an effect; and its cognizance of injury can be manifested only by *quantitative changes* in its nutrition or its function,—only by more or less of those acts, through which it depends on the total system, or ministers to it. Let the sensitive part of the eye or ear be struck, or receive an electric shock,—it conveys to the centre only its own specific message of light or sound; let the brain be disturbed by over-work, or by alcohol, by fever, or by mechanical injury,—its response is only in some distortion of thought or action; irritate a secreting organ—skin, liver, kidney, bowels,—by whatever means, by drug, by mechanism, by infection;—each, analogous to its fellow, answers only by an augmented effort of the cell-growth peculiar to itself. Under no quantitative variation of stimulus will the liver secrete urea, or the kidneys bile; any supply of nutrition to either organ, above its power of specific appropriation, either runs off unchanged (as in congestive albuminuria) or undergoes a peculiar development, identical for all organs, into the so-called products of inflammation." (p. 24.)

The statement that the influence of external agencies in modifying the structure and functions of the several organs is *quantitative* merely, appears to us a most unwarrantable assumption. It is negatived by one of the very instances adduced by Mr. Simon in its support; for if there is no clear evidence that urea has ever been secreted by the liver, there is ample proof that it has been separated from the blood by other glandular structures than the kidney; and we are surprised that Mr. Simon should not have remembered that biliary matter is continually secreted by the kidney in cases of disease of the liver. Take, again, the influence of mental emotion on the function of the mammary gland; a large part of this is undoubtedly merely quantitative; and we may even admit that a considerable part of the occasional depravation of the secretion is due to a mere alteration in the proportions of its ingredients. But the fluid is sometimes so changed, under the influence of violent passion, as to act on the infant after the manner of a powerful narcotic poison; and there cannot be a reasonable doubt that some substance entirely new to it has been communicated to it through the agency of the mammary gland.—We believe that the correct statement of the principle would be, that the morbid changes, whether structural or functional, occurring in any organ, always *bear a certain definite relation* to its normal structure or function; the amount to which either can be affected by external agency being restricted within very positive limits. Thus, the inflammatory process has a very close relation to the normal operations of nutrition; but it cannot be said to be the result of a mere quantitative change in any or all of these.

And the perverted glandular actions to which we have referred are all secreting operations, although an abnormal process (or, at least, a process abnormal as regards the particular gland,) is substituted for the normal one.

As an illustration of the influence of these laws, and of the inquiries based upon them, in purifying our theories of causation, Mr. Simon especially dwells on the difference in the aspect which the doctrine of "sympathies" now presents, from that which it has until recently borne.

"Every one can remember when sympathies were spoken of as ultimate facts; when no one thought of explaining them. If it happened that any two organs had the habit of suffering conjointly, with some show of uniformity, they were said to *sympathise*. By degrees the word got to be applied to the most accidental concurrences of disease, till men forgot that, to speak of organs sympathising with each other was only to say, in Greek instead of English, that they suffered conjointly; and the phrase was taken to be in itself an explanation, or to express a law of the animal economy, instead of suggesting a train of thoughtful inquiry. No bad instance of what Bacon terms the *idola fori*." (p. 25.)

We now know that, as distant parts of the body are knit together into a single system chiefly by two means of connexion, the *blood* and the *nerves;* so there are *humoral* sympathies, of which the blood is the medium, conveying the materies morbi to all parts of the system, and imparting it to those organs for whose texture it has a special affinity; and *nervous* sympathies, which depend on nervous cords for their transmission, and nervous centres for their reflexion.

The study of the *juvantia*, or the means which promote recovery from disease, is much more closely connected than is commonly supposed with that of the *lædentia*, or agents which tend to produce disease in the healthy body, or to aggravate disorder when it exists; and it should be prosecuted in the same spirit of inquiry. For without a knowledge of the *modus operandi* of our remedies, our practice is still in great degree empirical, even though we may be fully cognisant of the nature of the disease, and may have clearly traced the causes on which it was dependent. There is no point, as Mr. Simon justly observes, in which pathology comes more nearly into relation with practice than here; none, where it may do more service in rendering medicine rational and secure.

"Surely the counter-agents of disease, no less than its causes and *promovents*, belong to pathological study; surely the action of opium on the brain, or of turpentine on the blood, is as much a matter of investigation to the pathologist as the nature of delirium tremens or the causes of hæmatemesis; nor is the latter knowledge complete without the former. Independently, too, of other obvious reasons for connecting the two studies, it may be observed that the same medicines as cure one disease by their appropriate exhibition, produce another by their needless employment: they are the causes of disease to the healthy, in proportion as they are means of cure to the sick: they thus come directly, as on other grounds indirectly, within our etiological province. And surely, if it be a fit problem for the pathologist to determine, how arsenic produces gastritis or coma; it will be no inappropriate task for him to inquire by what manner of working the same drug effects the cure of intermittent diseases, or suppresses the desquamation of lepra." (p. 29.)

He particularly directs attention to the action of so-called *specifics*, as a matter in respect of which pathologists have been strangely indolent, but whence important information might be drawn, not merely as to the

modus operandi of remedies, but as to the nature of the diseases which they antagonise; and he justly remarks that "every dose of medicine, ineffectively given, silently testifies to the insufficiency of our knowledge; we cannot with certainty predict the action of our drugs, because we are ignorant of the pathological conditions of their efficacy." The admirable course of lectures on the "Influence of Researches in Organic Chemistry on Therapeutics, especially in relation to the Depuration of the Blood," recently delivered by Dr. Golding Bird at the College of Physicians, and published in the 'Medical Gazette,' is full of illustrations of the direct practical value of the inquiries which Mr. Simon urges upon our attention; and we may particularly refer to the distinction drawn by Dr. G. Bird as to the *modus operandi* of the *specific diuretics*, such as squill, digitalis, turpentine, and colchicum,—which increase the discharge of water without augmenting the solid matter of the urinary excretion,—and the *chemical or alterative diuretics*, of which potass in combination with vegetable acids seems to be the most efficacious; these last producing a great increase in the organic compounds excreted, especially in that mixture usually set down as extractive. We shall take an early opportunity of surveying the present aspect of therapeutical inquiry; and shall not, therefore, dwell longer upon this point at present.

In reference to the *classification* of facts, Mr. Simon remarks that, although by no means the chief object of pathological study, it furnishes a very good measure of the success with which the other objects are pursued; since correct classification can proceed only from accurate knowledge. Very different ideas are attached to this term, according to the degree of philosophical insight possessed by the individual; and very different will be the steps taken to carry these ideas into practice. We at present possess nothing that deserves the name of a system in pathology, and it is not desirable to attempt the premature construction of such an arrangement in the present transition-epoch of the science. But it is desirable to ascertain the direction in which our generalizing faculties may be most profitably employed; and the necessity of an *essential unity* as the condition of philosophical generalization may perhaps be best elucidated by examples of its deficiency.

"Suppose, then, it were proposed to classify diseases according to the colour displayed by the skin, under the several morbid influences. The advocate of the system would maintain it, no doubt, by plausible arguments: he would show how natural his method (for, are not all colours fixed by Nature?) and how comprehensive (for, are not all our patients of some one tint or other?): and he would point triumphantly to his diagram tabulated with perfect neatness. There, in the white column would stand his diseases of exhaustion; jaundice and cyanosis would stand as types for the yellow and the blue diseases respectively; scarlet fever would marshal the exanthemata; the green sickness would be a great fact; and, finally, if our philosopher happened to be a friend of the slave-trade, he would no doubt take blackness as the symbol of congenital inferiority, and put negroes at the head of his last column. Now, in laughing at the manifest absurdity of such a proposition, what is the flaw—let us inquire—which so instantaneously strikes us? Nothing in the chromatic details; we leave the facts as he states them; inflammation *is* attended by redness, and bilious patients *are* yellow; but we attack the principle, and laugh at its self-evident insufficiency:—thus. It professes to answer the great requirement of the reason; it engages to embrace the manifold as one; it affirms an unity; but the unity which it enunciates is irrelevant to the subject,

instead of being essential to it; for what has colour to do with life?" (Inaugural Address, p. 39.)

The fundamental idea on which all classification should rest, according to Mr. Simon, is that of *life as a power*; all pathological phenomena being viewed as operations of that power, acting under abnormal conditions. In the production of these phenomena, two elements are always concerned; namely, (1) impressions from without, as the causes of disease; and (2) the excitability of the subject, as the liability to disease. In attempting to classify morbid phenomena, therefore, it is necessary to study on the one hand the vital forces concerned in their production; and, on the other side, to consider morbid influences or causes, not so much in their relations to the outward world, as in their operation upon the living system. And for these purposes, it is requisite to go to the very root of the subject; to connect our heads of classification with those genetic laws, which stamp on each disease *ab initio* its individual type and specific import.

There are many generalizations, however, of a subordinate, and even somewhat empirical character, which are extremely useful in establishing the natural relations and affinities of disease, and in guiding and suggesting inquiries which may lead to the knowledge of the true correlation of morbid phenomena. Such are those which simply express the concurrence or non-concurrence of particular phenomena; and it is by aiding in their establishment, that the numerical method is capable of rendering greater service to pathological science, than by any simple expression of the relative frequency of particular combinations of symptoms. Thus Rokitansky asserts the absolute incompatibility of cancer and tubercle; if this assertion should be confirmed by further inquiry, it cannot but aid us in our study of the fundamental relations of these diseases; if, on the other hand, a single exceptional case should present itself, the generalization must be changed to a simple expression of the extreme infrequency of their coexistence.

Mr. Simon concludes his Address with a high-toned appeal to his hearers and readers to put forth their best exertions, not merely in the acquirement of what is already known, but in the prosecution of more extended researches.

"Before every other inducement to the study, I feel bound to give you this: Pathology is the scientific foundation of medicine; *your success in the practice of your profession,*—your success in prolonging human life and in lessening human anguish,—*will (cæteris paribus) be exactly commensurate with your pathological acquirements.* I am not unaware that the ingenuity of indolence may find partial contradictions to this sanguine belief, and may point complacently, no less to the imperfections of our present knowledge, than to the occasional felicities of the merest empiricism; but such arguments are almost hourly losing their low and limited application; and for the general accuracy of my statement, I appeal without hesitation to the many enlightened members of the profession, whom I have the honour to see here. I am sure that their daily experience amply corroborates my assertion, and justifies me in stating that your chief inducement to the study of pathology is included in those motives which should impel you, with hopes of distinction and utility, to labour in your profession generally." (p. 46.)

No one, we feel assured, even moderately acquainted with the present aspect of medical science and art, will hesitate in a cordial concurrence with these sentiments. The doubts which would have been legitimate

twenty or even ten years since, are now shown to be without foundation. The art has been rapidly advancing from an empirical to a scientific character; much that was untenable has been overthrown; much that was uncertain has been positively ascertained; and, above all, the relations between the natural and morbid processes have been developed to a degree that, even within a very limited period, was scarcely dreamed of. We cannot look back without a sort of horror to the absurdities that were practised not so very long since, and this even under the guise of an attempt at scientific precision. Take, for example, the treatment of chlorosis; in which the unfortunate patients were condemned, day after day, to lose by bleeding an additional quantum of the few blood-corpuscles they possessed; and this, forsooth, because the blood showed the buffy coat, and the patients had pains in their sides; whilst iron, whose remedial virtues we can now explain, was administered with a feeble and hesitating hand. How many poor creatures have had their health ruined for the whole remainder of their lives by this barbarous system, it would not be very easy to reckon up; but assuredly this was one of the cases in which Nature would have cured the disease much more rapidly and safely than the system of treatment then in vogue.

We have already gone over so much of the ground which is traversed by Dr. Mayo, in his 'Outlines of Medical Proof,' that we need do no more than briefly advert to the plan and scope of his *brochure*. It appears to have been suggested by Mr. Green's Hunterian Oration for 1847; in which that learned author laid down a set of principles for the due cultivation of the mind to be devoted to the study and practice of medicine. "The immediate application of the mind," says Dr. Mayo, "cultivated as I shall presume it to have been on the principles laid down by Mr. Green, is beset by many difficulties in respect to the mixed and varied modes of reasoning which are called out by this complicated subject;" and in order that the aspirant may fulfil the great task before him, he must have been taught, or must teach himself, to form a just conception of the peculiarities of proof incident to it and to its auxiliary branches of knowledge. This essay is intended as a sketch of the training which, in Dr. Mayo's opinion, he ought to undergo; and it is, in many respects, well adapted for its purpose. It appears to us, however, deficient in a due appreciation of the most important part of the genuine process of induction; namely, the "connecting idea;" and there is also a want of systematic arrangement, which indicates such a degree of indefiniteness in the mind of the author himself, as impairs the value of his production.

We have thus placed before our readers, chiefly in the words of the authors, the views of several members of our profession, distinguished by the honorable position they have gained for themselves, as to the means by which its character may be elevated, its credit augmented, and its usefulness increased; and we find a remarkable harmony amongst them. They all tend to this,—that the time is now come for abandoning all reliance upon authority, and for laying a secure foundation for such a truly scientific pathology as shall give to the practice of the art a degree of definiteness and certainty which it has never yet possessed. There is a complete agreement, too, in the conviction that phenomena of organization are equally

capable with those of the inorganic world, of being systematised and brought into correlation; and that it is only from the complexity of the conditions under which they usually occur, that any difficulty can arise in the way of their analysis and arrangement. It is urged on all hands that the method by which success has been attained in other sciences, is the one to be followed in medicine; whilst, on the other hand, any departure from that method, however specious may be the results obtained, is almost certain to lead into error, and thus in reality to retard the progress which it was intended to accelerate.

The past history of the profession, and many features in its present aspect, do not hold out much encouragement to the idea of rapid progress in a truly philosophical method. The truth may not be very palatable, but it ought nevertheless to be told, that the great bulk of the profession does not possess those habits of mind, which would fit them for the prosecution of scientific inquiries, even if they were otherwise disposed to undertake them. They have not manifested that singleness of attachment to TRUTH, however it may affect their own supposed interests, which is the first qualification for its pursuit; and they have not exhibited that capacity for the correct construction of even the most simple trains of reasoning, which is an essential requisite in applying to particular cases the teachings of experience or the scientific principles already ascertained. If they had, our medical publications would not teem, as they do, with the crudest speculations, the most illogical deductions. But although this is the *general*, it is far from being the *universal* character of the profession; there is a continually increasing minority, (in the ranks of which, we trust, all who have the good sense and discernment to be readers of this Journal, are to be found,) whose well-cultivated minds are alive to everything which can tend to the elevation of medical science and the improvement of medical art; and who are qualified, by previous education, and by taste for intellectual pursuits, for taking an active share in the movement. Of such we have only to entreat that they will persevere through difficulties and disappointments in the glorious path which is opening to them; and that they will, above all things, strive to cultivate in themselves and to diffuse amongst others that philosophic spirit, which, as Dr. Thomas Brown truly observed, is far more valuable than any limited attainments in philosophy;—"a spirit which is quick to pursue whatever is within the reach of human intellect; but which is not less quick to discern the bounds that limit every human inquiry;—which knows how to distinguish what is just in itself, from what is merely accredited by illustrious names; adopting a truth which no one has sanctioned, and rejecting an error of which all approve, with the same calmness as if no judgment were opposed to its own; but which, at the same time, alive with congenial feeling to every intellectual excellence, and candid to the weakness from which no excellence is wholly privileged, can dissent and confute without triumph, as it admires without envy; applauding gladly whatever is worthy of applause in a rival system, and venerating the very genius which it demonstrates to have erred."

But it is in the improvement of Medical Education that we place our highest and surest hopes for the advancement of science, and the elevation of the professional character; and we now invite the attention of our

readers to a consideration of the means, by which this may be most effectually accomplished. Let it not be supposed that such changes as those we are about to suggest will affect the rising generation alone; they must have a powerful reaction on the existing body. Those already in possession of the field will find themselves outstripped in the race for honour and emolument by younger aspirants, unless they do something to maintain the position they have acquired. A superior professional education cannot be given without superior teachers; and the increased knowledge required of the student cannot be tested without an adequate increase on the part of the examiner also. An extension of the curriculum of lectures and hospital attendance is of little avail, unless means are taken to ascertain that the student profits by it; and for this purpose such alterations must be made in the method of examination, and such an elevation must take place in its standard, as will necessarily produce a much higher grade of professional acquirement in *all* those who participate in it.

Before proceeding to the subject of purely medical education, let us dwell for a short time on the question of preliminary training. On the necessity of this we suppose that there cannot be two opinions; and yet we find that no evidence of it is required by one of our chief examining boards, and extremely little by another; whilst, little as this is, we believe that a considerable proportion of the students who are preparing to present themselves at Apothecaries' Hall, find the Latin examination their greatest obstacle. Nothing can more forcibly demonstrate the low grade of preliminary education common amongst those who enter the profession; for although we are far from considering classical acquirements as an adequate test of general intellectual cultivation, yet, as the education of the middle classes is at present conducted, it is pretty certain that a youth who finds it difficult to translate a page of Celsus or Gregory, has not learned much else. Let us first consider what are the chief requisites in the preliminary education of the medical student; and then inquire how these may be best fulfilled.

The object of all education is twofold; namely, to form certain habits of mind, and to impart a certain amount of useful knowledge. This distinction should be constantly kept in view. There are few objects of study that may not be made to answer both purposes; but the degree in which they do so will depend in part upon the mode in which they are taught, and upon the ultimate destination of the pupil. Thus there is no department of knowledge, however dry and barren, that may not be made, by a judicious teacher, an advantageous medium for intellectual discipline; on the other hand, there is none which may not be taught so carelessly or unphilosophically, as to be almost useless in this respect. So, again, the knowledge most useful in after life is by no means the same for the clergyman and the barrister, the engineer or the physician. We must therefore consider what are the subjects on which the medical student ought to have positive information, and whether the acquirement of a certain knowledge of these will afford a sufficient guarantee that the higher object of education has been answered.

Considering, then, that the destination of the medical student is to be scientific, we think that the study of the various departments of natural science ought to form a leading part of his preliminary education. A general acquaintance with the facts and principles of physics and chemistry

will serve as the best foundation for the study of the special branches of these sciences, on which he will subsequently enter; whilst a mastery of the outlines of zoology and botany will greatly facilitate his future physiological inquiries. And as a foundation for these pursuits, he ought to have previously acquired a tolerable knowledge of arithmetic, algebra, and mathematics. Now every one of the departments of knowledge we have named, if taught by a judicious instructor, is capable of being used as a means of mental discipline, to which none can, we think, be superior for the purpose in view. In the study of numbers and quantities, if pursued as an effort of the intellect rather than of memory, the reasoning powers are trained to act in the simplest, but at the same time the most definite manner. To this definiteness of apprehension we attach the greatest importance. The student ought not merely to *learn*, but to *know*, and *to know that he knows*. We have often fallen in with persons possessing a large amount of general information, but of so indefinite a habit of mind as not to have clear ideas upon any one subject; frequently thinking themselves well acquainted with that of which they were entirely ignorant; and as often, from their want of definite mental conceptions, imagining themselves ignorant of much that they really knew. In medicine, this indefiniteness is the greatest foe to improvement, whether individual or general. It is this which leads men to be satisfied with vague generalities, such as "vital principle," "nervous energy," and the like; instead of separating their knowledge from their ignorance, and seeking to widen their acquaintance with the mode in which Nature operates, by gaining admission to her penetralia. The education of the medical student should therefore be particularly directed to the correction and repression of this tendency, if it exist, and to the cultivation of its opposite.

Again, we have seen the importance of cultivating all those faculties which are employed, not merely in the acquisition of knowledge, but in the pursuit of truth; the medical student, above almost all others, ought to be qualified to analyse with penetration, to compare with a clear perception of real analogy, to reason with logical precision, and to apply principles to practice with ready apprehension. Now for the development and training of all these powers, we cannot imagine any better means than the well-directed study of the various branches of natural science; for it will afford everything which can be of service in the discipline of the intellect, training it in those habits which will be most serviceable in after life; whilst at the same time it will prove of direct benefit, by laying up a store of facts which will never be without their value to their possessor. We doubt whether the mere study of formal logic will ever make a good reasoner; and we are not disposed to recommend that it be introduced into the curriculum of preliminary education; but the course of scientific instruction should be so carried on, as to lead the student to a clear perception of the nature of the reasoning processes, their respective uses and applications, and the principal fallacies by which he is likely to be led astray.

We would not be thought to undervalue the study of language, in assigning to science the most prominent position in the preliminary education we are recommending. It has its own particular set of advantages, and can never be omitted without detriment. There are some valuable habits of mind which cannot be so efficiently cultivated in any

other way; and if we merely consider that we are mastering the great instrument of human thought, the great means of communicating and receiving knowledge, we must at once admit its vast importance. With respect to the choice of languages to be studied, there may be a difference of opinion. Practical utility would urge its claims in favour of French and German; but as means of mental discipline, the superior advantages of the classical languages of Greece and Rome must be freely admitted. It must be borne in mind, too, that even a slight acquaintance with the latter greatly facilitates the acquisition of the former; and that it renders much of the nomenclature of science pregnant with meaning, which would otherwise be a series of hard uncouth names. Moreover, whether justly or not, classical study is at present regarded as an essential part of a liberal education; and although the routine system of instruction too commonly followed deprives it of much of the value it might be made to possess, nevertheless, if judiciously taught, we believe that there is no single object on which so large a proportion of the intellectual faculties can be engaged with equal benefit. It is the too prominent and exclusive character given to classical instruction, in most of our present systems of education, against which our objections are urged; and the superior rank we would assign to the exact sciences, in the preliminary training of the medical student, chiefly depends upon what we believe to be their superior aptitude for cultivating those habits of thought which his professional pursuits most require. Let it never be forgotten that the great minds of the golden age of Greek philosophy were nurtured on no literature but their own; Plato and Aristotle were not brought up in classical schools, nor crammed by the tutors of ancient universities; but their minds were formed by the instructions of men who had already reached the highest wisdom of the time; and thus trained, they went forth into the field of philosophy to reap new harvests from their own sagacity and toil.

We consider, then, that every medical student, before commencing his professional education, should undergo a Matriculation Examination, which should test his acquaintance with languages, and the principles of grammar, arithmetic, algebra and geometry (to the extent, we will say, of quadratic equations, the first six books of Euclid, and conic sections), natural philosophy, chemistry, and natural history, with the outlines of comparative anatomy and physiology. It may be said that our requirements are too high; that we should deter young men from entering the profession; and that we shall thereby add to the number of unlicensed practitioners. Such an objection well deserves consideration; and we should perhaps best meet it by saying, that we would not *begin* with too high a standard, but that we would endeavour *progressively* to raise it to that which we have named. And we may remark that the standard of education among the poorer classes is undergoing such rapid elevation, that if that of the middle classes is not correspondingly raised, it will soon be left at a lower level. The existing Matriculation Examination of the London University would partly meet our wishes; but the classical, philological, and historical subjects have so great a preference in it over the mathematical and scientific, that we should desire to see it greatly modified, if it is to be the sole test of the student's acquirements in the latter. There is obviously, however, no existing board to which the conduct of such an

examination might with so much propriety be intrusted, as to the Senate of the University. We speak, of course, of England only. There would be no difficulty in making a corresponding arrangement for a matriculation examination of Scotch and Irish students by their own respective universities.

Our requirements would not extend beyond that which a well-educated youth of seventeen or eighteen years of age might be reasonably expected to have mastered; and we do not think it desirable that regular professional study should be commenced before that period. General experience teaches that when once the pupil has entered upon his medical curriculum, he attends to little or nothing else. This is partly, no doubt, because the course of study laid down for him is amply sufficient, if properly pursued, to occupy his whole time; but when the leisure period of examinations passed, and of practice waited for, arrives, how few there are who avail themselves of the opportunity to carry forwards their general education, whose progress had been too early suspended. Moreover, we are satisfied that the habits of mind we should thus form, would so greatly facilitate the acquisition of professional knowledge, as to render four years of purely medical study, equal to at least six commenced at fifteen or sixteen years of age, the preliminary training being thus curtailed of its most valuable portion. For as Dr. Ware justly remarks:—

> "The habit of acquiring knowledge is not attained at once. It is not sufficient that the mind be opened to knowledge. It does not flow in spontaneously. It must be sought, and actively appropriated. The power of doing this to advantage, is to be acquired; and it is in no way so easily or so well acquired, as by the teaching of the school and the college. The graduate, other things being equal, starts in his course with some advantage. He has already *learned how to learn*, and has only to occupy himself in the appropriation of knowledge." (Medical Discourses, p. 58.)

It is to be remembered, too, that we would include in our preliminary course, two subjects which at present form part of the medical curriculum, —namely, chemistry and botany. It appears to us that the present place of these subjects is extremely anomalous. We shall not, we trust, be accused of undervaluing chemistry as a branch of medical study, in saying that in our opinion the chemical course ought not to form part of the purely professional system of education. For how stands the matter at present? The student either gives but little heed to it, picking up only as much knowledge as may be requisite to enable him to pass his examinations; or, if he feel interested in the pursuit, he devotes to it an amount of time and attention which can be ill spared from the more directly professional courses. In the former case, he loses the whole benefit of that mental discipline which the study of chemistry is peculiarly qualified to afford; in the latter, he loses valuable time, which he will find it difficult afterwards to replace.

Moreover, it is *not* from the course of chemistry, as usually taught in our schools and colleges, that he gathers those refined applications of chemical doctrine, which constitute the practical value of the study to the physician. The chemistry of physiology is (or ought to be) taught in the physiological course; that of pharmacy in the course of materia medica; that of pathology and therapeutics in the course of the practice of medicine. We would have an even increased amount of attention bestowed upon these topics; and the student would be more likely to come to them with a

mind prepared to appreciate their value, if his attention had been given to chemistry as a science at that period of his educational course, at which it might be legitimately made a prominent object of pursuit, and at which the mastery of its principles would have the most beneficial iufluence on his intellectual character. Similar observations apply to Botany. The small practical value of this study, as regards its applications to medical practice, must be admitted by every one. Its chief benefits lie in the preparation which it affords to the study of physiology, the principles of which can only be thoroughly understood from a comprehensive survey of the whole organized world; and in the knowledge it communicates of medicinal plants. How soon its details are forgotten, after they have served to carry the pupil through his examinations, almost every one of our readers must have found out for himself. By putting it into the course of preliminary study, we should place it in a more favorable position for exerting its really beneficial influence; and we should leave more time, in the years of medical education, for the development of such details of physiological or systematic botany, as are most intimately connected with professional pursuits. What claim botany has to be included in the medical curriculum, whilst zoology and comparative anatomy are excluded, we are at a loss to understand. For if the former helps us to a more comprehensive knowledge of materia medica, the latter is indispensable to a really scientific acquaintance with physiology. We would have an outline of the latter, therefore, combined with the former in the course of preliminary study.

We arrive, lastly, at the subject of purely medical education; a subject on which it would be easy for us to enlarge, so many are the remarks which suggest themselves to our minds. We must content ourselves, however, with requesting the attention of our readers to the results of the observation, inquiry, and reflection, which we have for many years directed to it.

In the first place, we must unreservedly state our conviction, that the examination which at present constitutes the highest test of qualification for the great bulk of the profession in England,—that, namely, for the membership of the College of Surgeons,—is so far from being suitable to its professed purpose, that it tends to *keep down* the standard of professional acquirement, rather than to elevate it. This charge we bring, after an inspection of the lists of questions recently put to several candidates, who have taken the trouble to commit them to writing whilst still fresh in their memory. From the information we have collected in this and in other ways, we gather that the present examinations at the College are, as nearly as may be, of the same kind and scope as those of 25 and 30 years ago. We can see in them not a single indication of the vast change that has come over anatomical and physiological science within that period; many of the questions are almost puerile, and would be answered by the merest tyro of the dissecting-room; and many others are mere "catch" questions, designed rather to perplex the candidate, than to ascertain his real professional acquirements. The questions on anatomy are almost entirely restricted to bare details, many of them having no relation either to physiology, or to surgical or medical practice; a knowledge of the anatomy of the tissues does not seem to be at all required; physiology is

a dead letter; organic chemistry makes no show; and surgery is the only practical subject examined in. The examination is usually restricted to an hour, the candidate being sent about from one table to another, on the ringing of a bell, at each quarter; his questioners are consequently debarred from following up any subject that may have been started near the period of change; whilst the movement is peculiarly calculated to distract the attention and flurry the nerves of the anxious youth. The examiners are, for the most part, gentlemen advanced to that period of life when the acquisition of new knowledge becomes rather burdensome; and instances have not been wanting of this important post having been held by individuals actually imbecile from age. Such is the present condition of the examination for the membership; we have no doubt that the introduction of the higher qualification for the fellowship will in time react beneficially upon the lower grade, by necessitating a better acquaintance, on the part of the examiners, with the higher departments of medical science; but we should be sorry to be obliged to wait until then for the improvements we desiderate. It strikes us as a very curious anomaly, that whilst an acquaintance with the "principles of medicine and of midwifery" is required from the candidate for the *fellowship*, of whom it may be reasonably presumed that he intends to devote himself chiefly or exclusively to the practice of *surgery*, no such requirement is made,—so far as appears in the regulations, or as may be inferred from the records of the examinations which we have scrutinized,—from the candidates for the *membership*, by far the larger portion of whom are destined to enter on *general practice.*

Thus, then, notwithstanding the increase in the duration and cost of education, the larger number of lectures, and the lengthened term of hospital attendance, now required from the pupil, the College examination which was considered fit to test his capacity twenty or thirty years since, is still practically regarded as an adequate standard of acquirement; in other words, the student is practically told that this increase is more for show than for use, and that he may "get up" in one year all that the College will require of him. Can anything be conceived more likely to degrade the student, and to lower the general tone of professional instruction? For what encouragement has a lecturer to keep himself *au courant* with his subject, if his pupils be aware that the heads of their profession deem any scientific acquaintance with it superfluous? Nothing can be more dispiriting to a teacher, than an inattentive and unappreciating class; and it cannot be anticipated that he should long continue to keep up his instructions to a higher standard than is demanded by the requirements of his pupils, unless he is sustained by that conscientious desire to discharge his duty to the best of his ability, which ought to be the animating motive in the breast of every one who takes upon himself that responsible and honorable office.

We turn now to the examination at Apothecaries' Hall, the present condition of which we have had a similar opportunity of scrutinising; and we do not hesitate to say that, taking it altogether, it aims at a higher scientific standard, and is (putting surgery aside) of a much more practical character, than the College examination; whilst, at the same time, it is conducted in a manner much more agreeable to the student, whose thoughts are not distracted by the change from table to table; and being,

moreover, unlimited in its duration, there is a much less chance, either of the rejection of a deserving candidate, or of the passing of a man of doubtful qualifications. The examinations, too, are of a much more progressive character; and this may be attributed in great part, we believe, to the continual infusion of new and young blood into the body of examiners. The Apothecaries' Company most certainly deserves the thanks of the profession and of the public for the efforts it has made to raise the standard of medical education; and this not merely by requiring an augmented period of study, additional courses of lectures, &c., as the College has done: but by the increased stringency of its examinations. There was a time, we believe, when any considerable manifestation of acquaintance with the higher departments of medical study, on the part of candidates for the licence, was rather checked than encouraged; but we believe that no student need now fear being *too well* educated for the examiners at "the Hall." Still this examination is far from being what, in our opinion, it should be. It is too cursory,—the range of subjects it includes, aud the time to which it is usually restricted, being at strange variance with each other; the standard of qualification is not as high as we should desire to see it; and the system pursued is not sufficiently adapted to draw a distinction between the student who has industriously availed himself of the opportunities presented to him during his lengthened course of study, and the candidate who has been idle up to nearly the period of presenting himself, and has then been "crammed" by some accomplished grinder. We have ourselves known cases in which young men, ignorant of even the rudiments of medical knowledge, have been admitted members of the College after three months of such cramming (being blessed with good memories, and with a considerable capacity for work, upon occasion); and have successfully presented themselves at the Hall after three months more of such preparation.

Both the College of Physicians and the University of London make their examinations of a much more stringent character; and the latter, by publishing the lists of questions placed before the student on each occasion, gives proof to the public of the high standard of its requirements. But the College exerts its influence on only one, and that the least numerous, subdivision of the profession; and not upon by any means the whole of this, a considerable proportion of the provincial physicians not availing themselves of its invitation, but satisfying themselves with the acquisition of an University degree. There has been some reason, moreover, for the suspicion that its judgments have been determined, not solely by the acquirements of a candidate, but by his position in society and personal influence; as if admission were sought to a club, instead of a license to practise. But we trust that the calumny is now without foundation in truth. The direct influence exerted by the University of London is small; since its degree confers no privilege to practise, and its graduates are not numerous. But we think that it has done much to raise the standard of medical instruction; and has given an encouragement to superior teachers, of which they have been in great need. Its examination is very searching and prolonged; and being chiefly conducted in writing, it gives an opportunity to an accomplished student to manifest his superiority, whilst the timid candidate is not disturbed by the flurry of anxiety. And as a *vivâ voce* examination is superadded, full opportunity is given to the

student to do justice to his attainments, by explaining points on which he may not have expressed himself satisfactorily in his written answers; whilst the examiner has the power of testing the qualifications of men, whose replies had left doubt in his mind as to their right to pass. Like all examinations, however, conducted upon the plan of mere question and answer, whether orally or in writing, this one falls short in regard to its power of testing the *practical skill* of the candidate, either in medicine or surgery; such a knowledge of these subjects as may be derived from book-study, being sufficient to carry the student through his trials.

We shall not discuss the merits of the Scotch and Irish examinations, since upon these we are not sufficiently informed; but we have no reason to believe that their standard of qualification is higher than that of the London Colleges of Surgeons and Physicians, and of the Company of Apothecaries. We are not of course alluding to the examinations for *fellowships*, in any of the colleges; but to that for the mere diploma or license to practise. No examination to be compared with that of the University of London in comprehensiveness and stringency, is conducted, so far as we are aware, by any other body, academic or professional. The medical examinations at Oxford and Cambridge are well known to be utterly inefficient.

In the alterations we should propose, we have endeavoured to keep in view that which is feasible, as well as that which is desirable; but we must at present content ourselves with laying down principles, rather than elaborating details.—In the first place, then, *every* medical student, whatever his ultimate destination, should be required to submit, after four years of professional study, to such an examination as should test (so far as a single examination *can* do) his qualifications for entering upon general practice. His acquirements in anatomy, physiology, pathology, and materia medica, should be investigated by examiners who have made these pursuits their special objects of study; whilst in practical medicine, surgery, and midwifery, he should be questioned by eminent practitioners in these several departments. The former part of the examination might very well be carried on chiefly in writing; in the latter, the oral should predominate. But we should not be satisfied with any mere system of question and answer; the candidate's practical acquaintance with disease ought to be tested; and although such a plan would require the superaddition of a Clinical Hospital to the machinery at present in existence, and would involve an increased expenditure of time on the part of the examiners, we can see no insuperable difficulty in the way of its adoption, —none that could be put in competition with the advantages derivable from it.

We do not see why the General Practitioner should be compelled to undergo any separate examination at the College of Surgeons, if surgery be included in the preceding; there is no more reason, in our apprehension, why he should do so, than why he should be expected to present himself before the College of Physicians to be examined in medicine. But every one who is desirous of obtaining a surgical appointment would continue, as at present, to seek the diploma of the College; the examination for which should be restricted to surgical anatomy, pathology, and practice. And in like manner, the practitioner who desires a special medical qualification, should seek the license of the College of Physicians;

the examination for which should embrace physiological anatomy, physiology, materia medica and therapeutics, pathology, and practice of medicine. The Fellowship of the latter, like that of the former, should be the reward of a higher grade of qualification. As Obstetric practice forms so large a part of the duties of the general practitioner, the first examination in this department should be sufficiently stringent to render any other unnecessary. In all these higher examinations, as in the primary, the candidate should be subjected to *practical tests*, as well as required to answer questions. He should demonstrate and operate on the dead subject, and should be expected to recognise the minute structure of the different tissues and organs as displayed by the microscope, to distinguish the urinary deposits by the microscope and by chemical tests, and in other ways to show his capacity for the observation and investigation of morbid phenomena.

That the student of average capacity may be fully qualified for such examinations as we propose, by four years of faithful and diligent study, following a good general education, we feel no doubt whatever, from our own knowledge and experience. There are many schools in which a considerable portion of the students do actually pass such examinations, under the mere stimulus of emulation, or of the desire to test the amount of their acquirements. And this leads us to the remark, that in order to secure a steady devotion to the acquirement of professional knowledge through the whole period of study, we would alter the whole system of certificates. As at present given, they are almost valueless. The utmost that even the conscientious teacher can say in them is, that the pupil has been *bodily* present at his lectures; of his *mental* presence he cannot speak. And too frequently the certificate is given with very little knowledge on the part of the lecturer as to the amount even of actual *attendance*, still less of *attention*, on the part of the pupil. Now we would require teachers, instead of giving a formal certificate of attendance, to state the result of their inquiries into their pupils' knowledge (whether derived from lectures or from books is after all not of much consequence) by at least two written examinations in every winter course, and one for every summer course; in this manner they would ascertain how far the *object* of attendance on lectures had been attained by the student; and the General Examining Board would have some guarantee that his knowledge had not been "got up" during the few last months of his term of study, the greater part of it to be forgotten again as soon; but that it had been in course of acquirement during the whole period. We feel satisfied that the great majority of teachers would gladly welcome such a change of system, and would give their best endeavours to carry it into effectual operation.

It only remains for us to consider the constitution of the Board, by which the first and most general examination should be conducted. We cannot see the advantage of the proposed College of General Practitioners, so far at least as regards educational plans; for it seems to us very easy to frame a system of united action on the part of the different bodies at present in existence, which should amply meet the requirements of the case. Let the University of London furnish the examiners in Anatomy, Physiology, and Pathology (we suppose it to have already taken cognisance of chemistry and botany in the preliminary examination); let the College of Physicians provide the examiners in Practical Medicine; the

College of Surgeons those in Surgery; and the Company of Apothecaries those of Materia Medica and Pharmacy. If no Obstetric Board be constituted, the University might provide the examiners in Midwifery. An equitable division of fees should be made, according to the proportion of labour undertaken by these several bodies; and the constitution of the board should be such, as to prevent any unduly long tenure of the office by individuals who did not prove themselves to be specially qualified for it.

The whole direction of Medical Education, and the regulation of the relations of the different branches of the profession to each other and to the public, should be committed to a Medical Council; the majority of which should be composed of delegates from the several bodies we have named, and from the corresponding bodies in Scotland and Ireland; whilst a certain proportion should consist of laymen, the representatives of the public, to be named by the Sovereign in Council. We consider that these last should not be sufficiently numerous to force upon the professional portion of the Council, and thereby upon the professional public, any plans of their own; but that they should be in a position to hold the balance between the rival parties, into which the professional section would be most certainly divided; and we believe that they would thus essentially contribute to the settlement of their conflicting claims, and to the formation of a combined and harmonious system of action. That the representatives of the several Medical Boards in London have agreed upon the desirableness of such a Council, and have expressed their readiness to act in subordination to it, we regard as a happy omen that the difficulties in the way of a settlement of the various questions at issue, in regard to medical education and qualification, are likely to be overcome. In committing, however, as they would do, the whole nomination of the "Medical Council" to Government, they seem to us to have lost sight of the interests of the profession, or to have estimated at too low a rate the disposition amongst existing bodies to work together for its common weal, when once they are brought into a position of mutual co-operation.

Should we find that our views on these subjects meet with the approval of the intelligent part of the Medical Profession, we shall follow them up with some practical suggestions as to the mode in which they may be carried into effect, with the fairest consideration of existing interests.

ART. II.

On Wounds and Injuries of the Chest; being the Third Part of the Lectures on some of the more important Points in Surgery. By G. J. GUTHRIE, F.R.S.—*London*, 1848. 8vo, pp. 110.

THE extent of the work before us must not be measured by the number of its pages, for it is printed in small type and double column, and Mr. Guthrie's writing is, for the most part, terse and condensed. We think it necessary to make this prefatory remark, lest our readers should be misled by the impression that the space devoted to it by the author is not commensurate with the importance of the subject; they will, on the contrary, find ample material for careful study and reflection.

Again, we must remark, that the scope and nature of the work, as well as the character and experience of the author, place both almost beyond

the pale of criticism. We do not mean to affirm that Mr. Guthrie is unerring, or that we are disposed to subscribe servilely to all that he propounds; but we may with propriety admit, in reference to military surgery, what he claims for himself in pointing out the inefficiency of the army surgical staff, viz. that "there is no one who ought to understand the subject so well;" though we do not think with him in the conclusion of his sentence, that "there are many, perhaps, who do understand it better." In short, we are conscious that our own comparatively limited experience (in common, we apprehend, with that of all civil surgeons) would make it arrogant on our part to criticise that which can only be learned and known practically by extended observation and experience, guided, of course, by just and enlightened principles; and in no branch of surgery, probably, would theory be found to be more at variance with the results of actual observation, than in the effects of penetrating wounds and other severe injuries, such as are met with on the field of battle. Suffice it, then, to say, that our purpose is to analyse rather than to criticise the volume before us; and we trust that our humble labours may aid in extending the diffusion of the principles inculcated by Mr. Guthrie, by inducing our readers to possess themselves of this legacy from its author.

The thirteen Lectures, which constitute the contents of this volume, are published, the author remarks, "nearly as they were delivered on all practical points;" being open, for thirty years, to all the officers of the various branches of the public service who were willing to attend them. They comprise all those lesions of the chest which the surgeon is ever likely to be called upon to treat. Nay, we had almost added, the physician also, for the first three lectures are much more medical than surgical. We do not quarrel with Mr. Guthrie for this; on the contrary, we entirely concur in the opinion long since expressed by him, and now reiterated,—that the distinction between physic and surgery is an artificial one; "unknown in nature, foreign to her principles, and incapable of being preserved by those who have extensive opportunities of practising the profession." This, however, has become a trite subject, and scarcely one of discussion at the present day. All sensible men, except the very antiquated, hold the same opinion as to the *necessity* (we cannot use a qualified expression) of every surgeon fitting himself for the practice of *medicine*, whatever his field of labour may ultimately be; though, we apprehend, as few would, or consistently can, deny the value of that conventional arrangement which obtains where a large population is accumulated, and where there are extensive public institutions, affording ample opportunities to the officers connected with them for becoming authorities on the higher and more restricted branches of medicine and surgery. It is a consequence which naturally flows from the conditions alluded to; and the public have discrimination enough to be influenced by what is nothing more than a rational belief and just conclusion, that the hospital surgeon's is the fittest opinion to seek in cases of severe injury or other recognised surgical complaint, and the fittest hand to operate where such interference is required. Mr. Guthrie informs us that he has exerted his influence in the council of the College of Surgeons, in bringing about that desirable extension of the qualifications required of candidates for the membership, by which attendance on the *medical* practice of an hospital

is rendered compulsory; for this he deserves our acknowledgments and thanks; and we can readily believe that the military surgeon especially must have brought home to him, when on actual service in the field, the insufficiency of *pure* surgery, in coping with the cases which are brought under his notice, and are subjected to his care.

Those who are fond of military history, and we acknowledge ourselves of that number, will find Mr. Guthrie's work interspersed with many casual remarks, more or less pertinent to his subject, which will amuse and interest them. Thus, we learn, at the commencement of his first lecture, that "during the war in the Peninsula and France, the French cavalry, although always superior in number to the British, did not often avail themselves of that superiority, to attack in the manner they are reported to have done on other great continental occasions;" sword and lance wounds were, therefore, comparatively rare.

This information, we acknowledge, gratified our national vanity; but we were rather vexed with our author for endeavouring to destroy some of the vivid pictures we had been in the habit of conjuring up, in gratifying the aforesaid passion, which whilom led the Grampian hero to long to "follow to the field some warlike train." In most of the great modern battles we had heard of, the climax of the interest was usually looked for in that deadly conflict with the bayonet, by which so many engagements are said to have been decided, and usually in our favour, when we happened to be one of the parties engaged. But no; Mr. Guthrie pretends to tell us that these statements are next akin to old women's tales; mere fables of romance, and not matters of real history.

He asserts that bayonet wounds were not common; for, "although much has been written, and more stories have been told (indeed, so often told, that the people who relate actually at last believe them,) about charging with the bayonet, their crossing and locking, the heroic efforts made by the conflicting parties, and so forth, that it might be supposed these wounds were of frequent occurrence; the fact is, however, otherwise." He then proceeds to state, that the advance of regiments, some in line, others in column, with *intent* to charge with the bayonet, is indisputable; nor is he more willing to question the equally meritorious *intention* of the opposing party to receive the said chargers and their bayonets; but, he adds, by some means, or from some accidental cause of which he does not presume to give any account, "they generally do neither one thing nor the other;" for one party usually thinks "discretion the better part of valour, and walks silently and angrily away," those unfortunates alone suffering who cannot get off fast enough. For our part, we dislike such dull matter-of-fact; we shall adhere to our simple faith in all that is told us concerning these stirring romances of real life, as long as it is agreeable so to do, and leave Mr. Advocate Alison and others to settle the question with our author.

The first subject regularly brought under our consideration is inflammation of the pleura and lungs, constituting "the simplest of the more serious results from injuries not penetrating the chest." These affections, pleuritis and pneumonia, Mr. Guthrie remarks, "are rarely met with separately after wounds, and more particularly the latter, without implicating the former, constituting what has been called pleuro-pneumonia." Some cases are then given, illustrative of this statement, partly from

military and in part from civil practice: and our author adds, that he has seen many instances of persons struck in different parts of the chest, similar to those he quotes, with effects varying from the transient pain occasioned by the blow, to the more serious forms of pleuro-pneumonia; and he considers that the risk of internal inflammation is less where a ball actually makes a wound than where it merely bruises the part.

Case 9 we quote as curious and instructive, and because the author says he has seen many examples of the same form of injury:

"Major L—— was struck by a musket-ball on the left breast, which went through his clothes, the integuments, and the outer part of the great pectoral muscle, and slanted inwards for three inches towards the sternum, to which distance its track could be followed. It was evident that the ball had neither lodged nor penetrated, for no serious symptoms ensued, and I had, with him, as little doubt that it had been ejected the way it went in, by the elasticity of the cartilages of the ribs near the sternum." (p. 6.)

The extreme effects of external injury to the chest are exemplified by a case recorded by Dr. Stokes, in which extensive gangrene of the lung succeeded a blow on the side.

The remainder of the first lecture is occupied with discussing the subject of auscultation in the diagnosis of affections of the chest; and the most ardent follower of the stethoscope will have no reason to complain that Mr. Guthrie underrates the value of this important handmaid to the practitioner, whether medical or surgical; and we are free to confess that he has proved himself, at any rate on paper, and we doubt not is equally so by the bedside, anything but a novice in the use of this instrument. Judicious directions are given as to the method of listening to the natural sounds of respiration, and then for detecting deviations from their normal condition.

In commenting on the observation of Dr. D. Williams (of Liverpool), that a sound lung never fills the bag of the pleura, particularly towards the diaphragm, at least during ordinary respiration, Mr. Guthrie observes, that he requested Mr. Quekett, the assistant conservator at the College, to institute some experiments to settle this point. These we quote, partly for their intrinsic interest, and in part to show that scientific investigations of this sort need not necessarily be blotted by cruelty. We cordially thank our author for affording us this opportunity of recording, in such good company as his, our abhorrence of the "atrocious cruelties" too often committed under the veil of scientific research, but really for the gratification of a morbid curiosity, and an unjustifiable love of positive demonstration and personal conviction of the truth of that, which stands already fully attested by the observations and experiments of others.

"Mr. Quekett stabbed five sheep immediately on their throats being cut: in three of these, in which the knife was passed between the tenth and eleventh ribs, both the lung and diaphragm were wounded. In the fourth, in which the puncture was made between the seventh and eighth rib, the lung only was wounded; and in the fifth, in which the knife was passed between the last two ribs, the diaphragm only was penetrated, the knife passing through it into the abdomen. He therefore concludes, from these and other observations, that the base of the lung is always in contact with the surface of the diaphragm." (p. 8.)

Some introductory observations on the minute anatomy of the pulmonary tissue, and on the physical evidences of morbid action in the pleura and

lungs, derived from auscultation and percussion, form the concluding section of the first lecture. On these it is not our intention to make any detailed comments; but we must remark that Mr. Guthrie quotes no authorities later than Reisseissen and Regnaud (Journ. Hebdom., 1829), in regard to the pulmonary structure, being apparently ignorant how much has been since done in the way of addition and rectification, as to points on which they were very imperfectly informed. And we are startled, too, by the assertion that the pulmonary vesicles, "when dilated with air, constitute what is called emphysema of the lung." Is this a sufficient definition of the affection; and are such statements to be taken as a sample of the information on points of minute physiological and pathological anatomy, which is expected by the Examiners of the College of Surgeons, from candidates for their fellowship? That Mr. Guthrie himself knows better, is evident from the introductory portion of the Seventh Lecture, in which the distinction between *vesicular* and *interlobular* emphysema of the lungs is recognised, and Laennec is referred to as having particularly described and explained the former.

In commencing the Second Lecture, our author states that it is not his intention to give a "history of, or even the whole of the symptoms and the consequences of inflammation of the pleura and the lungs." The next paragraph, however, seems to contradict this announcement; for it contains the symptoms of acute idiopathic pleuritis. We shall not follow him through this division of his subject, which, however essential to its completeness as a whole, is to be met with in other works, more especially devoted to idiopathic affections of the chest. Mr. Guthrie seems to have been at considerable pains to collect and analyse the opinions and observations of others, and evinces much acuteness in commenting on them, and comparing them with the results of his own experience. Thus, Laennec, Andral, Dr. Williams, Dr. Stokes, and M. Grisolle are severally laid under contribution. On the mooted point of the mode in which pus is deposited in the lungs in the last stage of acute pneumonia, he declares in favour of the opinion of the last-mentioned author, that it may occur in distinct, circumscribed abscesses of the lung, and not necessarily in the form of diffused infiltration. Of the truth of this observation we apprehend there can be now but little question.

In the Third Lecture, the changes in the structure of the lung, from pneumonia, are further discussed; and the treatment recommended by various writers commented on. Large bleedings our author depends on as the sheet-anchor in the acute form of this affection, thinking it "almost a question, in some cases, whether the patient shall be allowed to die of the disease, or from loss of blood." He gives a graphic and rather amusing account of how he was led to adopt such very decided practice, when in charge of a regiment of infantry at Torbay, at the age of seventeen. We are almost tempted to quote this passage, but that it is rather too long. The sharp winds and exposed position of the men seem to have made pneumonia rife amongst them, and "the young doctor" began his practice, according to approved London-taught principles, by bleeding his patients ONLY three or four times, and to the extent of only fifty ounces, in the first forty-eight hours, and then administering tartar emetic, calomel, and opium. He was unsuccessful, and found, on examination after death, that his patients lived to the third stage of pneumonia, and

then died. Discomfited by these failures, he determined, that in future "they should die of the bleeding, and not from excess of inflammation," and that there ought to be "no limit to the abstraction of blood in the first instance, but the decided incapability of bearing its further loss." One extreme case we must find room for, premising that the subjects Mr. Guthrie had to deal with, both at Berry Head and in British North America, were all more or less qualified for his decided practice, by robustness of constitution and intemperate habits.

"One of the first of those I had to treat (in North America) was in a grenadier, some six feet three inches high, broad and well-formed in proportion; he owned he had drunk a gallon of rum during the afternoon (!), and very narrowly escaped, even with the loss of nearly as much of his blood, abstracted in a few hours. His first bleeding was into the wash-hand basin, and until he fainted, lying on his back; and the bleedings were repeated as soon as he again began to feel pain. I said to him, you will probably die, but it shall not be from rum or inflammation; and whenever he felt pain, he used to put his arm out of bed to have the vein re-opened, for Jack Martin was a very gallant fellow." (p. 18.)

In passing, we may notice a singular lamentation of Mr. Guthrie over the degeneracy of the present age, in point of early qualification to practise medicine and surgery. The remarks occur in a foot-note, and are evidently called forth by the recollection of his own early competency to undertake an office of important trust and high responsibility. They savour much of vanity and self-satisfaction, and are rather inconsistent with the observations already noticed, which occur in the first lecture, in reference to an extended course of study for candidates for the diploma of the College. We by no means agree in the opinion that a very early and independent responsibility should be incurred by those who have such a grave and serious charge as the lives of their fellow-creatures intrusted to them; and he should not forget that the field of acquirement has been very much enlarged since the time of Louis and of Cheselden,—aye, and even since Mr. Guthrie himself was a young man.

The relative value of antimony, calomel, and opium, in combination or separately, in inflammation of the chest, is then noticed; and this brings us to the close of the Third Lecture, with, as yet, little pure surgery to mingle with the physic.

At the commencement of the Fourth Lecture the subject of typhoid pneumonia is brought under consideration, and this is followed by some observations on the occurrence of pleuro-pneumonia as a sequence of amputation. Mr. Guthrie states, that he was the first to direct attention to this association, in his work on Gun-shot Wounds published in 1815. He considers these attacks of inflammation of the lungs, and purulent deposits in various parts of the body,—which have likewise been noticed to follow severe injuries and operations, by our author, Mr. Rose (in the Medico-Chirurgical Transactions), and others,—to be intimately connected with inflammation of the veins, and "the absorption of purulent matter, which may be deposited in the cellular texture of the body with impunity for a time, but which, in the lung or pleura, is followed by low inflammation, ending in death." This subject will be found elaborately discussed in another part of our present Number. He adds, that the treatment should be conducted on the same principles as that adapted for typhoid pneumonia.

The pathology and diagnosis of empyema we pass by, and proceed to notice (in Lecture Five) the mode of operating for this affection. The method by incision Mr. Guthrie disapproves of, except in cases where the operator has reason to anticipate the removal of some extraneous body. In this opinion we entirely acquiesce, and believe the position at which he recommends the puncture to be made—viz. between the fifth and sixth ribs, and one third of the distance from the spinous processes, forwards—to be the most judicious selection: it may be made rather farther forward on the right than the left side. Where there is reason to believe that a foreign body has intruded into the chest, such as a ball, our author considers that the place of election for incision should be a little above the diaphragm. To this point we shall have occasion to refer again presently. The statistics of the operation are alluded to, and certainly prove the superiority of the method by puncture over that by incision, and the advantage of not delaying the operation too long. In speaking of the admission of air into the pleural cavity, Mr. Guthrie seems to think very lightly of it, and states his opinion, from experience, that the "admission of common air never does any harm in persons in health; and that, when mischief ensues after an operation or an injury, it occurs from the irritation caused in a particular state of constitution, and not from the admission of air." We dare say that air might be admitted into a healthy pleura without much mischief resulting, but we have no personal experience in the matter. Moreover, we do not doubt that the state of the constitution may, and necessarily would, modify the effects of such an agent; but we apprehend that the introduction of air into a cavity,—be it lined by pleura, or be it surrounded by the organized wall of an abscess, it matters little,—which contains pus, must operate prejudicially, by assisting in the decomposition of the purulent deposit, and thus exciting a new morbid action, or augmenting that which existed before.

In discussing the important question involving the alternative of keeping open or closing the communication with the pleural cavity, our author thus expresses himself:

"Supposing that the constant admission of the air does no mischief, is it more advisable that the patient should run the risk of inflammation occurring from the propagation of irritation, from whatever cause it may be excited, by keeping the wound open, and allowing a constant drain from the part,—or should it in all cases be immediately closed? In all cases of serous effusions there can be little doubt that the fluid should be evacuated, and the wound closed. When the fluid is purulent, I am of opinion that a permanent drain should be early established. It is common for the operation to be repeated several times, without the serous discharge becoming purulent; and in such cases it usually becomes necessary at last to allow the wound to remain open, until the discharge shall cease of itself. ....There can be no doubt, in such cases of internal disease, about the propriety of making the opening, between the fifth and sixth rib, a dependent one, by attending to the position of the patient; but, having seen several cases in which an opening lower down, nearer the diaphragm, would have been more advantageous, as more likely to aid his perfect recovery when the patient was able to walk about, and even follow his ordinary occupation, I apprehend that, whenever more than one opening is necessary, and the first is made between the fifth and sixth ribs, the succeeding ones should be made lower down, so that, when it is thought right to leave the last puncture to become fistulous, it may be made as near the diaphragm as may be thought consistent with the safety of that part." (p. 29.)

The practice of injecting stimulating fluids into the pleural cavity, is deprecated by Mr. Guthrie, in common, we apprehend, with all cautious surgeons: and he considers the introduction of even warm milk and water inadmissible, except, "when there seems to be an adventitious cause, keeping up the irritation, which may perhaps be brought to the opening by the sudden abstraction of the injection." The authority of Dr. Gibson of Philadelphia is quoted in favour of this practice; and several instructive cases are given at length, illustrating the author's views and opinions,—especially in reference to the treatment of effusion and purulent deposits in the pleura after wounds.

An interesting observation by Mr. Quekett is here introduced. That gentleman had recently an opportunity of injecting the false membrane formed on the pleura of a cheetah (one of the leopard tribe) which died of empyema, and found that—

"The membrane covering the right side of the chest was upwards of one eighth of an inch in thickness, and the vessels, which were very numerous, were found to be very readily filled from the arteries and veins. The membrane itself could be peeled off from the pleura with little difficulty. The fluid found in the chest contained flakes of lymph floating in it, and when examined microscopically, was found to consist of numerous pus-globules or cells, with lymph-particles and globules of oil; the flakes, and the last portions of the false membrane deposited, were clearly made up of lymph-particles, whilst that which was first deposited, and into which the arteries penetrated, had assumed more of a fibrous than a cellular character." (p. 30.)

The early part of the Sixth Lecture is still occupied with empyema; and the author subsequently gives a summary of his opinions on the surgical treatment of this affection, which we may thus abbreviate. In empyema from a medical disease paracentesis should be performed early, before the lung is permanently compressed or bound down by false membranous adhesions; a small trochar and canula should be employed, and all the fluid be withdrawn if safely practicable; and the wound should be immediately closed. In a *surgical* empyema, the operator will frequently have no choice as to the spot at which he will make his opening; he must follow the indications he may find to direct him, either re-opening the original wound, or penetrating the chest wherever the matter may be discovered pushing the soft parts before it.

The subject of *pneumothorax* receives a brief notice and illustration. For the production of *metallic tinkling*, as an indication, on auscultation, of the presence of air in the pleural cavity, Mr. Guthrie considers that such air must be compressed: we cannot say that we are convinced by his reasoning, and do not agree with him in this opinion.

After making some general remarks on emphysema, at the commencement of the Seventh Lecture, and citing a few extreme cases from the writings of Paré, Wiseman, Littré, W. Hunter, &c., illustrative of its causes and treatment, our author proceeds to criticise the opinions of John Bell and Baron Larrey, with respect to the frequency of this affection as a sequence of gun-shot and punctured wounds of the chest. He says that, in his experience,

"The large opening made by a musket-ball never admits of emphysema to a troublesome extent; and, in fact, it rarely takes place at all. A slanting wound made by a small pistol-shot, may give rise to it, but it seldom does so. It commonly

succeeds long and tortuous wounds, made by swords, lances, or bayonets, and then only shortly after the occurrence of the accident." (p. 47.)

A little further on, Mr. Guthrie remarks that he considers the extent to which the lung recedes from the costal pleura, when an opening is made into the pleural cavity in the dead subject, as deceptive in reference to a corresponding lesion in the living frame; and that if the continued admission of air through the wound be prevented, collapse of the lung scarcely takes place at all. As to the treatment of this condition of the pleura and its contained viscus, our author sides with Hewson and B. Bell, and against John Bell and Mr. S. Cooper, in favour of making an opening for the exit of the air, when the urgency of the symptoms seems to require it. He considers Mr. Cooper's opinion, "that it is impracticable to make the collapsed viscus, or lung, expand until the breach is closed," erroneous, and states what is his own practice, in the following words:

"The course, then, to pursue in such extreme cases, is to puncture the chest, evacuate the air, withdraw the canula, and close the opening.

"The life of the patient having been thus saved, time is given for the wound in the lung to heal under the usual inflammatory processes, provided it will do so without a recurrence of the mischief, which if it should take place, must be met by another puncture, or the opening in the chest should be made permanent, in order to equalize the pressure of the air in the cavity." (p. 47.)

Hernia of the lung, as the result of injury, is, in Mr. Guthrie's experience, a complaint of rare occurrence. He has seen but two undoubted cases of it, and in these he remarks (in reference to an observation of Sabatier to the contrary), "the elevation of the part, or protrusion, took place during expiration."

A considerable section of the work is next devoted to the interesting subject of "Wounds of the Heart;" and our author has been at much pains to collect the most trustworthy records of cases of this form of injury. That recovery may take place, after even severe wounds of this important organ, seems to be attested beyond doubt; though, in the majority of instances, the nature of the lesion, when not fatal, can be only conjectural. Several examples, however, are recorded of survival for many hours, and even days, after perforation of one of the cavities of the heart.

Both Senac and Ollivier state that wounds of the right ventricle are more common than of the left; and, the former adds, more speedily fatal. Foreign bodies have been found lodged in the substance or cavities of the heart, both in man and animals, long after the subjects of the injury had recovered from its immediate effects. Thus, Mr. Guthrie quotes an instance related by Fournier, in the 'Dictionnaire des Sciences Médicales,' of a soldier who was wounded by a musket-ball in the chest, and, after nearly dying of hemorrhage, recovered and lived six years. On examination of his body, the ball was found "lodged in the right ventricle, near its apex, to which part the pericardium was attached, and by it to the mediastinum."

Perhaps the most remarkable instance on record of complicated injury to important organs, including the heart, and not immediately fatal, is that related by Dr. Babington in the 'Medical Records and Researches' for 1798, and quoted by our author. The subject of the accident fell on a bayonet, which pierced his colon, his stomach twice, liver, diaphragm,

right ventricle, and middle and upper lobes of the lung on the same side. He lived fourteen hours, and died universally emphysematous. These cases (we do not mean such extreme instances as that just quoted) are, however, rare, and a stronger evidence of their infrequency could not be adduced, than that cited by Mr. Guthrie, which we believe to be correct as far as our own observation and intercourse with others enable us to judge, viz., that there is not a surgeon of any of the largest metropolitan hospitals "who has seen or recollects a case of recovery after a wound of the heart, either in his own practice, or in that of his colleagues or predecessors." We apprehend that surgeons in the "stabbing countries" have certainly the advantages of us in this department of our craft.

It is not difficult to conceive that the complex and dense structure of the ventricles may facilitate the closing of a wound of their walls; and thus we might anticipate, what appears to be the case, that wounds of the right ventricle would be more certainly and speedily fatal than those of the left, and, *a fortiori*, that wounds of the auricles are rarely, if ever, survived. The exalted organization of these parts would, moreover, hasten recovery, if the attendant inflammatory action can be kept under control. Surgeons should therefore be prepared, where such an injury is suspected, to meet the symptoms as they arise, and not to give up the case for lost, as appropriate treatment has unquestionably been of avail in promoting a successful issue.

We pass by several recent cases of 'wounds of the heart,' which are quoted at the commencement of the Eighth Lecture. The next subject which occupies our author's attention, is the employment of auscultation and percussion, in diagnosing the condition of the heart where injury is suspected. He enters somewhat at large into the physiology of the heart's action and sounds, and quotes the opinions and experiments of Drs. Hope and Latham, and others, on many points connected therewith. On the mooted question of the causes of the two sounds, he states it to be the received opinion in this country, that "the *first* or dull sound depends on the muscular contraction of the ventricles; and that the *second* arises from the closure of the sigmoid passive valves of the heart, by the recoil of blood, thrown back upon them by the aorta and the pulmonary artery." This is probably a sufficient account of the second sound; but most physiologists who have specially attended to the question, are disposed to admit the impulse of the heart against the ribs, the rush of blood through the constricted orifices of the aorta and pulmonary artery, and (possibly) the tightening of the auriculo-ventricular valves, as *adjuncts* in the production of the first sound.

Some observations on the sounds of the heart in disease follow, but we need not detain our readers by referring to them. In speaking of the inflammation which necessarily succeeds a wounded heart, where the patient survives long enough, Mr. Guthrie attributes the severity of the symptoms and commonly fatal result to the serous membranes being affected, rather than the substance of the heart,—and we believe correctly. He explains the apparent anomaly of but comparatively few cases being recorded by military surgeons, by stating, that sufficient attention could not be paid to them on the field of battle. He adds, however, a notice of the seven cases related by Baron Larrey, in his 'Clinique Chirurgicale,' which were the results of duels or attempted suicide, not of wounds

received in the field. Four of these recovered; but our author regards them as wounds of the pericardium, rather than of the heart,—being, in his opinion, "well attested cases of the secretion of the pericardium draining from its cavity for days, and a cure being effected by adhesion, in all probability, of the two secreting surfaces to each other." The result of these cases led Larrey to recommend an operation for the relief of hydrops pericardii, which is quoted in the work before us; as also is Skielderup's recommendation that the sternum should be trepanned to expose the pericardium. The treatment of these cases, as suggested by the author's experience, is comprised in few words, which we quote:

"All extraneous matters should be removed if possible, and all inflammatory symptoms should be subdued by general bleeding, by leeches, by calomel, antimony, and opium, &c. The chest should be examined daily by auscultation, and as far as may be by percussion. If the cavity of the pleura should fill with blood, it ought to be evacuated to give a chance for life, as in other cases of a similar nature, to be hereafter noticed, and if the pericardium should become permanently distended by fluid, it should be evacuated by the method of Harvey, which appears to be the best." (p. 59.)

A brief notice of lacerations and ruptures of the heart, from blows or other serious contusions, closes this lecture. Respecting these cases, our author notices the observation of Morgagni, as confirmed by Ollivier, viz. that spontaneous rupture of the left ventricle is of much more frequent occurrence than that of the right, presenting, in this respect, a relative proportion, the reverse of that which obtains where the lesion is the consequence of blow or contusion; a fact to be accounted for, we apprehend, by the greater muscularity of the one cavity, and the more feeble powers of resistance of the other.

The Ninth Lecture contains a variety of matter on the subject of penetrating, large, and direct incised wounds of the chest; on the appropriate method of treating them, and especially where the internal hemorrhage is considerable. We have only space to select a few scattered passages here and there; and to give, which we shall do by quotation, the general summary on these important points at the close of the lecture. Thus we find our author recommending that, in all cases of incised wounds of the chest, "the patient should, as a general rule, lie on the wounded part, if he can conveniently bear it;" and this, not for the purpose of draining off effused blood or other fluid, but, to "allow the pleura covering the lung to be as closely applied as may be to the pleura lining the wall of the chest, with the hope that the adhesive process may take place between those parts, and by this means cut off the wound from the general cavity of the pleura." (p. 63.) Again, we are warned against probing or otherwise interfering with large incised wounds of the chest, unless the cartilage or bone be injured. The important question of hemorrhage in cases of incised wounds admitting of being accurately closed, is discussed under three heads: those in which the loss of blood is trifling, where it is moderate, but still capable of absorption, and where it is sufficiently copious to be alarming. On this and other doubtful points associated with this particular part of our subject, we will, as we promised, allow the author to speak for himself: he thus expresses himself at the close of this lecture:

"1. All *incised or penetrating wounds* of the chest should be closed as quickly as

possible, by a continuous suture through the skin only, and a compress supported by adhesive plasters, the patient being afterwards placed on the wounded side.

"2. If blood flows freely from a small opening, the wound should be enlarged, so as to show whether it does or does not flow from within the cavity. If it evidently proceed from a vessel external to the cavity, that vessel must be secured by torsion or by ligature.

"3. If blood flow from within the chest, in a manner likely to endanger life, the wound should be instantly closed; but as the loss of a reasonable quantity of blood in such cases, say from two to three pounds, would be beneficial rather than otherwise, this closure may be delayed until syncope takes place, or until a further loss of blood appears unadvisable.

"4. If the wound in the chest have ceased to bleed, although a quantity of blood is manifestly effused into the cavity of the pleura, the wound may be left open, although covered for a few hours, if the effused or extravasated blood should seem likely to be evacuated from it, when aided by position; but as soon as this evacuation appears to have been effected or cannot be accomplished, the wound should be closed. It must be borne in mind that the extravasation which does take place is usually less than is generally supposed—a point which auscultation and percussion will hereafter in all probability disclose.

"5. If auscultation and percussion should indicate that the cavity of the pleura is full of blood, and the oppression of breathing and distress are so great as to place the life of the patient in immediate danger, the wound, although recent, should be reopened, as in cases 56, 57, and 72.

"6. As soon as the presence, even of a serous fluid, in the chest, is ascertained to be in sufficient quantity to compress the lung against the spine, and time has been allowed for the closure of the vessel from which blood originally flowed, a counter-opening should be made in the place of election for its evacuation by the trocar and canula, which may be afterwards enlarged, unless the reopening of the wound should be thought preferable, which will not be the case unless it should be low in the chest." (p. 71.)

We may observe that the above principles are illustrated by, and drawn from, cases which are quoted by the author, or which occurred in his own practice.

Lecture Tenth opens with the axiom that "*gun-shot wounds of the chest*, penetrating the cavity, are always exceedingly dangerous;" followed by a criticism on the traditionary assertion of the late Dr. Gregory, of Edinburgh, that, of twenty-six wounds of the thorax received at the battle of Quebec, two only proved fatal. We doubt not that Mr. Guthrie's explanation of this apparent anomaly (for such the experience of others makes it) is correct, viz. that these statistics must have been founded only on those cases which survived sufficiently long to be received into hospital some days after the battle, and which were, therefore, necessarily the less severe cases. Our author states that he lost one in three after the battle of Toulouse, that the mortality was much greater after the battle of Waterloo, and even still more serious in the army on the Sutlej. It is indeed a grave matter if much of this fatality is attributable to the insufficiency of medical assistance; and it is a melancholy commentary on the unjustifiable parsimony of a Government, which is so lavishly profuse in much needless, and criminal because needless, expenditure of the public money. Of one thing there can be no doubt; it is this, that the lack of hands to supply the necessary demand cannot *now* be said to constitute a reasonably apology for this inefficiency in the medical staff of the army.

Several points of interest are referred to in this lecture, which we have

only space enough to glance at. The circuitous course of balls around the chest, for instance, without penetrating its cavity, the author regards as a rare occurrence, and as attributable, when it does occur, to the foreign body being reflected off something solid which it cannot penetrate: this may be a button, a piece of money, or a rib. He says he has extracted many balls which were impacted between two ribs, and with successful results. As to enlarging gun-shot wounds, save for some sufficient object, such as the removal of some extraneous substance, or to ascertain whether a rib has been fractured, he objects to the practice as an unnecessary and mischievous interference. If a ball pass fairly through the chest and lung, the danger of death from hemorrhage is proportioned to the proximity of the wound to the root of the lung; and Mr. Guthrie adds that he who suffers from such a wound may consider himself fortunate if he has previously been the subject of inflammation of the chest, by which the adjacent surfaces of the pleura are glued together, and its cavity is thus prevented from communicating with the external air.

Many instructive cases are cited in illustration of these and other points of interest associated with these wounds; amongst which we find those of Sir Lowry Cole, Sir Andrew Barnard, the Duke of Richmond (then Lord March), Colonel Broke, &c. Many of these show that inflammation of the substance of the lung is not so severe as might be expected, after such serious injury as contusion and laceration of its texture to a considerable extent. Indeed, our author seems to regard the pleurisy and consequent effusion as involving greater risk to the patient, unless prompt and appropriate aid be afforded. The passage he has on this subject is of sufficient importance to deserve quotation, as the opportunity of profiting by and acting upon it, may, unfortunately, occur to any surgeon:

"In cases in which the external opening or wound does not communicate freely with the cavity of the chest, the principal danger arises from the inflammation of the pleura, ending in effusion, which, if not evacuated, leads to the loss of the individual. It is the great fact to be attended to in the treatment of pistol wounds of the chest, or those made by small balls which do not pass out. All the persons I have seen die from small balls, have died with the affected cavity more or less full of fluid. The *post-mortem* reports of all persons killed in England, in duels, by wounds through the chest, unwittingly attest this fact, as well as the insufficiency of the surgical treatment they received; and the necessity for the future for its amendment. It is in these cases that the stethoscope is most valuable; its constant use is indispensable whenever the respiratory murmur has been gradually yielding before the encroachment of the effused fluid, which, in the first instance, looks nearly like red coloured serum. When this murmur ceases to be heard, except at what is the upper part of the chest, whatever the position of the patient may be, it is full time to enlarge the original opening, or to draw off the fluid by the trocar and canula." (pp. 77-8.)

The restorative powers of nature are exhibited in the very marvellous case which closes this lecture. It is that of a bombardier of the Royal Artillery, who was struck by a two-pound shot, which made a clear breach through his right side (of the chest, we presume), indeed, so clear, that General (then Captain) Macdonald saw the light quite through him, as he was led up to him! Yet this man recovered. This is even more wonderful than the notorious shaft case, the preparation belonging to which is in the museum of the Royal College of Surgeons.

Some cases are given at the commencement of the Eleventh Lecture, to exemplify the nature of the appearances usually met with after death, in gun-shot wounds of the chest when not followed by empyema. Sometimes small abscesses were found in the lung; in other cases this organ was soft and gangrenous, or its texture filled with a sero-purulent fluid. Instances are also quoted, in which the ball remained in the chest, even for years, without creating much disturbance, portions of wadding or clothing being expectorated from time to time. Our author deprecates what has been recommended by some surgeons, viz. the drawing a piece of cambric through the chest for the purpose of removing splinters of bone or other foreign bodies from the lung. It sets one's teeth on edge to think of such a procedure, though it probably would not be attended by much pain. He admits, however, the propriety of removing such extraneous objects as may be within reach of the finger; and for this purpose, as before stated, the external wound may be slightly enlarged, but all probings with "small, sharp-pointed instruments should be avoided." As to the expectoration of portions of clothing, wadding, &c., Mr. Guthrie states that he has seen many instances; but never one, as some say they have, in which a ball was coughed up. When any such foreign body gets into the cavity of the pleura, the result, in his experience, is usually fatal.

The consequences of traumatic inflammation of the chest occupy the early part of the Twelfth Lecture; and this leads to a discussion as to the most approved method of relieving the pleura of the effused fluid, whether serum or pus, which is compressing the lung against the spine; a subject we have already adverted to at an earlier part of this article. Mr. Guthrie remarks, that a desire has been manifested by surgeons of experience in these injuries, to have as dependent an opening as possible, to allow of the more ready draining of the cavity of the pleura; and with a view to establishing this practice on a sound basis, he instituted a series of experiments and dissections at the College of Surgeons. The result of these we give in the author's words:

"1. A trocar and canula pushed in between the eleventh and twelfth ribs, in a diagonal direction upwards, and on a line with the angle of the ribs generally, will in the *dead body* invariably enter the cavity of the chest without injuring the diaphragm.

"2. The same operation performed on the *living body* would, in all probability, if done at the moment of expiration, first enter the thorax, then pierce the diaphragm, and thus open into the cavity of the abdomen,—a difference in result to be explained by reference to the anatomy and physiology of the parts concerned; and showing that the operation, when required on man, should always be done by incision, and not by puncture with the trocar and canula." (p. 94.)

A careful review of the anatomy of the chest likely to be concerned in the operation follows; and then the mode of proceeding recommended by the author to attain the desideratum of a low opening is given, which we abridge from the detailed description in the text. The spot selected is the interval between the eleventh and twelfth ribs, to reach which an incision, three inches in length, is to be made, commencing two inches from between the spinous processes of the last two dorsal vertebræ. The latissimus dorsi and serratus inferior posticus are then to be divided, and

the external intercostal muscle thus exposed; a director is next to be introduced beneath the last-mentioned muscle, which, together with any portion of the internal intercostal that may present itself, is to be divided. The pleura is thus laid bare, and, if distended with fluid, will be found to bulge. A puncture is then to be made, the patient being directed to *inspire* at the time to prevent injury to the diaphragm; and when the presence of fluid is positively ascertained, this may be enlarged to an incision for the introduction of the finger or any instrument which may be required.

The remainder of this lecture is occupied with wounds of the diaphragm, and several illustrative cases are quoted. These show that, though such injuries usually prove fatal, and speedily so, they are not necessarily incompatible with an extension of life, and that without any very marked inconvenience, for a lengthened period. Mr. Guthrie has pointed out that wounds and lacerations of this important muscle do not heal, but become permanent, involving, therefore, as a necessary consequence, a hernial protrusion of the abdominal, as the less fixed, viscera, into the chest. These injuries are not of very rare occurrence in civil practice; it occurred to us, not long since, to witness the accident and examine the body of an aged man who was the subject of extensive rupture of the diaphragm, from the passage of a heavy waggon-wheel over the trunk. Death was almost instantaneous, and the stomach and a considerable portion of intestine had passed into the chest.

The subjects treated of in the last lecture are, protrusion of the lung, wounds of the intercostal and mammary arteries, and wounds of the head and neck. To each of these we shall devote a brief space before we conclude.

Mr. Guthrie informs us that he has seen many cases of protruded lung, the protrusion usually occurring during *expiration*, but in almost all it readily admitted of return into its natural cavity. When the opening is small, and the protruded viscus is held more or less firmly, he recommends that it should be left (unless actually strangulated, which is rare) to contract adhesion to the costal pleura; the risk of severe inflammation, he has ascertained from experience, is thus diminished. The extended portion, when thus circumstanced, loses its brilliancy and quickly shrinks, and it may then be removed without interference with its newly-formed connexions to the aperture by which it made its exit. This is a valuable practical hint.

Introducing the subject of wounds of the intercostal and internal mammary arteries, our author remarks that bleeding from these vessels is rare, and that the inventions of theorists to prevent such bleeding "are more numerous than the cases requiring them." To have been thus assured would, to use Mr. Guthrie's expression, have been "consolatory" to us some years gone by, when we remember that this, during our early pupillary responsibilities at the hospital, constituted, amid sundry other imaginary casualties, one of those calculated to tax the ingenuity, and try the presence of mind, of the uninitiated. How much do the realities of surgery, as of other things in life when experienced, vary from the preconceived notions formed of them, especially by the sensitive and imaginative mind. But to return to matters of fact. When an internal mammary artery is wounded and bleeding, we are directed to enlarge the

wound, search for the vessel, and tie or twist it *if practicable;* if not, the wound is to be closed and the result awaited. A similar course is to be pursued in the case of a bleeding intercostal artery. Mr. Guthrie says, "I have had occasion to twist and bruise the end of an artery bleeding in an intercostal space, and I have tied a vessel under the edge of a rib; but I have not met with any of the great difficulties usually said to be experienced in suppressing a hemorrhage from this vessel, when the wound was recent, and the parts were sound." (p. 104.) So much for this bugbear.

The concluding subject is "wounds of the head and neck," to which a brief notice is devoted in this work of our author. There are some good practical directions for the treatment of wounds of the neck in attempted suicide, &c., but only such as are known and acted upon by surgeons generally, who are in the habit of seeing and treating these cases. Some curious instances of the lodgement of balls and other foreign bodies in the maxillary sinus and neighbouring parts of the face are referred to; wounds of the jaws, involving more or less loss of their substance, and injuries to the eye, close the volume; and a few parting remarks will bring our review of the same likewise to a conclusion.

Of the general merits of the work we need not repeat our impression: the extended and favorable notice we have taken of its contents will have proved the sincerity of our good opinion. It will be observed that we have made our review, as we promised, as simply analytical as possible; indeed, we fairly admit that we have found but little food for criticism in the practice inculcated. The author has certainly proved himself an able advocate for the use of the stethoscope, and practically familiar with its value, as far as we can form a judgment. He is also not far behind the day in many of those improvements and rapid advances of some of the collateral branches of the profession, which have left so many of his contemporaries, of less active and vigorous mind than himself, far in the rear.

There is abundant evidence of the author's entire confidence in the justness of his own views, as opposed to those of others; and this is not infrequently expressed in a way which is not consistent, to say the least of it, with good taste, and which renders him justly obnoxious to the charge of vanity and egotism. Another habit strikes us as rather strange, though it adds to the interest of many of the cases which are cited, viz., the introduction of the names, at full length, of several officers, some of whom are still living, together with many particulars connected with their cases, which we should have thought they would rather not have had thus paraded before the public. We may especially notice the case of Lord B——, which is given in a very extended way, and where the injury was evidently received in a duel. But these are comparatively venial faults; and the latter remarks may be rendered inapplicable by the permission of the subjects of the cases having been obtained to insert their names.

We consider that the profession is indebted to Mr. Guthrie for this addition to his already published experience in military surgery; and, albeit we trust that they will not speedily have occasion to test the correctness of the principles inculcated, we cordially recommend these lectures to the attentive perusal and study of our readers.

ART. III.

On Poisons, in relation to Medical Jurisprudence and Medicine. By ALFRED S. TAYLOR, F.R.S., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital.—*London*, 1848. Fcap. 8vo, pp. 855.

HAVING in our former Article (pp. 172-201) discussed the subjects of the first three Chapters of Mr. Taylor's elaborate work, we now proceed to comment on Chapters IV and V, in which our author treats of the general diagnosis of poisoning. Alluding to the importance of this part of the subject to the medical practitioner, he remarks that—

"By mistaking the symptoms produced by a poison for those arising from natural disease, he may omit to employ those remedial measures which have been found efficacious in counteracting its effects, and thus lead to the certain death of the patient." (p. 38.)

The truth of this observation, and the serious consequences which may take place through such a mistake, cannot be too strongly enforced; for, in these times, when the conduct and skill of the medical attendant are so frequently called into question,—when the misdirected zeal of coroners, and the unfeeling tendency which exists in the minds of both advocates and judges narrowly to scrutinise, and too often discourteously to censure, the proceedings of medical men,—are taken into consideration, it must be obvious that an unskillful practitioner is very likely, under such circumstances, not only to become the scapegoat of a villain, but, at the same time, to be placed in the unenviable predicament of being looked upon as the only person connected with the case, on whose shoulders the whole of the blame ought to be cast. Again, as a correct knowledge of the symptoms of poisoning furnishes the medical jurist with the chief and most valuable part of his evidence, he ought at all times to be upon the alert to recognise them, and constantly prepared to give them a correct interpretation.

To this end we have taken the trouble to separate some of the leading points of these chapters, and to put them before our readers in an order somewhat different from that used by Mr. Taylor.

Among the distinctive evidences of poisoning, he refers to the following:

1. *The symptoms usually appear suddenly, soon after a meal, and often while the individual is in perfect health.* This is especially the case with the corrosive poisons, with the irritants, and with prussic acid, oxalic acid, strychnia, &c. Upon this point Mr. Taylor remarks:—

"It is rare that the appearance of the symptoms is protracted for two hours, except under certain peculiar states of the system. It is said that some narcotico-irritant poisons, such as the poisonous mushrooms, may remain in the stomach twelve or twenty-four hours without giving rise to symptoms; and this is also affirmed to be the case with some animal irritants, such as decayed meat; but with regard to the first point, it has been shown by Dr. Peddie, that mushrooms may produce symptoms in half an hour; and a case has fallen under my own observation, where the symptoms from noxious food came on within as short a time after the meal as is commonly observed in irritant poisoning by mineral substances. In cases of poisoning by phosphorus, the symptoms do not commonly begin until after the lapse of many hours." (p. 39.)

And on turning to the body of his work we find it stated, with reference to opium, that—

"These symptoms usually *commence* in from *half an hour* to *an hour* after the poison has been swallowed. Sometimes they come on in a few minutes, especially in young children; and at others their appearance is protracted for a long time." (p. 582.)

It must be remembered, however, that there are many circumstances which have an influence upon the time at which the symptoms set in; thus, they generally come on more quickly when the poison has been given in a state of solution, when the stomach is nearly empty, or when the vital powers of the individual are prostrated by fatigue or disease. On the other hand, the action of a poison will be retarded if it be administered in a solid form, when the stomach is full, when the patient is in a state of intoxication, or when he has been habituated to the influence of the drug. It has been observed, also, that during sleep the action of a poison is delayed. Dr. Christison has noticed this with respect to arsenic; and a case has been recorded by Dr. Letheby, in which the effects of about half an ounce of nitre were most probably warded off by the same influence. (Pharm. Journ., vol. vii.) Again, it happens in chronic, or slow poisoning, that the symptoms may be prolonged for a considerable period, and may not assume any active character until very late in the history of the case; under these circumstances, they frequently baffle the scrutiny of the medical attendant. Mr. Taylor refers to a very instructive case of this sort:—

"In November, 1846, a case was referred to me for investigation, in which it was alleged that a farmer in one of the midland counties had been poisoned two years before by his housekeeper, who was a respectable person, and most attentive to him as a nurse during his illness. He had been attacked with vomiting and other signs of gastric disorder, about three months before his death, but recovered under medical treatment. About eight days before death, the symptoms recurred with greater violence than ever, and he sunk under them. They were referred to ulceration of the stomach, so closely did they resemble those of disease. As there was no suspicion of poison, the body was not examined; and nothing would have been known respecting the real cause of death, but for a statement made two years afterwards by the housekeeper, that she had on two occasions administered to her master small doses of arsenic." (p. 39.)

A case of a very similar description came under our notice last winter. An old woman of dissipated habits, had been the subject of repeated attacks of vomiting and purging. She had been attended at various times by five or six medical men, not one of whom suspected that she was suffering from the effects of poison. During one of these attacks she expired; and then the post-mortem examination and the analysis of the tissues revealed the fact, that she had both suffered and died from the effects of arsenic. A similar case has still more recently occurred in Bristol.

Lead, mercury, and copper, also, are poisons which may operate slowly and insidiously on the animal body; often procrastinating their more evident effects to a very distant period. This, in fact, has been frequently observed among persons whose occupations expose them to the slow and constant action of these poisons.

Again, Mr. Taylor refers to a class of poisons termed *accumulative*, which may be administered at intervals for some time in small doses without producing any marked effects; but which appear to accumulate in the body, their power becoming manifested at some unexpected time with sudden and violent energy. To this class belong digitalis, and perhaps opium,

prussic acid, and, as some suppose, arsenic. This peculiar mode of action, however, is but little understood; it is not very frequent, and, as our author states, is more likely to be witnessed in medical practice, than in cases of attempts to poison. Still the facts connected with it are of some importance to the medical jurist, and may seriously affect the value of his evidence in a case of chronic poisoning. Mr. Taylor has, therefore, discussed these facts in various parts of his work, as at pages 39, 113, 317, 595, 657, and 827.

2. Another means whereby the practitioner is enabled to recognise a case of poisoning, is *by attending to the symptoms;* for these are generally well marked, and we can often recognise in them the local as well as the constitutional effects of the poison. The former take place in general quickly; and are characterised by pain and heat along the alimentary tract, by more or less thirst, by vomiting, and by purging. The latter come on more slowly, and are known by the alterations which they produce in the condition of the mind, the sensations, the motor functions, the circulation, and the respiration. In illustration of both of these classes of effects we shall quote from the work of Mr. Taylor:—

"The Irritants are possessed of these common characters. When taken in ordinary doses, they occasion speedily violent vomiting and purging. These symptoms are either accompanied or followed by intense pain in the abdomen. The peculiar effects of the poison are manifested chiefly on the stomach and intestines, which, as their name implies, they irritate and inflame. Many substances, belonging to this class of poisons possess corrosive properties, such as the strong mineral acids, caustic alkalies, bromine, corrosive sublimate, and others. These, in the act of swallowing, are commonly accompanied by an acrid or burning taste, extending from the mouth down the œsophagus to the stomach." (p. 33.)

"Narcotic poisons have their operation confined to the brain and spinal marrow. Either immediately or some time after the poison has been swallowed, the patient suffers from cephalalgia, vertigo, paralysis, coma, and, in some instances, tetanus. They have no acrid burning taste, like the corrosive irritants, and they very rarely give rise to vomiting or diarrhœa." (p. 34.)

"Narcotico-irritants. Poisons belonging to this class have, as the name implies, a compound action. They are chiefly derived from the vegetable kingdom. At variable periods, after being swallowed, they give rise to vomiting and diarrhœa like irritants; and sooner or later produce stupor, coma, paralysis, and convulsions, owing to their effect on the brain and spinal marrow. They possess the property, like irritants, of irritating and inflaming the alimentary canal. As familiar examples, we may point to nux vomica, monkshood, and poisonous mushrooms. This class of poisons is very numerous, embracing a large variety of well-known vegetable substances; but they rarely form a subject of difficulty to the medical practitioner. The fact of the symptoms occurring after a meal, at which some suspicious vegetables may have been eaten, coupled with the nature of the symptoms themselves, will commonly indicate the class to which the poison belongs." (p. 35.)

There are a few other means whereby a practitioner may be enabled to recognise a case of poisoning; as by inquiring, *when several persons have partaken of the same food, whether all suffer in the same way. Whether any domestic animal has eaten from it, and is poisoned thereby. Whether poison can be detected in the food taken, or in the matters vomited.* All these may furnish valuable hints to the medical attendant, and especially to the medical jurist; we will quote a case in illustration of one of them.

"An intelligent barrister related to me the following case which he was engaged

in prosecuting on the Western Circuit some years since. A woman poisoned her husband with arsenic mixed in soup; and after the deceased had made a full meal, she threw the remainder out of a window into a farm-yard, thereby thinking to defeat all attempts at discovery of the means which she had adopted to destroy her husband. It happened at the time, that a pig and several fowls were feeding under the window, and they ate up what fell on the ground. The whole of these animals died under symptoms of irritant poisoning. The husband also died: no poison was detected in the stomach, although there were the traces of its action; but on opening the bodies of the animals, the medical witnesses found not only the appearances usually produced by irritant poison, but arsenic itself was readily discovered in the viscera. This sort of evidence supplied that which was required to complete the case: for while no poison was detected in the body, no portion of the poisoned soup could be procured. The prisoner was convicted and executed." (p. 162.)

Our author refers to the fact, that various articles of food may become poisoned through carelessness or ignorance, as by the employment of unclean copper vessels, by the use of water contaminated with lead, or by the practice of colouring confectionery by means of poisonous pigments. The detection of these sources of mischief is a matter of the greatest importance; for it may be the means not only of saving the lives of all who are exposed to the danger, but of exculpating an innocent person from the very grave charge of murder.

Again at page 78 Mr. Taylor states that—

"A diagnosis may often be obtained by observing the colour of the matter first vomited. In poisoning by sulphuric acid, it is black; by nitric acid, brown, or yellow, or greenish; the same in poisoning by muriatic acid: in the case of oxalic acid it is of a coffee colour, or of a dark greenish-brown. In all these cases the liquid is acid, and acts upon organic colouring matter, e. g. the dyes of dresses. In alkaline poisoning there is a strong alkaline reaction in the vomited liquids. In poisoning by the salts of copper, the matter vomited is of a greenish or bluish colour. Some substances are at once denoted by the odour; as prussic acid, oil of bitter almonds, ammonia, opium, and alcohol. In other instances, the poison can be detected only by chemical processes, and for the ready application of tests an extensive acquaintance with practical toxicology is required." (p. 78.)

In Chapter V, Mr. Taylor gives a very complete account of the diseases which are likely to be confounded with poisoning. As this subject, also, is of vast importance both to the practitioner and medical jurist, we shall give it a proportionally full consideration; endeavouring, so far as we can, to supply the few deficiencies which Mr. Taylor has left.

"The diseases," he says, "the symptoms of which resemble those produced by irritant poisons, are cholera, gastritis, enteritis, gastro-enteritis, peritonitis, perforation of the stomach or intestines, strangulated hernia, colic, and hæmatemesis." (p. 54.)

And we may add to these, the rupture of an abdominal viscus, and death from hemorrhage into the peritoneal cavity; for the symptoms attendant upon such casualties are much more nearly allied to the actions of an irritant poison, than to those of a narcotic, under which Mr. Taylor has classified them.

Many acquittals on criminal charges have taken place, in consequence of the great difficulty which exists in distinguishing the common *English Cholera* from arsenical poisoning; and, in truth, if we were to rely upon

the symptoms alone, the difficulties would be always too great to establish the proofs as clearly as the law demands for a conviction. The points which are, in Mr. Taylor's opinion, to be specially attended to, are the following:—In irritant poisoning the alvine evacuations are often tinged with blood, rarely watery and gruel-like, and on analysis they will generally yield traces of the poison; the urine is particularly well adapted for such an examination; the attack is not dependent on the season of the year, or on any epidemic influence; nor can it be traced to any errors in diet. If the case prove mortal, death generally occurs in twenty-four hours. English cholera is not commonly fatal; and when it is, the attack lasts three or four days or more. Again, in arsenical poisoning there is usually an appearance of certain peculiar symptoms which are never observed in cholera, such as itching and redness of the conjunctivæ, a great sensibility of the skin, and occasionally paralysis and coma.

Rupture of the gall-bladder, of the spleen, the liver, the uterus, or its appendages, will suddenly give rise to alarming symptoms, the occurrence of which, in a person previously healthy, may occasion a great deal of difficulty in the diagnosis. Death, in such cases, may take place from peritonitis, or from internal hemorrhage. Mr. Taylor has not referred to many cases in illustration of this class of accidents, notwithstanding that instances of it are comparatively frequent. A case of death from rupture of the gall-bladder occurred in our practice about two years since. We were called upon to make a post-mortem examination of the body of a man, whose sudden death and previous healthy condition had given rise to a suspicion of his having been poisoned; he was attacked suddenly with great pain in the right side, and severe vomiting; extreme collapse soon followed, and he died after an illness of fifty-two hours. It was found, on examination, that death had resulted from the effects of a ruptured gall-bladder, in the duct of which three calculi were firmly impacted.

Numerous instances are on record, in which death has been the consequence of the rupture of a fallopian tube. The symptoms attendant upon cases of this description closely resemble those caused by the administration of an irritant poison. Three of these cases have been reported by Dr. Letheby (Pharm. Times, vol. i), two by Dr. Oldham (Guy's Hospital Reports, 1845), and one by Mr. Allport (Lancet, 1845): all of them were characterised by a sudden accession of pain, by constant vomiting, great thirst, rapid prostration of strength, extreme collapse, and death; the whole period of the attack not having continued longer in any of them than twenty-four hours. Again, Dr. Munk has recorded a curious instance, in which death arose from the giving-way of a fallopian tube, which had been over-distended by the retained catamenia; in this case, also, the symptoms were somewhat similar to those produced by an irritant poison.

Others of the abdominal organs may also give way, so as to occasion death by the escape of blood; thus, in the 'Medical Gazette' for October, 1842, Dr. Neile has reported a case of death from spontaneous rupture of the spleen; and a similar accident is recorded by Dr. Salter in the January number for 1846. In both of these cases the accession of pain was sudden, the collapse and sickness great, and the death very rapid.

We can, therefore, easily imagine how a practitioner may be embarrassed by such cases; but he will find that a little attention to the mode of

attack, the nature of the vomited matters, the sensitiveness of the abdomen, the extreme prostration of the vital powers, accompanied by frequent syncope, by a pale, and almost bloodless countenance, and by the total absence of any erratic condition of the mental, sensorial, or motor functions, will generally be sufficient to enable him to distinguish the true nature of the case.

The diseases which simulate narcotic poisoning are apoplexy, epilepsy, disease of the brain, heart, and blood-vessels.

Apoplexy of the brain is a condition of which the symptoms are really produced by many of the narcotic poisons; and it is extremely difficult to distinguish between the apoplexy caused by poison, and that which arises from disease. Mr. Taylor says the remembrance of the following circumstances may help us in our diagnosis:—

"1. Apoplexy, as a disease, is sometimes preceded by warning symptoms before the fatal attack comes on. In poisoning, such symptoms would be wanting, unless the poison were administered to a person who had already been threatened with apoplexy. 2. Apoplexy, as a disease, does not commonly attack persons under the age of thirty. The fatal cases increase progressively with the age, and, according to the research of Dr. Burrows, the disease is most common between the ages of sixty and seventy. We shall presently see that there are, however, exceptions to this rule. Poisoning may be witnessed in a person at any age. 3. The relation between the time of the attack and the time at which food or medicine was taken. Thus, if the comatose symptoms do not come on until five or six hours after some liquid or solid has been swallowed, they are much more likely to be dependent on apoplexy from disease than on poison. This is a most important character; but its occurrence is of course purely accidental, for it is by no means unusual that an attack of apoplexy should speedily follow a meal made by a previously healthy person. 4. In apoplexy from disease, it is usually observed that coma is at once induced; but in poisoning, coma comes on slowly, and is generally preceded by vertigo and stupor. 5. The discovery of poison in the food taken, or in the contents of the stomach; this would at once establish the fact of poisoning. 6. The discovery of appearances in the brain indicative of apoplexy, such as effusion of blood; this would negative, *cæteris paribus*, the presumption of poisoning." (p. 60.)

Mr. Taylor has brought together cases which show that attacks of apoplexy may take place even in children. He says:

"I have known a child between two and three years of age, die from congestive apoplexy, and the disease has been observed to occur even in infants." (p. 63.)

And he quotes a case of Dr. Campbell's, in which it occurred to a child eleven days old.

The *tetanus* which arises from disease, or from irritation propagated to the nervous centres, is not very likely to be confounded with the tetanic convulsions produced by certain narcotico-irritant poisons, as those belonging to the nux-vomica tribe; for, as Mr. Taylor states, "idiopathic tetanus is by no means common;" but the disease is generally to be traced to exposure to cold and wet, or to some wound or injury done to the tendinous or nervous structures. A form of tetanus *(trismus* or *tetanus nascentium)* has been observed to arise spontaneously in very young children, attacking them in the course of the first week after birth. Mr. Curling says that it has scarcely ever been observed to supervene later than a fortnight. This form of the disease is nearly always fatal, carrying off the child in a period of time which varies from twenty-four

to seventy-two hours. For many years past, it has been an exceedingly rare disease in this country, Dr. Francis Ramsbotham never having seen a case of it: nevertheless, it seems to have been very common and very fatal in Dublin about the end of the last century; for Dr. Joseph Clarke states, that at the close of the year 1782, it was found that this malady killed seventeen per cent., or nearly one in six, of all the children born in the Lying-in Hospital of that city. The symptoms which accompany tetanus, generally arise in from three to eight days after exposure to cold, or after the receipt of an injury; they progress very gradually, and death rarely takes place in less than twenty-four hours; indeed Mr. Curling mentions only three cases out of 128, in which death occurred in a shorter period than this. On the other hand, in cases of poisoning attended by convulsions and tetanic spasm, the symptoms come on very quickly, the tetanic spasm intermits, and death commonly takes place in an hour or two. Mr. Taylor states that the diagnosis will generally rest upon the following circumstances:—

"The period of time which has elapsed since any substance, liquid or solid, was swallowed by the patient. 2. The gradual or sudden and violent accession of symptoms,—the latter indicating poisoning. 3. The duration of the case. Tetanus as a result of injuries, rarely proves fatal in less than twenty-four hours; and in the idiopathic form, it seldom destroys life in less than three or four days. In tetanus produced by the compounds of strychnia, given in fatal doses, the person rarely survives two hours. 4. The discovery of nux vomica, strychnia, or other poison in the food, in the matter vomited, or in the contents of the stomach after death. Prussic acid gives rise occasionally to tetanic convulsions, manifested by opisthotonos, emprosthotonos, and general spasm of the trunk and extremities; but the very sudden invasion of symptoms after a liquid has been taken, the rapid termination of the case, and the presence of the poison in the stomach will render a diagnosis easy." (p. 65.)

Convulsions are a very frequent cause of death among children; and they may either arise from the action of poisons, especially those belonging to the narcotic class, or from natural causes. Our author remarks that—

"Convulsions are a very common effect of the action of overdoses of opium on young children, and they are not to be distinguished from those which arise from natural causes. During the fit, the eyes are distorted, the pupils contracted or dilated. The spasms may affect the organs of respiration; the jaws are closed, and saliva in a frothy state escapes at the mouth. There may be also stertorous breathing; and, from impeded respiration, the tongue, face, and the surface of the skin will become livid, owing to imperfect aeration of the blood, and the child may die asphyxiated. Under prompt and appropriate treatment, except when it depends on poison unsuspected, the attack may be alleviated, and the child recover." (p. 67.)

At page 61, Mr. Taylor gives an account of a case in which the medical practitioner was deceived; believing that the child laboured under an attack of convulsions, when it was really suffering from the effects of opium. We believe that such cases occur very frequently; for in the course of our inquiries into numerous cases of poisoning, we have been surprised at the great mortality among the infant population; and we have no doubt that this partly arises from the common practice of administering laudanum to children. It is a fact that in many parts of England it is customary for the parents to dose their children before they are sent to school, or in the event of their being left at home, in order that they may sleep, and give no trouble. During the trial of Sarah Scarborough, at the last Lent

assizes at Cambridge, it was given in evidence by one of the druggists of March, that he was in the habit of selling, in pennyworths, as much as two pints of laudanum weekly; and he admitted that other druggists sold more than he did, all of whom knew very well that it was used to drug the children. We believe the practice to be peculiarly prevalent in the north of England, where laudanum is popularly designated by the significant term "quietness."

The giving way of an aneurismal tumour, or the rupture of a blood-vessel, has frequently occasioned symptoms which have been mistaken for poisoning. Cases of this kind may occur under very curious and unexpected circumstances; for example, there is in the museum of the London Hospital a preparation of a rent aneurismal sac, occurring upon a branch of the superior mesenteric artery; the case happened in the practice of Dr. Barnett, of Limehouse, and until the post-mortem examination was made, he was fully impressed with the idea that his patient had been poisoned. Again in Dr. Bennett's report in the 'British and Foreign Medical Review,' for July, 1845, a case is quoted from Dr. F. Flogel, in which a soldier while on parade fell from his horse and died suddenly; all the organs of the body were sound, except the liver. In the posterior mediastinum were found two pounds of blood, which had escaped from a rent in the vena azygos. And we might quote many other instances of sudden death from the bursting of a blood-vessel or aneurismal tumour, in which the nature of the cases was not suspected during life, and where the symptoms were very similar to those which arise from the action of prussic acid. In all such cases, however, the post-mortem examination will always reveal the true cause of death.—We must refer our readers to Chapter XIII of Mr. Taylor's work, for a further consideration of several points which bear upon this part of the subject.

Chapters VI and VII are devoted to *the consideration of the general treatment of poisoning;* and while we are ready to acknowledge the great value of Mr. Taylor's observations upon this part of his subject, we are compelled to remark that here, as in other parts of the work, the method and style adopted by our author seem to us capable of considerable improvement; for the former will hardly ever permit us to gain a complete view of the subject, or to make any practical application of the principles which the author professes to teach; whilst by reason of errors in the latter, he is continually diverting the attention of the reader from the main points of his argument. We shall, therefore, in making our selections from his best passages, put them in the order which we consider to be most suited to the interests of our readers.

Mr. Taylor does not believe in the existence of any specific or universal remedy for cases of poisoning; but he states that the treatment must be varied according to the kind of poison; and that it is advisable to obtain, if possible, some previous knowledge of the nature of the substance taken, and then to administer the remedies which have been found most useful in that particular kind of case.

Certain general principles, however, are always to be kept in view. Thus it is our duty, 1st, *to effect the removal of the poison from the stomach as soon as possible;* and 2dly, *to neutralize, or diminish its action by the employment of an antidote.*

The first of these objects may be accomplished either by the administration

of an emetic, or by the use of the stomach-pump. Mr. Taylor regards this as the most important part of the treatment, saying:

"This is the great point to which we must look for the safety of the patient. All other treatment, even that of antidotes, must be subordinate to this. So long as any portion of the poison remains in the stomach, the patient is not safe; and if we delay the employment of means to remove it, he will die, although the stomach may be subsequently entirely cleared of the poison." (p. 79.)

And again he says:

"It has sometimes happened that the party first called to see the patient, has been too much occupied in seeking for or administering antidotes, and has neglected to empty the stomach, which is the first, and by far the most important, consideration in practice." (p. 79.)

We think, however, that this advice is both too general, and too strong; for there are many cases of poisoning in which the patient would be sacrificed, if the medical attendant were to wait for the evacuation of the contents of the stomach; let us instance those from the mineral acids, corrosive sublimate, oxalic acid, prussic acid, liquor potassæ, or its carbonates, &c. Here it is obviously the duty of the practitioner to administer an antidote as quickly as possible, and then he may proceed to empty the stomach of its contents.

The stomach-pump is a most useful instrument, effecting the complete removal of the poison, and admitting of the introduction of antidotes. Its employment, however, requires care; for there are instances in which the patient has been suffocated by the passage of the tube into the trachea, instead of into the œsophagus. We remember to have seen a case of what was supposed to be purulent infiltration of the lung; it proved to be an infiltration of mustard and water, which had been injected down the wrong passage. To guard against the occurrence of such a disaster, the tube should be thrust as far back into the mouth as possible, and directed along the roof and posterior part of the pharynx.

The cases of poisoning in which the stomach-pump will be most useful, are those arising from the influence of narcotics and narcotico-irritants; in these the patient is frequently insensible, and consequently, without aid from such an instrument, we should not have the means either of removing the poison, or of administering an antidote.

"There are some cases in which it is advisable not to employ the stomach-pump, but to trust to vomiting. I allude to those instances in which the poison taken has been a mineral acid, or caustic alkali, bromine, iodine, or, in short, a corrosive substance of any kind whatever. In poisoning by any of the corrosives, those parts of the body which come in contact with the substance, are chemically acted on and destroyed. The attempt to pass an instrument in such a case, might lead to the laceration of the softened membranes, and, in some instances, to the perforation of the œsophagus or stomach. Such cases may be easily distinguished from others; for, on opening the mouth, the chemical action of the poison may be plainly seen on the teeth, gums, tongue, and back of the throat." (p. 80.)

Mr. Taylor has taken, we think, a very limited view of the nature and power of *antidotes;* for he restricts the term to substances which are capable of exerting a chemical influence upon a poison. His words are: "The so-called antidotes act upon the poison, and not upon the body; their action is, therefore, strictly of a chemical nature." (p. 73.)

This definition shuts out a great class of remedies, the beneficial

operation of which depends either on a physical or physiological action. For example, the exhibition of charcoal powder has been found to be of use in most cases of poisoning; vinegar, infusion of coffee, or green tea, have each been administered, with the greatest advantage, in cases of poisoning by opium and alcohol; brandy and ammonia have roused patients, and saved them from the effects of prussic acid; and diuretics have been given, in cases of poisoning by arsenic, with the most beneficial results. Yet, as none of these substances can be said to exert a chemical influence over their respective poisons, they cannot, according to Mr. Taylor, be admitted into the class of bodies called antidotes. Again, according to his definition, there are many poisons, such as opium, strychnia, cantharides, and in fact the whole tribe of narcotico-irritants, which have no antidotes whatever; inasmuch as there are no remedial substances which are capable of exerting a chemical influence over them. We, however, have always been accustomed to take a much broader view of this question, and to consider an antidote to be really what the name implies; that which is given against a poison. We think, moreover, that the various antidotes may be very conveniently classified under three heads: 1, Chemical; 2, Physical; and, 3, Physiological antidotes. The first of these owe their action to a chemical influence which they exert over the poison, changing it thereby into an insoluble, or almost inert compound. Under this head we could include most of Mr. Taylor's antidotes, such as chalk, magnesia, soap and water, &c., which may be used as antidotes to the mineral acids; vinegar, lemon-juice, and tartaric acid, which may be administered in cases of poisoning by the caustic alkalies, or their carbonates; alkaline and magnesian sulphates, either of which may be given to neutralise the salts of lead or baryta; white of egg, flour and water, and milk, which are chemical antidotes to corrosive sublimate, nitrate of silver, sulphate of copper, chloride of tin, and most of the corrosive mineral poisons.

Mr. Taylor very properly cautions the toxicologist against the indiscriminate use of chemical antidotes, by observing that—

"In rendering a poison insoluble by the administration of an antidote, it is necessary to bear in mind that the substance used for this purpose should be itself inert, or we shall be only substituting one poison for another." (p. 74.)

By not attending to this point, great danger may arise. For example;—it was formerly the custom to give a solution of sulphuret of potassium in cases of poisoning by arsenic:—it was known that the sulphuret of arsenic was *insoluble;* hence it was rather hastily inferred that it was *inert*. We must now look upon it as a sort of miracle, that persons thus treated ever recovered. The alleged antidote itself, the sulphuret of potassium, is a poison; so that if the patient did not die from the poison originally swallowed, his life was endangered either by the sulphuret of arsenic resulting from the action of the antidote, or by the antidote itself.

Again, our author points to the fact that the administration of a chemical antidote is not sufficient to put the patient completely out of danger, or to warrant the practitioner in relaxing his efforts to get rid of the poison; for although it may have formed a compound which is insoluble in water, yet it does not follow that it should be insoluble in the gastric juice, and we here agree with his remark, that "nothing but the complete expulsion of the substance from the body can give security to the patient, or hope of success to the medical attendant." Mr. Taylor has not alluded to a very

great error which is often committed by toxicologists; who frequently imagine that when they have found out a means of making a poison insoluble, they have thereby discovered its antidote; very little attention being directed to the cost of the remedy, to the readiness with which it may be procured, or to the time which there may be for the operation of its antidotal powers; and hence we meet with such absurdities as the following: a proposition for the use of the mixed oxides of iron in cases of poisoning by prussic acid; or for the use of gluten, a mixture of gold dust and iron filings, the protosulphuret of iron, and protochloride of tin, as antidotes for corrosive sublimate; that of hyposulphite of soda and ioduretted iodide of potassium as remedies for poisoning by opium, &c. &c., as if these substances were to be met with in every house, or as if there was abundance of time to go about in search of them.

Under the head of *physical antidotes* we would place those substances whose remedial powers depend upon some physical influence, such as an absorbent or suspending power which they may exert over the poison. To this class belong charcoal, the hydrates of iron and magnesia, gruel, oil and water, chalk and water, flour and water, &c., all of which are useful as a means of suspending the poisonous agent, and of protecting the coats of the stomach during the time which is lost in the administration of an emetic, or the employment of the stomach-pump. Mr. Taylor, however, almost rejects this class of remedies, and says it is quite a misapplication of language to call them antidotes; for he has examined with some care the chemical action of charcoal, hydrated oxide of iron, and hydrate of magnesia, on solutions of arsenious acid, and, from his results, he puts no confidence in them as antidotes to this body. He finds that charcoal powder does not remove any notable quantity of arsenic from its solution, and he says, "I therefore most fully agree with the statements made many years since by M. Devergie, that not the slightest confidence can be placed in animal charcoal as an antidote in any case of poisoning." He has come to the same conclusion with reference to the hydrates of magnesia and iron, saying—

"I have placed these substances in the list of antidotes, in deference to the views entertained by some eminent toxicologists; but I do not consider it the less necessary to state here those circumstances which induce me to believe that no reliance can be placed on either of them in cases of poisoning by arsenic." (p. 86.)

The circumstances to which he here alludes are,—1, That these substances do not completely precipitate arsenious acid from its solution; and, 2, that the arsenites of these bases are themselves poisonous. We admit that both of these circumstances are to be considered as formidable objections to the use of hydrate of oxide of iron, or magnesia, as *chemical* antidotes; but we think that Mr. Taylor does not place a sufficient reliance upon the physical powers of these substances. He allows, however, that they are of some value, and that matters of this kind may serve to envelope the poison, and so mechanically prevent them from coming into contact with the surface of the stomach. Now this is a very important admission, and would justify him, independently of any deference to the views of eminent toxicologists, in placing such substances in the list of antidotes. As far as our own experience goes, we feel warranted in relying to a very great extent upon the use of physical antidotes; and we are disposed to think, moreover, from the observations which fall now and then from

our author, that he also has had ample opportunity of witnessing their value as remedial agents.

Among the *physiological antidotes*, may be classed all those substances which, when administered internally in cases of poisoning, operate beneficially upon the various functions of the living body; as by rousing the patient when he is depressed by the poison, or quieting him when he is excited thereby; by relieving pain, checking vomiting or purging, or by promoting the expulsion of the poison through any of the secreting organs. As instances of these, we may mention the use of stimuli, in cases of poisoning by hydrocyanic acid, the vapour of ether, or chloroform; that of astringents, such as infusion of coffee and tea, in cases of poisoning by opium or alcohol; and the use of diuretics, to which we have already referred, as a means of eliminating poison from the animal body. Again, Dr. Christison has remarked that the remote operation of lead may sometimes be corrected by mercury given to salivation, and that the violent salivation caused by mercury may be occasionally corrected by nauseating doses of antimony. Mr. Alison has more recently stated, that chlorate of potash possesses the property of subduing mercurial ptyalism; and that, from the results of numerous trials, he believes this salt to stand in an antagonistic relation to mercury. Mr. Taylor, however, dismisses all these remedies very briefly, since he does not admit that they are to be considered as antidotes. In fact, he is compelled to take but a very limited view of the subject, in consequence of the peculiar definition which he has given of an antidote.

The other points of treatment to which our author has referred in his general remarks, are not put forth with much prominence. He mentions the necessity for keeping the patient roused in a case of poisoning by opium; and states that the best mode of doing this, is to pass a succession of shocks from an electro-magnetic apparatus, along the spine and cardiac region.

"In the absence of this apparatus, the patient may be kept roused by causing two active persons to walk about with him, by dashing cold water upon the chest, back, and head, by rubbing the chest and back and palms of the hands with compound camphor liniment, or any other stimulating embrocation. It has been recommended to employ flagellation to the palms of the hands and soles of the feet. In the case of young infants, it has been found sometimes beneficial to plunge them into a warm bath, and suddenly raise them into the cold air; this is said to have acted as a very effectual stimulant." (p. 81.)

But he has not adverted here to the great advantage which is frequently derived from the performance of artificial respiration. We pointed out in our former article, that most of the organic poisons are very prone to undergo decomposition and a sort of combustion in the living body; so that the system has within itself the power of destroying their virulence. This decomposition will always take place, provided the functions of circulation and respiration be maintained with sufficient activity. In most cases of organic poisoning, therefore, it is proper to give every assistance to the full operation of the power whereby this may be effected; even when the patient is on the verge of the grave we may frequently maintain this act of decomposition, by the performance of artificial respiration. And considering the great advantage which has resulted from it in practice, we are somewhat surprised that Mr. Taylor has not directed some attention to this mode of treatment.

We now turn to his observations on *the way in which an examination of poisoning ought to be conducted:*—the object in such cases being, not merely to determine whether the individual is suffering from the effects of poison, but also to learn in what manner, and by whom, the poison has been administered to him.

This part of the duty of the medical attendant is, we fear, too frequently neglected; and as a consequence, the crime of secret poisoning has become a very common one, much more so than the records of the public journals appear to indicate; in fact, Mr. Taylor, in speaking of a case of slow poisoning by arsenic, says:—

"I believe this mode of poisoning to be more frequent in this country than is commonly supposed; and it behoves practitioners to be exceedingly guarded in their diagnosis, for the usual characters of arsenical poisoning are completely masked." (p. 315.)

Our own experience quite confirms this statement; indeed, we have no hesitation in saying that the present enormity and extent of this crime have never been equalled in any period of history. Nor will this be wondered at, when we consider how many circumstances have tended to develope it. With a government careless about the sale of poisons; a druggist ready to profit by it; a profession negligent of its duties, or else crippled in its means of performing them; a coroner thrifty of labour, and a magistrate prone to avoid it; an advocate ever ready to defend, and a judge slow to convict;—who can be surprised that the progress of secret poisoning has been thus onward?

The duty of the medical attendant, however, is clear; and by the proper performance of it, he may frequently turn aside many of these untoward circumstances, so as by his conduct to elevate the character of his profession, forward the ends of justice, and promote the well-being of society. To this end he should always be on the alert, in order to be enabled to detect the action of a poison; and he ought also to be careful and judicious in his mode of investigating it. He should note carefully, attentively, and methodically, every circumstance which may tend to throw a light upon the difficulties of the case; remembering that with him rests the duty, not only of ministering to the relief of the sufferer, but also of clearing up the various perplexities which may surround the inquiry. And should it become a matter of juridical investigation, it is to him that the judge will turn for the most valuable testimony: the appeal being made to him, as to one qualified by habits of observation, by familiarity with disease, and by a frequent intercourse with the dying, in the expectation that he will place the clearest evidence before the jury. But how complete will be the disappointment, how sad the disaster, and how degrading the result, if it is found that he has been neglectful of his duty, or unequal to the performance of it. We have seen such a man in the witness-box, enduring, as Dr. Smith says, the scrutiny and displeasure of the bench, the browbeating of the bar, the derision, the laughter, and the contempt of the audience, the discontent of his friends, the disgrace cast by him upon his profession, the exposure of the public press, with all the pernicious consequences which may follow to his reputation and fortune.—But there is a reverse of this picture. We have seen others who have brought credit upon their profession, who have acquitted themselves nobly; who

have neglected no part of their duties; and we have been gratified by hearing the highest compliments passed by judges upon the conduct of medical witnesses, whose watchfulness had obviously been the means of bringing an atrocious criminal to the bar of justice, and whose evidence had been found to be sufficient to convict him.

Here, however, it is our duty to caution our readers not to run off into the other extreme; not to be too officious. As we have said, there is a strong tendency in the public mind to misconstrue the actions of medical men, and to set them down to the influence of some personal motive. In no place is this tendency more likely to be misdirected, or more certain to be turned against an officious practitioner, than in a court of law; there, if he has unduly thrust himself forward, his conduct will be sifted, and there he must abide the issue of it; and what can be more damnifying, than to be addressed in language like the following: "This, sir, is the woman whom you have involved in an expensive prosecution, whose expectations you have blasted, whose life you have brought into the most imminent danger, and whose name you have attempted to stigmatise with indelible disgrace."

To avoid being placed in any such unpleasant position, we would recommend our readers to make themselves acquainted with the rules laid down by Mr. Taylor (pp. 101-8), for the investigation of cases of poisoning; for by the careful observance of them, the medical jurist will in general be enabled, without difficulty, to determine the probable time of death, the probable cause of death, and the actual means whereby death was brought about. In addition to what Mr. Taylor has directed to be done, we would recommend the practitioner to preserve the urine for analysis.

Mr. Taylor very properly urges the practitioner to make the most careful examination of every important organ; and to bear in mind that the body is inspected, not merely to show that the individual has died from poison, but to prove that he has not died from any natural cause of disease. It is necessary, therefore, to inspect the valves of the heart, the coronary arteries, to slice the brain through and through, and, in some cases, to open the spinal cavity. At the trial of Tawell for the murder of Sarah Hart, the neglect of these points was made an important element of the defence, and it procured for the unfortunate witnesses a severe rebuke.

Among the evidences of poisoning to be obtained from the post-mortem appearances, Mr. Taylor mentions, redness of the stomach and intestines, ulceration, softening, and perforation of the bowel. The first of these he considers to be a main character of the action of an irritant poison. He says—

> "It is sometimes diffused over the whole mucous membrane; at other times it is seen in patches over the surface of the stomach. It is sometimes met with at the smaller, but more commonly at the larger extremity of the organ; and then, again, we occasionally find that the rugæ or prominences only of the mucous membrane present this red or inflamed appearance." (p. 114.)

In cases of poisoning by arsenic, we have generally observed that the redness of the mucous coat of the stomach assumes a very peculiar character: being made up of a great number of small stellate dots. This character is not referred to by Mr. Taylor, although we have found that it affords a very important indication of the action of arsenious acid.

And again, the fact is not sufficiently dwelt upon by him, that the small intestines are commonly inflamed in cases of poisoning by the corrosive irritants, while the rectum and lower portion of the large intestines are generally found reddened after the operation of arsenic, savin, gamboge, &c. &c.

Although the redness of the stomach and alimentary canal is usually regarded as a strong evidence of poisoning, yet it is often due to the operation of other causes. Among these Mr. Taylor mentions gastritis, enteritis, the active process of digestion, the juxtaposition of the stomach to the spleen and the oozing of blood therefrom, putrefaction, the gravitation of the blood, and the administration of some colouring matter. The operation of some of these causes may be readily detected; others, however, require care and a more extended examination. Upon this subject our author quotes the important conclusions which have been arrived at by Dr. Yellowly, viz.:

"1. That vascular fulness of the lining membrane of the stomach, whether florid or dark-coloured, is not a special mark of disease, because it is not inconsistent with a previous state of perfect health. 2. That those pathologists were deceived, who supposed, from the existence of this redness in the stomach, that gastritis sometimes existed without symptoms. 3. That erroneous conclusions as to the cause of death were frequently owing to the same mistaken observations:—the effects of putrefaction and spontaneous changes, induced by the law of vitality, being sometimes attributed to the action of poisons. 4, That the vascularity in question is entirely venous, the florid state of the vessels arising from the arterial character of the blood remaining in the veins for some time after its transmission from the arterial capillaries at the close of life. The appearance is, however, sometimes due to the transudation only. 5. That the fact of inflammation having existed previously to death, cannot be inferred merely from the aspect of the vessels in a dead part." (p. 116.)

These conclusions, supported as they are, by the testimony of Andral and other pathologists, have obviously a most important bearing upon medico-legal practice; and Mr. Taylor has given them due consideration in this and other parts of his work.

Our author has omitted to mention that, in cases of poisoning by the mineral acids and caustic alkalies, the lining membrane of the stomach is frequently of a brown, or almost black colour. And we have observed instances of chronic lead poisoning, in which the intestines were paler than natural, and tinted into a dull leaden hue. Again, it has sometimes happened, even in cases of poisoning by an irritant, that neither the stomach nor intestines have exhibited any red or inflammatory condition. Mr. Taylor has given several instances in illustration of this at page 327.

We agree with our author, that *ulceration of the stomach or intestines* is a comparatively rare accident from poisoning. We have never seen an instance of it. It is, however, a very frequent consequence of disease, occurring during fever, phthisis, and dysentery; in these cases the ulcers occupy the position of the mucous glands, some of which will generally exhibit an inflamed or transition state.

Softening of the coats of the stomach and perforation may result from poison, disease, or the action of the gastric juice. Mr. Taylor states, that softening and perforation arising from the first cause may be recognised by the corrosive influence, which the poison will have exerted upon the mucous membrane of the mouth and œsophagus; while, in cases of softening

from disease, or the action of the gastric juice, the change is confined to the stomach alone. This statement is no doubt true in a general way, but there are exceptions to it; for we have seen several cases of poisoning by oxalic acid, in which there was not any trace of its action, either in the mouth or œsophagus, although the stomach was reduced to a gelatinous mass. Apropos of this, Mr. Taylor says, at p. 250, that it is rare to hear of the coats of the stomach being perforated by this acid, and that he has found nothing to bear out the view, that perforation of the stomach is a common effect of the poison. Now, although this acid may not always *perforate* the walls of the stomach, yet the statement, as it is here given, conveys an erroneous impression. It might lead the reader to believe, that oxalic acid could not exert any solvent action over the tissues of this organ; but, if we take the admission of Mr. Taylor, that "it undoubtedly renders the mucous coat soft and brittle, and dissolves, by long contact, animal matters," we have sufficient to show that oxalic acid can put forth a remarkable disintegrating power over the coats of this organ. We have the notes of five cases of poisoning by oxalic acid before us; and we might quote many more, in all of which the dissolving power of the acid was a remarkable feature; and if it can *dissolve* the coats of the stomach, why may it not be said to *corrode* and *perforate* them? But, to go to the test of experiment, a plan of proceeding which has always found great favour with Mr. Taylor, it will be found that even a weak solution of oxalic acid will quickly act upon the gastric membrane, and will render it pulpy and gelatinous, provided it be exposed to the temperature of from 80° to 90° F.

Another kind of softening and perforation of the stomach is referred to by our author, under the name of "*spontaneous,* or *gelatinized perforation.*" This also is a post-mortem result. He says it is usually met with at the cardiac end, and is no doubt due to the action of the gastric juice, for the softening has occasionally been found to extend itself to the neighbouring organs. Mr. Taylor remarks that—

"This form of perforation is, so far as I can judge, by no means common. It is reported to have been met with in children affected with hydrocephalus,—in those who have died from typhus fever; and, according to Andral, in females who have died during parturition. Dr. Macintyre informed me, that he had met with two cases of this kind of perforation, in young subjects, affected with diabetes. The conditions for its production, whether local or constitutional, and the circumstances under which it occurs, are very obscure." (p. 122.)

According to our experience, it is a purely chemical act, depending on the presence of a considerable quantity of gastric juice, death having occurred during the active performance of the digestive process. It will be known, therefore, by the ragged character of the perforation, by the stomach containing more or less partially digested food, by its contents being very acid, and by there being no discoloration of the mucous coat.

Mr. Taylor has mentioned some other causes of perforation, such as ulceration, worms, &c.; but these will be so readily discovered by the practitioner, that we do not think it necessary to make a further reference to them.

There are a few post-mortem signs of poisoning, to which our author has not alluded; for example, he has not referred to that highly congested state of the lungs, which has been noticed in cases of poisoning by prussic

and carbonic acids. This condition of the lungs has also been observed after death from opium, strychnia, arsenic, and a few other poisons. In these cases, the right side of the heart has generally been found distended with dark fluid blood, and the left comparatively empty. Again, the blood is commonly black and uncoagulated in cases of poisoning; it escapes freely from the incisions which are made during the examination, and it is found to gravitate, so as to render the dependent parts of the body turgid and purple.

When bodies are exhumed for the purpose of making a chemical examination of them, it is necessary to observe a great deal of care in conducting the operation; for it has been found that the soil of graveyards frequently contains arsenic. Mr. Taylor has referred, on various occasions, to this fact, and at page 366 he puts the question—"Is the arsenic in a soluble form?" In order to establish the importance of this question, he refers to the case of Elizabeth Johnson, who was tried at the Liverpool Lent assizes, in 1847, for the murder of her husband. The deceased died on the 3d of December, 1846, and his body was exhumed on the 9th of March, 1847, that is, just three months after interment. Large quantities of arsenic were detected in the tissues; but it appears that the grave was a very wet one, and that the coffin was split from end to end. These facts laid the case open to a suspicion that the arsenic might have been washed from the soil into the body; and the judge put it to Mr. Leigh, the chief medical witness, "whether, supposing arsenic to exist in the soil of the church-yard, it was not possible for some of that arsenic to have been introduced into the body, along with the water?" Mr. Leigh replied, that he thought it was possible; and this unfortunate admission constituted the ground of the acquittal of the prisoner.

It is now known, that *arsenic, in the state in which it exists in the soil, is not soluble in water*, and that it cannot in this way impregnate the tissues of a dead body. We may here state that chemists are not in a condition to decide upon the *source* of arsenic in the soil of graveyards. Orfila asserted that it resulted from the disintegration of human bones; but as it is now quite established, that neither the bones nor any other parts of the human body contain arsenic as a normal constituent, and as arsenic is met with very commonly in other soils, existing wherever ferruginous compounds abound, we are compelled to reject this opinion of Orfila. Be its source, however, what it may, abundant opportunity has occurred to show that it exists in the soil in the states of arsenite and arseniate of lime, iron, and copper, and also as a sulphuret in many metallic pyrites.

With these facts before us, it is extremely important that we should be most careful in conducting the exhumation of a body, so as to avoid the admixture of any soil with the parts intended for analysis; for although the arsenic, in the state in which it exists in the soil, is not soluble in water, yet it will be made soluble by the processes to which the chemist is accustomed to resort in effecting his analysis. Another precaution which is properly dwelt on by Mr. Taylor, is, that the parts removed for examination should not be allowed to come into contact with any metal, nor with any surface, except that of clean porcelain, or wood:

"It has been recommended that they should be washed with chloride of lime, or placed in alcohol; but this is decidedly improper. The use of any preservative

chemical liquid would not only embarrass the future analysis, but would render a special examination of an unused portion of the liquid necessary—the identity of which would have to be unequivocally established." (p. 103.)

This reminds us of the necessity for a strict observance of another rule, viz. *that every care should be taken to maintain the identity of the substance to be submitted to analysis.* This point is most rigorously insisted upon by lawyers; and, in order to carry it out, Mr. Taylor gives some very useful directions, urging that the matters taken from the body should never be placed on any surface, or in any vessel, until it has been ascertained that such surface or vessel is perfectly clean. They should be preserved in wide-mouthed bottles, with as little air as possible, and secured with well-fitting corks, covered with bladder. A piece of bladder or skin should be tied over the mouth of each bottle, and the whole rendered perfectly secure by affixing a seal in such a manner as to prevent access to the contents. If there is not an opportunity of doing this at the time of the examination, the practitioner should take charge of the suspected matters, and he should not allow them to pass out of his sight or custody, until he has secured them with his private seal. It is proper also to attach a label to each bottle, stating what it contains, and from whom and at what time its contents were removed. The neglect of any of these precautions may prove fatal to the identity of the substance, and may serve as a good reason for rejecting the most powerful chemical evidence. We remember a case, which shows the necessity for the observance of these rules, and for the exercise of a little thought in conducting operations of this kind. The body of an old woman, who had died under circumstances which excited a strong suspicion that she had been poisoned, was examined by two medical men, who attended her during her last illness. The body was in a state of active decomposition, and, in order to get rid of the offensive odour which emanated from it, they moistened a cloth with Ledoyen's disinfecting liquid (a solution of nitrate of lead), and laid it over the abdominal viscera. The consequence of this was, that, upon making an analysis of the intestinal tissues, a large quantity of lead was discovered; and, if the chemist had not inquired into the source of the poison, his evidence might have complicated the case, and have led to an incorrect verdict.

The chemical analysis of matters supposed to contain poison, is by far the most difficult part of the inquiry into a case of poisoning; and, as we have already stated, it should not be undertaken by any one who is not fully conversant with both the principles and practice of analytical chemistry. It is necessary, also, that he should have had some experience in the special chemistry of the poisons; for it ought to be remembered that neither the visceral contents, nor the tissues of the body, are likely to contain poison in any large quantity; and moreover, that the presence of organic matter has a great influence in masking the common characters of a poison, so that it cannot be detected by ordinary processes and reagents. Again, without some practice in chemical research, the operator is very likely to be misled by impurities in his materials, or by imperfections in his processes. And when to this is added, that the law attaches the greatest importance to the results of his investigations, and sanctions the severest cross-examination, in order that the jury may be convinced of his skill and judgment,—and when we further consider that upon his evidence

the life of a fellow-creature, and the welfare of society depend,—it will be easily conceived that the duties attached to this part of the inquiry are most onerous, and should not be undertaken by any one who is not a proficient in toxicological chemistry.

With these impressions, we are doubtful whether we shall be doing our readers a service by commenting upon the rules which have been laid down by Mr. Taylor for the guidance of the analytical chemist.

The following is his mode of treating the subject. He first directs attention to the *kind of material* to which an analysis may extend, as—1. To the pure poison, found in the possession of the accused. 2. To the article of food which is suspected to be poisoned. 3. To matters which have been vomited or otherwise evacuated from the body. 4. To the contents of the viscera, and to the tissues of the body.

He next refers to the *causes which operate against the detection of poison*, as—1. Its nature; the poison being volatile, or easily decomposed, as is the case with prussic acid, alcohol, and the organic poisons. 2. The influence of vomiting and purging, which may operate in getting rid of every trace of the poison, though this is a very rare occurrence. 3. Its loss by absorption and elimination: this takes place most rapidly when the poison has been given in solution; in fact, under such circumstances, it may be removed entirely from the alimentary canal. In cases of mineral poisoning, however, the *corpus delicti* will still exist in the organs and tissues of the body; and of all these the tissue of the liver, according to Flandin, is likely to furnish the largest amount of it; for he asserts that it contains nine tenths of the whole quantity carried into the circulation. Having entered the circulation, it is proper to remember that there are two ways in which the poison may be got rid of; it may be digested and consumed, as is the case with most of the narcotico-vegetable poisons, or it may be excreted by the various depurating organs. Orfila has found that arsenic passes off most readily by the kidneys. Danger and Flandin assert that it escapes by the liver, the lungs, and the skin; and they have also ascertained that the salts of copper are more readily detected in the bronchial secretion than in the urine. 4. The influence of treatment: as when the poison has been neutralized and pumped out of the stomach, or carried off by purgatives. 5. Loss by putrefaction: this more commonly occurs in the case of organic poisons, since it has been established beyond all doubt, that the mineral poisons may remain in the dead body for any length of time. Arsenic has been detected ten years after interment; and Mr. Taylor mentions, at page 365, that he has discovered it in the contents of a stomach which he has had by him for thirteen years.

It is of considerable importance to the medical jurist to know what period of time is necessary for the complete elimination of a poison. This question is discussed by Mr. Taylor in various parts of his book, as at pages 23, 363, 386, &c.; the chief facts connected with it appear to have been derived from the experiments of Briand, Danger, and Flandin. The first of these chemists found that antimony and arsenic are got rid of in from ten to fifteen days; but Danger and Flandin have detected arsenic in the secretions of a sheep poisoned by a large dose of it, up to the thirty-fifth day after its administration. In their experiments upon young dogs, however, they found that it was got rid of in from six to ten days. Mr. Taylor states at page 23, that he was unable to detect arsenic in the body

of a person who had died eight days after the exhibition of a very small dose of it. From all this it would appear that we are not in a condition to speak positively upon this question, or to say more than that the organic poisons are usually got rid of in a few hours or days, that arsenic and antimony are probably eliminated in from one to two weeks, while mercury, lead, and silver may remain in the system for a much longer period.

In the third place, our author refers to *the objects of a chemical analysis*, as—1. The determination of the *nature* of the poison. 2. The determination of the proportion or *quantity* in which it has been taken. We doubt whether Mr. Taylor's remarks on this subject will prove to be of any great service either to the chemist or to the medical practitioner. The former ought to be too well qualified to be dependent on such directions, somewhat trivial as they are; and the latter would not be justified in relying upon them, if not previously experienced in chemical analysis. To give a few illustrations;—what person having the slightest experience in chemical matters, need be told that he is not to use apparatus impregnated with poison, or materials upon which he could not depend? The man who is in danger of coming to a hasty and premature opinion, is not fitted for the task of an analytical chemist; nor is he whose experience has not qualified him to detect and interpret every fallacy, and to avoid every impediment. We should not have found fault with Mr. Taylor for commenting upon these points, had his remarks been put forth merely as illustrations of the dangers which surround the subject of chemical toxicology, or with the object of keeping in check the rashness of any one who would attempt to perform a chemical analysis without some previous acquaintance with the difficulties attendant upon it; and that there are such men, who can have a doubt? Our author, in fact, at page 107, in attempting to excuse the conduct of a forgetful witness, says, "many who are summoned to give evidence in courts of law, are not much accustomed to the analysis of poisons; and probably may never have performed the usual experiments, until that particular case occurred to draw their attention to the subject." We feel it to be our duty to pass an unhesitating censure on all such persons, as being the originators of that want of confidence and discourtesy so commonly manifested towards the scientific witness in our courts of law, and to point to them as the cause of the frequent failure of criminal prosecutions. We regret, therefore, that they should receive any encouragement from a medico-legal writer.

We should not have written a word in censure of these so-called *rules* of Mr. Taylor, much as we think them out of place, if they had been put before the reader simply as *lessons* in chemical toxicology. Indeed we are disposed to think that the author commenced his subject with the intention of doing something of this sort; for in Chapter XI he has given us a list of the reagents and apparatus required by the analyst: this list, however, is far more suited to the notice of an inept schoolboy, who wishes to learn a trick or two in chemistry, than to the serious consideration of a man who purposes to undertake an inquiry which is to involve the life or death of a fellow-creature; and we object to it, not merely on account of its scantiness, but also because it contains many substances which are useless, and others which are positively objectionable. We do not know, for example, why it should include the two mineral alkalies,

nor can we see the necessity for three alkaline carbonates; neither is there any occasion for the employment of two soluble salts of baryta, nor for that of sulphate of lime or sulphate of strontia. Black flux is a bad preparation, and it should be discarded; for, as a reducing agent, it is not at all equal to a mixture of charcoal and carbonate of soda, or to that of cyanide of potassium and carbonate of soda. Again, the process given by the author for the preparation of cyanide of potassium is not a good one; for it produces a salt, which is damp, deliquescent, and easily susceptible of decomposition. Hydrosulphuret of ammonia is a compound so prone to spontaneous change, and so likely thereby to give false indications, that it should not be trusted in the hands of an inexperienced operator. And lastly, iodic acid is of no practical value as an analytical agent; for we can employ much better tests for sulphurous and sulphuric acid; while the fact of its being decomposed by urine, saliva, or indeed any putrid organic matter, is sufficient to justify us in condemning it as a test for morphia.

At page 145, Mr. Taylor remarks that—

"One of the most difficult problems which a medical jurist has to solve in relation to poisons, is that which is commonly left untouched in works on toxicology; namely, what steps are to be pursued in order to determine the nature of a suspected poison. It is easy to verify, by the application of chemical tests, the nature of a mineral poison, when we know or really suspect what it is; but all who have exercised themselves in these matters must have felt the difficulty amidst the multiplicity of tests, to make a selection and apply them in particular cases."

Now with such a conviction upon our author's mind, one would have supposed that he would have avoided all such multiplicities, and that he would have furnished the reader with some assistance in getting over the difficulty to which he refers; but he has done nothing of the sort. It is true, he has drawn up seven tables exhibiting the reactions of tests upon the various poisons in their pure state, including sulphate of indigo, the salts of lime, baryta, strontia, cadmium, nickel, cobalt, manganese, gold, platinum, uranium, ammoniuret of iron, prussiate of potash, iodide of mercury, indigo, smalt, prussian blue, and many other most strange substances; and yet he has left almost untouched the great question, as to how a medical jurist is to proceed with the analysis of an organic mixture, which is merely suspected to contain a poison of some sort.

Mr. Taylor advises the chemist to get a hint or two upon the subject before he starts, to seek among the symptoms for some token of the nature of the poison, some sign whereby he may be able to guess at it. This mode of proceeding, however, is open to every kind of objection; it is likely to prejudice the judgment of the operator, and to destroy the confidence of all who may be interested in the inquiry, by making them believe that the principles of chemistry are uncertain, that the results of its operations are subordinate to the other portions of the case, and that they can never be made an independent or primary part of the investigation. Besides, the operator may be misinformed respecting the symptoms, or not shrewd enough in his guesses; and in seeking for *some suspected* body, after the special directions usually given for its detection, he may waste the whole of his material, and may miss every poison contained in it. Indeed, this is by no means an improbable circumstance; for, in the case of Sarah Hart, Mr. Cooper was led to believe that oxalic acid was in her stomach, and he sought for it and for the mineral poisons,

until he had almost exhausted his material, and then accident, for it was nothing else, led to the discovery of prussic acid. It is proper, therefore, and most advisable that we should have some fixed rule, some general plan of proceeding, whereby the operator may neither waste his material, nor fail to detect every kind of poison contained in it. We are somewhat surprised that Mr. Taylor does not admit of the necessity for such a rule, for in commenting upon an analytical process suggested by Fresenius and Babo, he says:

"However desirable it may be to possess such a method as is here sketched out, the absolute necessity for it is not apparent. Arsenic may be most satisfactorily detected by processes which are not fitted for the detection of other metallic poisons. If each poison has its own particular process, and this is satisfactory so long as it is confined to its proper object, it is impossible to allow that the admissibility of chemical evidence in cases of arsenical poisoning, should be made to rest on the universal application of the same process, and with a like degree of certainty to other poisons." (p. 157.)

As we have already remarked, we scarcely know whether we are consulting the interests of our readers by entering more fully into this part of the subject; still, as it has been treated so imperfectly in this and other works upon toxicology, we consider it our duty to offer a few general directions upon the mode in which the analysis of the contents of a stomach and of the other parts of the body should be proceeded with.

In analysing the contents of the stomach,—1. Note their quantity, colour, odour, and general appearance. 2. Ascertain if they are acid or alkaline. 3. Apply the tests for the vapour of hydrocyanic acid. 4. Strain them through a fine linen cloth, and examine the solid portion for the leaves, berries, &c., of poisonous plants, for the glittering particles of cantharides, and for any small pieces of arsenic, or other solid poison. 5. Take half of the strained portion, distil it from a salt-water bath, until about one third of it has passed over; examine this distilled portion for the volatile poisons, such as alcohol, prussic acid, oil of bitter almonds, savin oil, &c., and then, if necessary, continue the distillation. 6. Evaporate the residue from a water-bath, until it is nearly dry. 7. Digest this nearly-dry portion in alcohol of about the sp. gr. of ·840; this liquid will dissolve the organic poisons, the mineral acids and alkalies, and most of the metallic poisons; it will not dissolve tartar emetic, sulphate of copper, or sulphate of zinc, any one of which may have been administered as a remedial agent. 8. Filter the alcoholic solution, and ascertain if it is acid or not; pass sulphuretted hydrogen gas through it, and set it aside for twenty-four hours; then observe if there has been a deposit of any metallic sulphuret, if so, the nature of the metal must be made out. 9. Treat the clear liquid with a solution of acetate of lead, and filter. 10. Pass sulphuretted hydrogen through the filtered portion, then filter again and evaporate from a steam bath; test for an acetate of the alkaloids. 11. Diffuse the precipitate from operation 9 through water, and pass sulphuretted hydrogen through it; filter and test the clear liquor for sulphuric, oxalic, and meconic acids. 12. Examine the alcoholic residue for tartar emetic, sulphate of copper, &c.

After these preliminary investigations, the chemist will be enabled to form a very correct opinion as to the nature of the poison present, and he may then proceed with much confidence to a special examination of the

remainder of his material, by which means he will have an opportunity of verifying his former results, and of determining the amount of the poison in it.

In those cases in which it is merely necessary to seek for arsenic, we have found that the plan proposed by Dr. Letheby, in the 'Pharmaceutical Journal' for October, 1845, is by far the most simple and easy of application. He directs us to acidulate the suspected liquid with a few drops of nitric acid, then to introduce a few pieces of granulated zinc, and to keep the mixture near to a boiling temperature for about an hour; the arsenic will attach itself to the zinc, and on washing the latter with a little boiling water, and introducing it into Marsh's apparatus, it may be made available for the generation of arseniuretted hydrogen. Dr. Letheby states that the delicacy of this test is very great; it will serve for the detection of arsenic when it is dissolved in 200,000 parts of water, and it is not at all difficult to detect the 1-200th of a grain of arsenious acid, even when it is mixed with many ounces of organic matter. This test appears to be particularly applicable to the detection of arsenic in urine and other organic fluids.

In many cases, however, the chemist is called upon to determine the presence of a mineral poison in the tissues, as of the liver, intestine, spleen, kidneys, and muscles. In all these cases, the mineral is firmly attached to the organic matter, and cannot be washed out of it by ordinary solvents; in order to make it soluble, therefore, it is necessary to break up the tissue; and various processes have been suggested for this purpose. Orfila recommends that the tissue should be dried, and then incinerated with nitre; but a very little practice will convince any one that much of the poison will be dissipated during the operation. In the case of mercury and arsenic, it will be almost entirely volatilized. M. Flandin has proposed that the tissue should be carbonized by means of boiling-hot sulphuric acid, gradually heating the mixture until the acid is got rid of; but here again we are quite certain that neither mercury nor arsenic will resist the high temperature necessary to such a process. To avoid this loss, the other mineral acids have been resorted to; thus Orfila has suggested the use of nitric acid; Reinsch employs a mixture consisting of one part of muriatic acid and seven of water; and Dr. Letheby has spoken very favorably of a liquid composed of two parts of muriatic acid and one of nitric. The objection to the processes of Orfila and Reinsch is, that they are not suited to the solution of every kind of poison; for example, tin and antimony would not be dissolved completely by nitric acid; and muriatic acid will not act upon the sulphuret of arsenic, a compound which is very likely to be formed in the dead body of a person who has been poisoned by arsenious acid.

In reference to the value of Reinsch's and Marsh's tests, Mr. Taylor makes the following remarkable statement:

"These tests are fully equal to the detection of arsenic in all the forms in which *it is most commonly found in practice;* that they do not detect all other metallic poisons, or that their operation on arsenic is occasionally rendered obscure by the presence of other substances, are objections which amount to nothing in the hands of those who limit the application of these tests to the purposes for which they were originally designed." (p. 158.)

Now we must observe, in the first place, that they are not equal to the detection of arsenic in all the forms in which it is most commonly found

in practice; and secondly, that the objections referred to were found to be very serious, even in the hands of such experienced chemists as MM. Fresenius and Babo, who have therefore discarded the processes of Marsh and Reinsch, and have come to the conclusion, that the tissue should be broken up by means of hot concentrated muriatic acid and chlorate of potash, and the metal then precipitated as a sulphuret. As far as our experience goes, this process is a most effectual one for the destruction of the tissues, but it presents one great disadvantage—it leaves a mass so charged with saline matter, that it is extremely difficult to effect a complete separation of the poison from it; nevertheless, we do not admit that "the process appears to be rather adapted for separating arsenic from its ores, than for detecting it as a poison in medico-legal cases." And we are rather surprised that Mr. Taylor should condemn it because—

"In practice we do not find the compounds of lead, bismuth, copper, and mercury mixed with arsenic; and therefore it is useless always to have recourse to a process which invariably presupposes the admixture of these metals with the poison criminally administered." (p. 159.)

Now, if the process of Fresenius were rendered more complicated by reason of the presupposition to which Mr. Taylor alludes, there might be a good cause for rejecting it; but as this does not involve any alteration whatever in its general routine, and as it is just possible that these substances may also be present, we take it that the objections here put forth constitute the best reason in the world for admitting the process; because, as Fresenius said when he advanced it, it is necessary that a process laying claim to general applicability, should lead, not merely to the detection of arsenic, but also to that of every other metallic poison. Mr. Taylor's reasoning and advice go all along upon the supposition, that the chemist knows exactly what he is going to find, before he commences his operations. This, in fact, is a common error with writers on toxicology, who are ever speaking of the advantages of such and such processes for the detection of such and such poisons; as if the operator was always fully aware of the exact nature of the poison in a suspected liquid, *before* he had made an analysis thereof. It cannot, we think, be too strongly enforced, that when a chemist is engaged in an inquiry of this sort, he should not be biassed by any preconceived opinion, but he should proceed steadily and systematically, so as neither to waste his material, nor to miss anything of a poisonous nature, nor to be deceived by any fallacy in his results.

The process which we have found to be best suited to the destruction of the animal tissues, and to the solution of every kind of mineral poison, is the following, which is a modification of one recommended by Dr. Letheby, in the journal before referred to: 1. Cut up the tissues into very small pieces; put them into an evaporating dish, and drench them with a liquid consisting of one part of nitric acid, two parts of muriatic, and four of water. 2. Keep the whole at a temperature just below the boiling point, until the tissue is completely broken up; its dissolution may be assisted by crushing it with a pestle. 3. Strain off the clear yellowish liquid, and evaporate it to dryness, taking care that it does not boil violently; by this means a brittle, charred mass will be obtained. 4. Powder this mass, sprinkle it with a few drops of muriatic acid, add a little water, and make it boil. 5. Filter the solution, and wash the residue with water. 6. Pass sulphuretted hydrogen gas into the filtered liquid, and set it aside for any metallic sulphuret to deposit; collect this deposit

on a filter, and examine for arsenic, copper, &c. 7. Neutralize the residual liquor with ammonia, continue the stream of sulphuretted hydrogen, and examine the deposit, if there be any.

In the analysis of the tissues, there is one important point to which the chemist should direct his attention, viz. that lead and copper have been said to exist normally in the animal tissues: under such circumstances, however, they cannot be removed or dissolved out by simply boiling the tissues in water; while in cases of poisoning by these substances, a maceration of the tissues in water will generally furnish some evidence of the presence of the metal.

On the mode of framing medico-legal reports. Mr. Taylor has given some very useful hints upon this part of the duty of the medical jurist, by directing attention to the manner in which it should be performed,—in regard, 1st, to symptoms; 2d, to the post-mortem appearances; and, 3d, to the results of the analysis. We have already alluded to the chief points which are to be noted with respect to the symptoms and post-mortem appearances; those which refer to the chemical analysis do not commonly come under the consideration of the medical practitioner, and they need not be noticed here.

In framing reports, Mr. Taylor advises that the facts should be put down seriatim, stated plainly, without circumlocution, and in language easily intelligible to non-professional persons. He also advises the writer not to encumber his statements with opinions and inferences, but to reserve his conclusions until the end of the report. These conclusions should be strictly kept to the matters which are the subject of inquiry; and they should be based on medical facts, not upon moral circumstances. Further, they ought to be founded only on what he has himself seen or observed.

With respect to the use of notes. This is a point to which very few medical men give proper attention, and yet, in consequence of the great length of time which frequently intervenes between the committal of a prisoner and his trial, it is highly necessary that the practitioner should have some means of preventing the leading facts of the case from slipping from his memory. This can only be accomplished by means of notes, which should be made at a proper time and in a proper way; for the law relative to the admissibility of notes or memoranda in evidence is, as Mr. Taylor remarks, very strict, and is rigorously insisted on by the judges.

"In order to render such notes or memoranda admissible, it is indispensably necessary that they should be taken on the spot at the time the observations are made, or as soon after as practicable. It is not necessary to their admissibility as evidence, that the observations should be written down by the practitioner himself, provided they are made under his immediate inspection at the time, or at his suggestion, and are soon afterwards looked over and corrected by him. Thus, whenever, at a trial, a medical witness produces notes for reference during his examination, the question is invariably put to him, as to when the notes were made. Their admissibility depends on his answer." (p 105.)

The object of this is to prevent collusion, fraud, or any trimming-up of a case, so as either to benefit or injure the prisoner. Again, the notes used by a witness must be the original, and not a copy made by another person. Lastly, Mr. Taylor states that—

"There is another rule of law with respect to the use of notes or memoranda in evidence, which is not perhaps so generally known to medical practitioners; but

it is essential that it should be observed. The notes may have been fairly made on the spot, in the manner required by law; but when a witness is about to refer to them, he will probably be asked whether he is using them for the purpose of refreshing his memory, or whether he is about to speak only from what is written on the paper, without having any precise recollection on the subject. If he is referring to them for some fact which he has altogether forgotten, then the notes are, *pro tanto*, inadmissible as evidence; for it has been held by our judges, that notes can only be used in evidence for the purpose of refreshing the memory on a fact *indistinctly remembered;* they are not permitted to be used for the purpose of reviving impressions entirely forgotten. The most eminent legal writers lay it down, that if there be any single point in the notes which the witness does not recollect, except that he finds it there written, such point is not evidence. Notes are only allowed to assist recollection, not to convey information." (p. 106.)

In Chapter XII, Mr. Taylor discusses the value of evidence derived from the effects of poisons on animals; and he agrees with Devergie, that experiments performed on dogs and cats are not, in any case, fitted to show the *doses* in which particular poisons are injurious or fatal to man, nor can they be safely trusted to prove the rapidity of action of different poisons. "All that they are fitted for," he says, "is to enable us to ascertain whether a particular substance be injurious to animal life or not, but nothing further." (p. 162.) We think that this is rather a sweeping conclusion; especially for a toxicologist, whose brethren have so constantly appealed to experiments made upon animals, for the sole purpose of determining the *nature* of the action of a poison. The history, in fact, of almost every drug in the materia medica, certainly of every modern one, will show us that the profession has been accustomed to regard the mode of action of any substance upon our domestic animals, as strongly indicative of what its effects would be upon man. Wherein, we would ask, do the effects upon the two classes of animals differ in the cases of opium, arsenic, conia, aconite, oxalic acid, corrosive sublimate, belladonna, carbonic acid, prussic acid? and indeed, we might go through the list of poisons, down to our fashionable anæsthetic agents, ether, sulphuret of carbon, and chloroform. This statement of Mr. Taylor's seems to us based rather upon a knowledge of a few legal quibbles, than upon any sound principle of physiology, or upon any practical inquiry into the matter. Our own experience, derived from a greater number of experiments than we would like to mention, has convinced us that there is a very complete parallel between the effects of poisons upon man and upon domestic animals. Here and there we may see what appears to be a shade of difference, but this difference is never greater than that which has been observed between the operations of the same drug upon different individuals of the same race. The case, however, is different in respect to those animals which are far removed from man by reason of their habits, and the nature of their food; the herbivorous feeders, for example, are not by any means so susceptible of the influence of vegetable poisons as man is, many of these animals being able to partake of enough poison to render their flesh unwholesome, without their systems being at all affected by it.

"Thus the flesh of the pheasant which feeds on the buds of *Calmia latifolia*, in North America, is deemed poisonous during the spring. The flesh of hares which have fed upon *Rhododendron chrysanthum* is considered to be poisonous." (p. 166.)

Snails have been rendered poisonous, by feeding upon the leaves of

Coriaria myrtifolia, and it has been long known that the honey derived from bees which have fed on the flowers of poisonous plants, may become poisonous, although the bees remain unaffected by it. Again, Mr. Taylor refers to the fact, that in some districts of North America, to the west of the Alleghanies, the herbage has no injurious effect on the animals which are there pastured, though their flesh and milk, when used as food, are poisonous to man, producing a disease called the *trembles;* and it is not improbable that the flesh of fish, mussels, and other marine animals, may be occasionally rendered poisonous by the food of which they have partaken.

As Mr. Taylor has not given any very connected account of the existence of certain poisons in the human body, as natural constituents of it; and as this is now a very important affair with the medical jurist, we think that a slight allusion to it may be acceptable to our readers.

It has been asserted by many chemists, that arsenic, copper, lead, and manganese are normal constituents of the human body; let us examine, seriatim, the facts in connexion with this statement.

Is arsenic a normal constituent of the human body? Dr. Christison says:—

"This startling proposition was first advanced by M. Couerbe, and by Professor Orfila soon afterwards. The latter subsequently stated that it exists only in the bones, and not in any of the soft solids. It is now clear, however, that both of these experimentalists must have committed an error. Orfila himself admits that his early researches are vitiated by the subsequent discovery of arsenic in some kinds of sulphuric acid; and all recent attempts by others to obtain his results have failed. Thus MM. Flandin and Danger could not detect arsenic in any part of the human body, when it had not been administered. Pfaff was unable to detect an atom of it in the bones of a man or the lower animals by Orfila's own process; Dr. Rees was equally unsuccessful; and in 1841, a committee of the French Institute, who superintended the performance of an analysis in three cases, by Orfila, reported that he failed in every instance, to find a trace of arsenic, by a process which could detect a 65th part of a grain intentionally mixed with an avoirdupois pound of bones." (p. 281.)

M. Flandin endeavoured to show that the errors committed in the statements originally made by Orfila, arose from the presence of sulphite and phosphite of ammonia in the animal matter upon which he operated. Legrip still thinks that arsenic may exist in the bones, and that the process of incineration usually adopted is sufficient to dissipate the poison. In a few words, however, it may be said that we have had no proof whatever of the normal existence of arsenic in any part of the human body; and yet an opinion to the contrary continues to be propagated in our law circles. Upon this point, Mr. Taylor remarks:—

"It is singular how long an error in medical jurisprudence, when once diffused, will continue to find circulation, although the experiments upon which it was based may have been long since refuted. The refutation of Orfila's opinion, that arsenic existed as a natural constituent of *bone*, took place in 1841; but no case of poisoning by arsenic now comes to trial, in which the most ingenious objections, founded upon his first experiments, are not urged to the chemical evidence of the presence of the poison. Rightly or wrongly, applicable or inapplicable, they are invariably raised by a counsel in defence." (p. 350.)

Does arsenic exist in any article of food or of diet commonly made use of? It is a custom with some farmers to steep corn in a solution of arsenious acid, before it is sown; and it might be supposed that the grain produced

from such seed would contain arsenic, and that it might thus become an accidental constituent of the human body.

"This question has not escaped the notice of chemists. M. Audouard states, that he has detected arsenic in the crop of corn, when the seeds had been previously soaked in a solution of arsenious acid; the poison was, however, in very minute quantity. On the other hand, M. Girardin, by a satisfactory series of experiments, has proved that there is no detectible quantity of arsenic in corn under these circumstances. In some of his experiments he used more than four pounds of corn, and he could not discover in this large quantity the least trace of arsenic." (p. 372.)

These results of Girardin receive confirmation from some experiments of Herberger, who, in the year 1843, grew some of the seeds of *Triticum spelta*, which had been steeped in a solution of arsenic; the parts of the plant were examined before the formation of the stem, before flowering, and also just before ripening; but without giving any evidence of the presence of arsenic. (Jahrb. für Prakt. Pharm., xii, p. 386.) We also have had an opportunity of verifying these results; for in the spring of 1846, a great number of pheasants having been found dead in their preserve, their crops were removed and sent to us for analysis, and with them some young wheat (about six inches high) which had been grown from poisoned corn, and of which it was suspected that the birds had fed. The earth about the roots of the plants yielded distinct traces of arsenic, but the leaves of the plant were perfectly free from it. It may be considered therefore as settled, that arsenic is not a constituent of wheat, and that this poison is not likely to gain access to the human body by such means.

Another article of diet, vinegar, has been found to contain arsenic. This liquid is commonly obtained by the decomposition of acetate or pyrolignate of soda or lime, by means of commercial sulphuric acid; and as the latter is frequently contaminated with arsenic, it may become a source of poison in the vinegar. Deschamps was the first to notice this fact; and it has subsequently been investigated by Chevalier, who found that some of the vinegar of commerce contained as much as 1-2200th of metallic arsenic, a quantity equal to about one fourth of a grain of arsenious acid in each ounce of the vinegar. He says, however, that the vinegar was exceedingly strong, and required to be mixed with five or six parts of water before it could be employed for culinary purposes (Journ. des Chim. Méd. xi, p. 334); so that this admixture would dilute the poison too much for it to exert any injurious influence upon the system.

Lastly, it has been discovered that arsenic is a constituent of mineral waters. Walchner has found it in the sediments of the chalybeate waters of Griesbach, Rippoldsau, Sienach, Rothenfels, Cannstadt, and in the hot springs of Wiesbaden, and the effervescent waters of Ems, Pyrmont, Lamscheid, and Brohl, near Andernach. Figuier has confirmed these observations with respect to the mineral waters of Wiesbaden; he asserts that it exists in the water in the form of arsenite of soda; and that when the protoxide of iron contained in the water is further oxidized, it combines with the arsenious acid, and is deposited as arsenite of iron: he further states that 79 gallons of the water of Wiesbaden yielded, according to his analysis, about 1-100th of a grain of arsenious acid. (Chem. Gaz. vol. v, p. 53.) Chevalier and Gobley have published an account of their examinations of the mineral waters of France, and they report

that arsenic exists in the acidulous and ferruginous cold springs of Royat, Hauterive, Provins, and in the saline hot springs of Vichy, Saint Mart, Bains, Plombières, Mont d'Or, and Bourbonne; likewise in the sediments of the springs of Royat, Provins, Jaude, Saint Mart, Hermonville, Martigné, Briant, and Spa. Dr. H. Will also has examined this question, and he says that 1044 bottles of water from Joseph spring at Wiesbaden, 516·5 from Wenzel spring, and 886 from the Leopold spring, contain respectively 1 grain of arsenious acid. (Liebig's Annalen, lxi, p. 192.) Similar observations have been made by Killer, respecting the Kissengen water, the sediment from which contains abundance of arsenic. Chatin has found it in the spring of Versailles. Flandin in that of Passy; and Tripier has detected it in the mineral waters of Meskontine, in Algiers; and we have discovered it in the sediment of the chalybeate springs at Tunbridge Wells. So that here is a legitimate means by which arsenic might be introduced into the human body. It must, however, be remembered that the arsenic exists in these waters in so small a proportion, that as we have said a person must consume from 516 to 1044 bottles of the Wiesbaden water in order to take a single grain of arsenious acid into his body; and during the time which is necessary to accomplish this, the poison is being eliminated as fast as it is introduced into the system; for arsenic, according to the experiments of all toxicologists, is not an accumulative poison. We have no occasion to fear, therefore, that the tissues of the human body will become impregnated with arsenic, either in this manner, or by any other fair means.

Is copper a normal constituent of the human body? This is another very important question to the medical jurist, and we must differ from Mr. Taylor, who says that, "practically speaking, it has no force." It is very probable that we shall have an opportunity of judging of the truth of this observation before many weeks have gone by; for we predict that the unfortunate occurrences at Northampton will bring many of the facts connected with this question into a very prominent position. Our author ranges himself with those chemists who do not admit that copper is a normal constituent of the human body; and with him we find Chevreul, Danger, and Flandin. On the other side, are Orfila, Devergie, Henry, Sarzeau, Barse, Rossignon, Boutigny, Legrip, Bertozzi, Heller, Braunson, Besanez, Millon, Deschamps, Harless, and we may add Christison, for he concludes his remarks upon this question by saying:

> "On the whole, whatever may be thought of the physiological question, whether copper forms a constituent of the textures and fluids of vegetables and animals, it seems well established that this metal is often present there in minute proportion; and consequently its possible presence must not be overlooked in medico-legal researches." (p. 459.)

All of these chemists, excepting Dr. Christison, have detected copper in the fluids and tissues of man or animals, none of which had been poisoned by it. Orfila has frequently met with it in the human body. Barse discovered it in the bodies of two subjects taken from the hospitals of Paris, both of whom had died from ordinary disease. And M. Rossignon, in September 1843, addressed a note to the French Academy, stating that he had recognised copper in the blood, muscles, and certain secretions of man and animals. Bertozzi found copper in very large quantities in the coloured biliary calculi. Heller has confirmed his results, and has

extended his researches to the human bile; he declares that he has frequently found this metal in the bile of adults, but never in that of children. (Heller's Archiv, 1845.) Braunson, and Gorup-Besanez have obtained the same results. (Buch. Repert. xlii.) More recently M. Millon has detected it in the human blood; but this is only in confirmation of some early experiments by Sarzeau. The process whereby Millon is enabled to demonstrate its presence, is rather peculiar. He receives the blood from the vein into about three times its bulk of water, and pours the mixture into a bottle containing chlorine gas. On agitating it, the blood immediately coagulates, becoming brown, and finally gray: the clear liquor is then filtered off, and tested for copper, &c. He believes that the copper is, like the iron of the blood, attached to the globules; and he thinks it is probable that it may, like that metal, participate in some physiological change. (Acad. des Sciences, Jan. 10, 1848.)

Some facts, giving a strong support to these views, have been made out by Harless, who, in his examination of the blood of the *Ascidia* and *Cephalopoda* generally, noticed that it became deep blue on exposure to the air. He had some of the blood of the *Eledone* sent from Trieste to Nuremberg, and on analysis by Von Bibra, it was found that the ash contained 24·9 per cent. of phosphate of lime and copper. Iron seemed entirely absent, and it appears to have been replaced both here and in the liver by copper; he found in the ash of the liver 1·12 per cent. of metallic copper. Harless has considered the probable source of this metal in these creatures, and he says it is impossible that it could have been derived from the sheathing of ships, for the animals were caught at a distance from any harbour in which ships lay; he concludes, therefore, that it must have been taken with their food. He has also found copper in the *Cancer pagurus*, *Acanthias zeus*, *Helix pomatia*, and *Conger vulgaris*. (Müller's Archiv, No. ii, 1847.)

M. Legrip has inquired into the proportions in which copper exists in the tissues of man and animals; and he states that 500 grammes (= 16 oz., troy) of a mixture of two parts liver and one spleen, contain 0·0045 grm. (= ·07 gr.) of this metal; and that 1000 grms. (= 32 oz., troy) of a similar mixture of the tissues of the cow, contain 0·0082 grms. (= ·1266 gr.) (Journ. de Chim. Méd., Mai 1847). So that, upon bringing together the various facts connected with this question, we do not think there can be any doubt respecting the normal existence of copper in the tissues of the animal body.

Another question arises as to *its source*. It appears, from the inquiries of Heller, Devergie, &c., that the quantity of copper in the tissue increases with the age of the animal. This fact renders it probable that the metal is derived from the food. Meissner noticed, some time ago, that vegetables yielded a trace of copper; and Sarzeau has detected it in wheat, coffee, flour, sugar, and cheese. More recently, Girardin, Deschamps, and others have confirmed the results obtained by Sprengel, Boutigny, and Vever, who noticed that plants cultivated upon soils containing copper, became impregnated with it. Girardin states, that corn raised from seeds which have been steeped in solution of sulphate of copper to destroy fungi, always contains an appreciable quantity of copper; while that produced from seed not so prepared exhibits only a trace of the metal. (Comptes Rendus, Dec. 15, 1845; and Académie des Sciences, Jan. 1848.) This

fact is strikingly opposed to the one made out by the same experimenter, viz. that corn grown from an arsenical grain does not contain poison.

Dumas refers in his lectures to the presence of copper in wheat; and Orfila made some observations, last summer, before the French Academy of Sciences, in confirmation of this fact; adding, that he judged, from the quantity of corn consumed in France, that no less than 3650 kilogrammes (= 8042 pounds, avoird.) of metallic copper annually found their way into the bodies of Frenchmen. (Med. Gaz., vol. iv, p. 773.)

Another source of copper is in the vessels in which food is prepared. It happens that when copper vessels are not kept well polished, or when the articles of food prepared in them contain any sour or fatty matter, the copper is rapidly acted upon, and the food rendered unwholesome. Wildberg mentions a case in which a lady and her daughter were speedily killed by making use of some sour-krout which had been kept for two hours in a copper vessel. Dr. Christison and Mr. Taylor give many instances of poisoning from similar negligence.

Wines and vinegar also will act upon copper, and so will lard, oil, or butter. Copper is frequently contained in the fine green pickles of our London grocers; and Rossignon declares that he has detected it in the gelatin supplied for soup to the Hospital of Saint Louis. He has also met with it in sorrel, chocolate, bread, coffee, chicory, sugar, barley-sugar, &c. It has likewise been discovered by Walchner, Lignier, and Will, in conjunction with iron and arsenic in mineral waters (op. cit.); from all which facts it would appear that there are many channels whereby copper may be conveyed into the animal body.

With respect to the normal existence of lead in the tissues, toxicologists are not prepared to offer any very positive opinion. It is stated by Orfila, that it is a natural constituent of the body; and this statement is supported by the experiments of Devergie, Barse, Millon, and Legrip. Taylor, however, together with Christison, Danger, and Flandin, are of an opposite opinion.

Legrip states that he has found lead in the mixed tissues of the liver and spleen of man and animals, even when they had not been exposed to its poisonous influence. From 500 grms. (= 16 oz., troy) of these tissues from man, he obtained ·0027 grm. (= ·0417 gr.) of metallic lead, and from 1000 grms. of the same tissues from a cow, he procured ·0032 grm. (= ·0594 gr.) of it. (Op. cit.) Millon also states that it is easy to demonstrate the presence of lead in the human blood by means of the process to which we have alluded; and Gorup Besanez had reason for thinking that he had met with it in the bile of the ox. (Buch. Rep., xlii.)

These observations, however, are too few in number, to permit of any conclusive opinion upon such an important subject; nevertheless it is very probable that lead is a common constituent of the human body, as there are so many means whereby it may be introduced into it. For example, a large number of the community are in the habit of drinking water contaminated with lead; others make use of the poison in the form of less common beverages, as adulterated wines, spirits, &c.; and then, again, many persons are engaged in occupations whereby the system may receive lead into it, without being actually poisoned.

Manganese is another metal which has been discovered in the tissues of vegetables and animals. Millon has recognised it in rather large

proportion in human blood (Müller's Archiv, ii, 1847); and, more recently, Drs. Gundelach and Strecker have met with manganese in the bile of the pig. (Liebig's Annalen, lxii, p. 203.)

It has also been detected in the ashes of many vegetables, through which it may perhaps gain access to the animal body. The salts of manganese, however, are not ranked among the poisons; and, from the peculiarity of their chemical reactions, they are not very likely to be confounded with any of the mineral poisons.

We now find that we have devoted so much space to the discussion of what may be termed the general principles of toxicology, that we have but little room left for the consideration of the remaining portions of Mr. Taylor's work, namely, those which refer to the description of the individual poisons. These portions constitute about three fourths of the volume; and a very superficial examination of them will show that they have been collected from a large store of materials. In fact, every division is crowded with illustrations; and this redundancy almost becomes a fault, for it often encumbers the subject, and demands the exercise of a great deal of discretion by the reader, in order that he may make choice of those facts which are necessary to a right conclusion. Another fault which may be found with Mr. Taylor, and by which he has increased the number of his pages, is the frequent citation of the same fact, or of the same case. He refers, for example, no less than four times to the opinions of Orfila, Danger, and Flandin, upon the way in which arsenic is eliminated from the body. And he is so pleased with the idea of arsenic being a constituent of the nails of coffins, that he has thought it necessary to mention it more than once. Again, the frequency of his allusion to the case of John Tawell is very remarkable; we have met with it sixteen times, and we have been curious to know what could be the occasion of the author's frequent reference to it. As we could not help noticing that, wherever it was brought in, it was made the subject of censure and crimination, it struck us as being an act of persecution. At pages 650 and 679, however, the cause of all this became evident, and we then discovered what Mr. Taylor would call the "*corpus delicti.*" It appears thus:

"Some objection was taken at the trial of *Tawell* to a very short abstract of a case by Mertzdorff, reported by me in a former work. (Man. Med. Jur.)" (p. 650.)

"It is to be regretted, however, that he" (referring to Sir F. Kelly, the counsel for the defendent) "took upon himself to pronounce as false those reports of cases published in medico-legal works, which went to show, that, owing to various circumstances, the odour of prussic acid might not be always perceptible in the stomach of a person poisoned by it." (p. 679.)

These objections, made by Sir Fitzroy Kelly, have evidently created an unpleasant feeling in the mind of Mr. Taylor, and he has taken care to lose no opportunity of giving vent to it. We, however, cannot blame the learned advocate, as our author does, for the part which he took in that defence; on the contrary, we are disposed to think that he acted well, and that his conduct was in strict accordance with his professional duties. For, when he found, after an examination of the witnesses, that the body of Sarah Hart presented none of the evidences of poisoning; that the two medical men who conducted the post-mortem examination had

originally thought, but upon no good grounds, that she had died from oxalic acid; and when, moreover, he found that the medical witnesses had never had the slightest experience in any case of this kind, but that they had entirely based their conclusions upon the brief reports of a few cases, mentioned in the works of Christison and Taylor; it was natural that he should have had many misgivings as to the value of their evidence, and have consequently felt it to be his duty to ascertain whether the cases, here alluded to, were recorded fully, fairly, and honestly. Upon finding that they had not this value, but that both Christison and Taylor had committed grave errors in their reports of them, we think that he was justified in rejecting them as evidence, and in passing a severe censure upon those authors whose carelessness might place the life of a fellow-creature in the utmost jeopardy. At page 679 of the present work, Mr. Taylor has made an attempt to qualify these errors, but without any great success; his reasoning, in fact, appears more like that of a special pleader, than of an ingenuous author, who has the honest wish to set himself right with his reader. Nor can we admit that "the whole of the defence" set up in the case of Tawell, "was involved in a mass of contradictions;" for, on reviewing the facts of the case as they were presented to the consideration of Sir Fitzroy Kelly, we find—1. That there were no post-mortem appearances indicative of death by poison. 2. That the medical men had not perceived any odour of prussic acid, either in the body of the deceased, or in the contents of the stomach; although Mr. Champneys had smelt at her mouth immediately after death, and Mr. Cooper, with the other medical witnesses, had been heating, stewing, and boiling the contents of the stomach for hours. 3. That the contents of the stomach were analysed, without any poison being detected, until Mr. Cooper came to the sediment in the bottle (which sediment consisted, according to his evidence, of half-digested apples), and there he found a small quantity of prussic acid. 4. That prussic acid may be *made* and obtained by the distillation of certain parts of apples: this was proved from the evidence of one of the crown witnesses, who had actually obtained more prussic acid from a pennyworth of apples than Mr. Cooper had done from the dregs in question; and in confirmation of the principle involved in this fact, the advocate received additional assurance from Professor Graham, Dr. Letheby, and Mr. Herapath, who were present at the trial. 5. That, having obtained a very small quantity of prussic acid from the sediment in the bottle, Mr. Cooper *calculated* that one grain of it had existed in the whole of the contents. 6. That the medical witnesses had had no experience whatever in cases of poisoning by prussic acid. And, 7. That they had founded their opinions upon cases which were proved to have been incorrectly reported.—We hardly know how a defence, based upon these considerations, can be said "to involve a mass of contradictions," or how it can be affirmed "that the advocate was arguing against incontrovertible facts;" nor can we admit that "the cause of death was obviously prussic acid," and that "a struggle was made to show, on the most untenable hyper-chemical and hyper-pathological grounds, that the deceased had died from apoplexy or some other sudden death." (p. 63.)

Such statements convince us that Mr. Taylor is not only prejudiced against the case, but that he has also been misinformed upon it, and we

will quote from page 41 in further proof of this. He says, when speaking of the influence of disease over the action of poisons :—

"A most absurd degree of importance appears to have been attached to this modifying condition in Tawell's case. It was, as I am informed, alleged that epilepsy would increase the effects of prussic acid, so as to render this poison more rapidly fatal in a smaller dose."

But as far as we have been enabled to learn from the best authorities upon the matter, there was not anything of the sort alluded to in the defence; and even if there had been, its import would not have been so absurd; seeing that Mr. Taylor does himself admit, that, "as a general principle it may be affirmed, that whenever the body is much debilitated by disease. poisons acquire greater virulence of action," (p. 41 ;) and again, when speaking of the action of opium, he states that "very large doses may be borne in cases of hydrophobia, colic, delirium tremens, and tetanus, while the effects of small doses are aggravated by disease of the brain." (p. 595.)

The following also is another point upon which we think he has been misinformed. He says, with reference to the generation of prussic acid from the sulphocyanide which exists in the saliva, and may also be present in the contents of the stomach :—

"This hypothetical objection was taken at Tawell's trial, to the chemical evidence, but as the above facts show, there is not the slightest foundation for it. If it were true, a human stomach would always contain prussic acid from the reaction of the muriatic acid in the gastric secretions on the sulphocyanide contained in the saliva." (p. 697.)

Altogether it strikes us, that Mr. Taylor has often gone a great deal out of his way, to discuss various points connected with Tawell's trial; and in so doing he has never for one moment considered that, at the time when the case was argued by Sir Fitzroy Kelly, it was involved in the greatest doubt, and that every point of it had to be cleared up. But to leave this subject, which, perhaps, has occupied too much of our attention, as well as of Mr. Taylor's, we will only say, that his comments upon it would have told much better, had they been put forth with less acrimony and more judgment.

And now, at the close of our review, we have merely to repeat that Mr. Taylor's work contains a vast body of facts, which embrace all that is important in toxicology, all that is necessary to the guidance of the medical jurist, and all that can be desired by the lawyer. In fact, we opine that its chief fault lies in its *too great* accumulation of materials, without due discrimination as to their relative value; for this becomes a source of perplexity to all save the most experienced toxicologist; whilst it furnishes the lawyer with a store of cases out of which he may glean enough to give probability and support to every possible kind of opinion. There is no class of works, perhaps, in which scrupulous accuracy, lucid arrangement, and sufficiency without redundancy of information, are so much required, as in treatises on toxicology : for the tendency of the forensic advocate, to direct all his proceedings by "authority," makes him attach an undue weight to every assertion of those who have gained a reputation for their toxicological skill; and the most competent witness is liable

to have his statements continually brought to the test, not of direct experiment, but of the dogmata of Christison and Taylor, Orfila or Devergie.

It is the peculiar duty of the critic, therefore, to point out what he considers to be faulty in such works; and we trust that in our discharge of this, we shall not be thought to have stepped beyond the bounds of that courtesy which Mr. Taylor has a right to expect at our hands. Taking it altogether, the work is one which reflects the greatest credit upon his industry and zeal; and whilst we think it far from perfect either in plan or execution, we could not point to any other treatise, British or foreign, which so correctly represents the present state of toxicological science.

ART. IV.

1. *The Plant; a Biography. In a Series of Popular Lectures.* By M. J. SCHLEIDEN, M.D., Professor of Botany to the University of Jena. Translated by ARTHUR HENFREY, F.L.S., &c., Lecturer on Botany at St. George's Hospital. With Five coloured Plates and Thirteen Wood Engravings.—*London*, 1848. 8vo, pp. 365.
2. *The British Desmideæ.* By JOHN RALFS, M.R.C.S. The Drawings by EDWARD JENNER, A.L.S. With Thirty-five coloured Plates.—*London*, 1848. 8vo, pp. 248.

THE study of vegetable physiology has of late been prosecuted with much zeal and success, both in this country and on the Continent; and special attention is being paid to those low and simple forms of vegetable existence, which present the phenomena of life in their least complicated form, and which exhibit, in the condition of single, isolated, independent cells, all that can be regarded as essential to our ideas of vitality and organization.

The importance of studying animal and vegetable physiology in connexion with each other, and the mutual assistance they are capable of rendering, were first enforced, in this country at least, in Dr. Carpenter's 'Principles of General and Comparative Physiology;' and since the publication of that work, the whole tendency of scientific investigation has been to bear out and extend that view. For it is now seen that not only is the little Protococcus (or any other equally simple plant) the type of the most complex *vegetable* organism, not only does the single cell of which it consists perform all the functions of the most perfect plant, but it is equally the representative of all those portions of the *animal* structure which are the most active instruments of its vital functions; and thus the history of its development, growth, propagation, and final dissolution, is being repeated (in all essential particulars) in every organized fabric. This view is thus recognised and expressed by Prof. Schleiden:

"Regarding the vegetable kingdom as a whole, as an individual, the various stages of life and development of which lie as close *beside each other*, as they follow *after one another* in a single plant, we are enabled to regard the simplest form as also the commencement of the vegetable world; and then we find that this, like the individual plant, is produced and developed from a single cell. When on old damp walls and palings, or in glasses on which we have let soft water stand for several days in summer, we find a delicate bright green and often almost velvety

coat, we meet with the first beginning of vegetation. Under the microscope, we detect in these green masses a number of small spherical cells, filled with sap, colourless globules and chlorophyll. In other places occur similar cells, but yellowish, brown, or red; and almost all, at least at present, may be regarded as perfect plants, which have received various names from botanists. The most suitable name for them is Protococcus, primary vesicle. From this simple cell, vegetating as an independent plant, the development of the vegetable world takes its departure, and ascends by continually greater combinations and complications, to the most complex plants, which we are compelled to look upon as the highest states, although the uninitiated may think it strange when I name as a representative of this highest expression of vegetable development, the little, common, and therefore despised, daisy.

"The forms immediately following the above-mentioned simplest plants also consist of a simple cell, but this is elongated into a filament, and often branched, thus exhibiting a higher development of form; next, the cells arrange themselves into lines in manifold ways; a variety of forms of vegetation soon grow up, which in water appear as the *Silk-weeds* or *Confervæ*, generally of a green colour, or on decaying organic bodies as moulds, in very various and often most elegant forms, with the most brilliant play of colour. Then the cells unite to compose flat structures, known to botanists by the name of *Ulvæ*, and frequently growing in the sea, almost like young lettuce-leaves, sometimes green, sometimes red, often afford a meagre meal to the poor inhabitants of the coast. Next they crowd together in solid masses, forming clumps and balls of the greatest possible variety of shapes. New commences an unfolding of richer and more varied forms than were possible before in the simple element; but the differences of development in length and breadth, or length, breadth, and depth, are especially frequently repeated in the lower stages of the vegetable world in the individual groups, and in the higher stages in almost all the individual organs." (pp. 93-4.)

The beautiful work of Mr. Ralfs is entirely devoted to the enumeration and description of the British forms of a single group of the simpler Algæ; a group which has been included in the animal kingdom by Professor Ehrenberg, but of whose vegetable character we do not think that any reasonable doubt can now be entertained. Upwards of two hundred and fifty species of this single tribe are known to inhabit the pools, ditches, and other collections of stagnant or slow-moving fresh water in our own country; and as the minute attention of naturalists has only been recently directed towards it, and as one third of the number of species now known have been discovered in this country since the prospectus of Mr. Ralfs's work was issued, it is scarcely to be doubted that a rich harvest still remains for future discoverers. Like other organized beings whose conditions of existence are very simple, they are usually *cosmopolite* in their distribution; nearly every British species having been found in France, and most of them having American habitats also. For our acquaintance with nearly all the plants of this group, we are dependent on the microscope; very few of them being discernible by the naked eye, and the greater number being of extreme minuteness. Most of them usually exist as isolated cells, except when they are undergoing the process of duplication; but, in the genus Desmidium and some others, the cells generated by this doubling process remain in connexion, and form a continuous filament. One of the chief characteristics of the group is, that each cell or joint is composed of two symmetrical valves, the junction of which is always marked by the division of the endochrome (or green cell-contents), often also by a constriction, which is frequently of considerable depth,

so as almost to give to the body the appearance of being composed of two cells instead of a single one. This subdivision, however, is so imperfect in some cases, as scarcely to distinguish the plant from the Protococci and other simple forms constituting the family Palmelleæ; and the close relation of the two has been recently made still more apparent, by the discovery that the phenomenon of *conjugation* is common to both. To this phenomenon we wish to take the present opportunity of directing the attention of our readers; as there now appears strong reason for no longer regarding it as a peculiar and exceptional occurrence, but as presenting the sexual operation in its very simplest condition.

When adverting on a former occasion (vol. i, p. 193) to the reproductive process as performed by the simpler Algæ, we described the two methods then known to exist amongst them, namely, multiplication of cells by subdivision, by which a filament or leaf-like expansion may be produced by a process of continuous growth from a single cell, or, in the lowest tribes, a succession of generations of cells that become detached and independent; —and multiplication by the setting-free of germs, formed within the parent cell, which have in many cases an active movement whereby they are dispersed, and are then known by the name of *Zoospores*. There is a tribe of thread-like Algæ, common enough in our fresh waters, to which the name of *Conjugatæ* has been given; from the circumstance that, at certain times, the cells of two filaments lying side by side form mammillary projections, which meet and unite; the partition between them breaks down, so that a free communication takes place between the corresponding cells of the opposite filaments; the endochrome of one cell passes entirely into the cavity of the other, or, in some instances, the endochrome of both cells passes into a common receptacle, which is formed between them; and from this mixture of endochromes is formed a new body, termed the *sporangium*, which is believed to be a reproductive body giving origin to the first cell of a new generation. In some species of the same tribe, this conjugation or mixture of endochromes, followed by the production of a sporangium, is believed to take place between adjacent cells of the same filament, instead of between cells of two distinct filaments. But the conjugating process, though first observed in the *Zygnema* and its allies, is by no means confined to them; being now known to take place so generally through the group of *Desmideæ*, as to be entitled to be ranked as one of its distinctive characters; whilst it has been observed also in the *Coccochloris*, one of the simplest cellular Algæ, nearly allied to the Protococcus, and will probably be detected in the other members of the same group. Moreover, it has been recently discovered by Mr. Thwaites to take place in the *Diatomeæ*;* and this circumstance will probably be regarded as quite decisive with regard to the vegetable nature of that very interesting group, notwithstanding the arguments of Professor Ehrenberg and his followers in favour of their animality. Having ourselves had the opportunity of examining Mr. Thwaites's preparations, we cannot feel the slightest hesitation as to the correctness of his description; whilst, on the other hand, having also had the opportunity of inspecting the preparations which are regarded by Professor Ehrenberg as conclusively demonstrating the existence of a gastric apparatus in these beings, we feel bound to state that nothing, in our opinion, save the force of a preconceived idea, could have led to such an inference from the appearances which they

* Annals of Natural History, vol. xx.

present. In his last paper on the subject (op. cit. Mar. 1848), Mr. Thwaites explains his views as to the process of conjugation generally, and points out the claim which it has to be regarded as analogous to the sexual operations of the higher plants. These views may be thus epitomised.

The primordial cell of a flowering plant begins by developing new cells like itself, by a process of continuous growth. These cells, however, instead of being all alike, after a time begin to present certain differences, whereby the several organs of the plant are evolved. The whole progeny of the primordial cell thus enters into the composition of a single structure, whose heterogeneous parts are mutually dependent upon each other. The same mode of increase continues, until the individual is fully evolved; and it may also produce a multiplication of individuals by the process of budding, each bud being capable of maintaining its own existence when detached and placed in favorable conditions. But there is a limit, in most cases, to this method of increase; and the race would die out, unless some other mode were adopted to infuse fresh vitality into the new generation. This method essentially consists in an intermixture of the contents of two cells, both of which may have been the offspring of the very same primary cell; these two cells being the pollen-grain, and the embryonic vesicle of the ovule. From this admixture originates the embryo, which goes through the same set of changes as that just described. It is comparatively rare in the vegetable kingdom for this sexual union to take place between organs that do not form part of the same individual organism; the small tribe of diœcious plants constituting the only exception to the rule.—Now in the simple Algæ, also, the first cell developes many others, by self-multiplication; but the new cells thus produced become independent plants, and may separate from each other, and maintain an isolated life; each cell possessing all the conditions for its own continued existence, instead of being bound in a relation of dependence to its fellows. Mr. Thwaites considers that the various cells which have originated from one primordial cell ought to be regarded, not as so many individuals of a species, but rather as parts of one individual; in making this use of the term *individual*, however, we should not, we think, act in conformity with its ordinary acceptation. No single cell of the embryonic mass of the higher plant can be accounted as a distinct individual, because it cannot continue to exist and grow, if isolated from the rest of the structure, all parts of which are bound together in a relation of mutual dependence. But all the cells of a Desmidium or of a Coccochloris, which have descended from a common ancestor, do possess the power of maintaining an independent existence, and must be regarded as distinct individuals, unless a new meaning is to be given to the term. We would suggest to Mr. Thwaites to designate as *co-individuals* those which have sprung by continuous growth from one primordial cell. Now this method of increase, it would seem, cannot go on indefinitely; for, as in the flowering-plant, it would appear that a union of the contents of two cells is necessary to give origin to an entirely new generation. The process of conjugation between two cells of a *Zygnema*, a *Desmidium*, or a *Diatoma*, is thus to be regarded as analogous to the fertilization of the ovule by the pollen-grain in the flowering plant. It is no sufficient objection to this view, to say that the two conjugating cells are usually to all appearance so exactly alike, that it is scarcely conceivable that one should perform the function of the male, and the other that of the female. For

indications of a difference do present themselves in certain cases; and even if there were none, the hypothesis would not be invalidated, the great mutual resemblance of *all* the cells of these tribes being considered. In several species of Zygnema, some of the filaments consist of cells, into which the endochromes of all the cells of the other filaments discharge themselves, and within which the sporangia are uniformly produced. These would seem to be analogous to the ovules or female organs; whilst the other filaments, all whose cells discharge their endochrome, would seem to represent the pollen-grains or male instruments of fecundation. And bearing in mind the fact that, in these simply constructed beings, there is no specialization of root, stem, leaf, or flower,—every individual cell performing by and for itself the functions of all these diverse parts,—it is scarcely to be expected that there should be any greater specialization of male and female cells than that just indicated. In the majority of instances, however, the sporangium is not formed within either of the parent cells; but its production takes place either in the hollow of the bridge that connects them, as in some of the Conjugatæ; or is an altogether new growth exterior to both, as in the Desmideæ, Diatomeæ, and Palmelleæ.

If, then, the union of the contents of two cells is necessary for the production of a true embryo, even in these low and simple plants, the question naturally suggests itself,—in what light are we to regard the *zoospores* which are set free from the interior of the cells in many species, in a manner that reminds the observer of the *swarming* of bees? Mr. Ralfs suggests that they are probably to be regarded as detached *gemmæ*, analogous to those of *Marchantia polymorpha* and other plants; and in this opinion we are much disposed to agree with him, more especially since they would be remarkably analogous also to the free polypoid gemmæ of Tubularia and other zoophytes. Mr. Ralfs offers the following conjecture as to their purpose:

"I would suggest that they possess merely a limited vitality, which is destroyed, unless they are at once placed in circumstances favorable to their growth, whilst, on the other hand, in the conjugated cells some important change takes place during the the commingling of their contents and the formation of the sporangium, like what happens in the production of seeds in general, which renders the sporangia capable of retaining the vital principle uninjured throughout long periods of drought." (p. 11.)

The recent extension of our knowledge regarding the existence of the conjugating process in these humble plants, obviously leads to the idea, that, as the essence of sexuality is thus met with in the lowest and simplest members of the vegetable kingdom, as well as at the opposite extremity of the series, all the intermediate forms of Cryptogamia will also be found to possess it. There are numerous facts which seem most readily explicable on this theory; and it may prove that the contrary doctrine held by most of the principal botanists of our time is founded upon insufficient observation, and that the old Linnæan term *cryptogamic* is really a more correct expression of the character of the reproductive process in these plants, than the term *agamic*, which some have proposed to substitute for it.

It would be quite foreign to our purpose to enter into a minute critical examination of Mr. Ralfs's work; but we feel great pleasure in bringing it under the notice of our readers, as a valuable contribution from a member of our own profession to a department of botany peculiarly fertile

in objects of interest to the philosophic naturalist, who finds therein the simplest and clearest manifestation of phenomena that lie at the very foundation of all his ideas of vital operation, and who thus obtains a guiding clue, as well as a definite object, in his analysis of those more complex conditions under which they are elsewhere presented to him. And we must not omit to add that the whole "getting up" of the book reflects the highest credit on all the parties concerned in its production; especially as regards the drawing, engraving, and colouring of the plates; which are fully equal to any representations of similar objects in the best continental works, such as those of Ehrenberg, Kützing, and De Brebisson.

The other work which we have now to take into hand, is of a very different stamp, and of a very different kind of excellence. The name of Professor Schleiden is well known in this country, in association with that of Schwann, in connexion with the development of those great doctrines of cell-life, which now constitute the very foundation of physiological science. Curiously enough, however, the discoveries on which Professor Schleiden's general reputation chiefly rests, are not admitted by the generality of observers as ascertained truths;—we allude to his account of the mode of origin of vegetable cells from free cytoblasts, which, if true at all, is of rare occurrence, and is far from being a general expression of the history of vegetable cell-development;—and also to his doctrine that fecundation consists in the implantation of a pollen-tube in the ovule, the embryo being really developed within the former; a statement that is vehemently opposed by many botanists of great eminence, and must at any rate be still considered as *sub judice*. Judged solely by these claims to distinction, therefore, the reputation which he has acquired from his share in the development of the cell theory might be somewhat unstable. But he has fully established his position as one of the most eminent botanists of the present day; and any work proceeding from him is deserving of respectful attention. We trust to have, ere long, the pleasant task of commenting on his large work on 'Vegetable Physiology,' a translation of which, by Dr. Lankester, has been for some time promised. Meanwhile, we are very glad to receive this agreeable production of his pen, which the scientific botanist and the general reader may alike peruse with pleasure and profit. This is a merit which so few treatises possess, that we may be fairly called on to explain the nature of it; we prefer letting this appear, however, in the brief account which we shall give of the character and contents of the volume.

The treatise consists of twelve lectures, which were "composed during the course of the last eight years, at the instance of an intelligent and cultivated circle, free from 'the dust of the schools,' and were by no means intended for publication." Each lecture is devoted to a special subject, and may generally be considered as complete in itself; an advantage, however, which is somewhat counterbalanced by a certain deficiency of continuity in the whole series, such as could not, perhaps, have been attained without a sacrifice of the merits of the individual portions. The following is Professor Schleiden's own account of his purpose, which should be constantly kept in view in the perusal of the volume:

"My chief aim was, in fact, the satisfaction of what may be called a class-vanity. A large proportion of the uninitiated, even among the educated classes, are

still in the habit of regarding the botanist as a dealer in barbarous Latin names, a man who plucks flowers, names them, dries and wraps them up in paper, and whose whole wisdom is expended in the determination and classification of this ingeniously-collected hay. This portrait of the botanist was, alas! once true; but it pains me to observe that now, when it bears resemblance to so few, it is still held fast to by very many persons; and I have sought, therefore, in the present discourses, to bring within the sphere of general comprehension the more important problems of the real science of botany, to point out how closely it is connected with almost all the most abstruse branches of philosophy and natural science, and to show how almost every fact or larger group of facts tends, as well in botany as in every other branch of human activity, to suggest the most earnest and weighty questions, and to carry mankind forward beyond the possessions of sense, to the anticipations of the spirit." (p. 2.)

It is in the highly *suggestive* character of these lectures, and the picturesque form in which the materials are disposed, that their chief merits consist. We may safely say that in no really scientific treatise with which we are acquainted, have the beauties of Nature been ever so attractively displayed. It would be difficult to convey to our readers any idea of the character of the work by analysis or extracts; and we must content ourselves with indicating the subjects of the several lectures, and with giving a few quotations which may serve as specimens of their style.

The First Lecture treats of the "Eye and the Microscope" in their relation to botanical investigation; a subject sufficiently trite, but discussed in a style which savours so little of the schools as to be quite refreshing. Take the following as a sample:

"On the 26th of January, 1843, a great crowd collected at the Round Down Cliff, near Dover, in anxious expectation to witness the event of the grandest and most daring blasting ever attempted by the skillful combination of human ingenuity. The labour of years had been expended on the preparations, in the opening of shafts and galleries. The largest quantity of powder ever yet used, 185 cwts., was ignited at once by means of a gigantic galvanic battery. Almost in silence was the enormous cliff hurled into the sea; in one minute were a million tons of chalk torn away, and a surface of almost fifteen acres covered twenty feet deep with the fragments. From this may be estimated the tremendous force which must have been exerted. And with what did the power of the human mind enter into this gigantic struggle? With the remains of creatures, a thousand of which may be annihilated by the pressure of a finger. We wonder, and ask ourselves: What does 'small' mean in Nature?

"There can, however, be no possible doubt that it indicates a most barbarous age, or a very low state of refinement, when the value, the importance of a thing is measured by great and small; a standard indeed which finds no application in all that we know most essential and valuable, for the human mind is not to be defined by foot, inch, or line. Physical magnitude imposes only on the sensuous nature; cultivated man seeks to know the object of his contemplation perfectly in all its relations; and then only, from the perfect knowledge, does he permit himself to judge as to the essential and unessential; very frequently this leads him to declare that most significant which has the smallest dimensions." (p. 36.)

The Second Lecture, on the "Structure of plants," thus opens:

"As we watch a clever juggler, exhibiting the magic-like operations of his art, we become gradually quite lost in amazement, until at last he elicits from us the expressions of admiration which are the usual accompaniments and reward of his success. But if we are then allowed to walk on to his stage, in the strictest sense of the phrase 'to look at his cards,' how our amazement fades away when we become aware of the complicated preparations required, of the many aids which must

be at hand—in a word, of the various and abundant means he must make use of, to bring about results which yet after all have no relation to the means employed. And taking a wider field, when we look around us on all the circumstances of life, do not we soon find it to be a characteristic feature of the circumscribed position of man, that his boldest efforts attain, at last, to little or nothing; that when he has availed himself of all the assistance which talent and favouring circumstances afford, he must in the end confess, that what he has obtained by all this toil and labour is but a small recompense for the outlay?

"Nature offers a direct contrast to this. Accustomed, from our youth upward, to see her works outspread before us in eternally renewing riches, we commonly pass them coldly by. The contemplative mind is attracted by her, and begins to divine, with a kind of softened terror, the mysterious powers in action round us. With what wondrous means, we think, must not this great artist be provided! What wondrous chains of powers yet unknown, must there not be hidden in her bosom! Science seeks the solution of this enigma, and in trembling assumes its task, fearful lest perhaps human intelligence be unequal to comprehend and grasp a complexity so marvellously interwoven; and the further we penetrate, the greater waxes her amazement. Every step brings us to a simple solution of an entangled question; every compound phenomenon directs us back to simpler causes and forces, and our astonishment becomes at last converted into devout adoration, when we behold with what small means Nature attains the most stupendous results. By the simple relation that bodies in motion have a mutual attraction, Nature arches over us the whole starry heavens, and prescribes to the sun and its planets their undeviating courses." (p. 42.)

But we need not ascend to the stars to become cognizant of this truth. It is equally evident, to those who will take pains to seek for it, in the organized beings with which our own fair earth is peopled; and the microscope, in furnishing us with the means of discovering in the vegetable world the point of departure of the general theory of organization, has rendered a service to science not inferior to that which the telescope has conferred in bringing beneath our ken the invisible world of space. Our author thus introduces his account of the phenomena of cell-growth:

"From the slender palm, waving its elegant crown in the refreshing breezes, high aloft over the hot vapours of the Brazilian forests, to the delicate moss, barely an inch in length, which clothes our damp grottos with its phosphorescent verdure, from the splendid flower of *Victoria Regina*, with its rosy leaves cradled in the silent floods of the lakes of Guiana, to the inconspicuous yellow blossom of the duckweed on our own ponds—what a wonderful play of fashioning, what wealth of forms! From the six thousand years' old Baobab, on the shores of Senegal, the seeds of which perhaps vegetated before the foot of man trod the earth, to the fungus, to which the fertilizing warmth of a summer night gave an existence which the morning closed,—what differences of duration! From the firm wood of the New Holland oak, from which the wild aboriginal carves his war club, to the green slime upon our tombs, what multiformity, what gradations of texture, composition, and consistence! Can one really believe it possible to find order in this embarrassing wealth, regularity in this seemingly disorderly dance of forms, a single type in these thousand-fold varieties of habit? Till within a few years of the present time, indeed, the possibility was not yet conceived; for, as I have before remarked, we may never expect to be enabled to spy into the mysteries of Nature, until we are guided by our researches to very simple relations. Thus we could never attain to scientific results respecting the plant, till we had found the simple element, the regular basis of all the various forms, and investigated and defined its vital properties." (p. 43.)

This one lecture contains nearly all the information that is given by the author in the present course respecting the organization of plants;

and we cannot but think that a little more extension might well have been given to this part of the subject. The characters of the most important elementary tissues are lucidly described; but scarcely anything is said of the manner in which these tissues are combined or worked-up (as it were) into the stem, roots, leaves, and other organs of the plant, the functions of which cannot be rightly understood without this knowledge.

The Third Lecture is devoted to the "Propagation of plants;" a somewhat delicate subject for a popular audience, which is gracefully and yet scientifically handled. Here, too, we have an ingenious psychological introduction, on the apparent disunion between the abstract pursuits of science and the spiritual contemplation of Nature in which the vivid imagination delights; a disunion which, however, is but apparent, since the higher pursuits of science tend to lead back the mind to a purely spiritual view of external things. We shall dwell briefly on one of the principal topics unfolded in this lecture; since the doctrine first propounded by our author, and received with favour by many botanists and physiologists, proves to be so completely inconsistent with the observations of other eminent inquirers, that, in spite of its plausibility, we believe it must be abandoned. We shall first state this doctrine in Professor Schleiden's own words:

"Every plant produces within itself a certain number of single, free, unconnected cells, which at a certain epoch spontaneously separate from the plant..........These cells are especially destined for the reproductive function, since from every one of them is a new plant developed. An essential distinction, however, occurs in this development; one, indeed, recognised at an early period, and so exclusively regarded, that the higher agreement was altogether overlooked. The following are the two modes of development:

"A. In the one case, the cells destined to the reproduction are at once scattered on the earth or in the water, where the new plants are to grow. Then either the whole cell is gradually transformed into a new plant, new cells originating in it and taking its place, in these others, and so on, which is the case in the Algæ, Fungi, Lichens, and one part of the Liver-mosses; or the cell expands into a longish utricle or tube, but only one extremity of this tube becomes filled with cells, which gradually grow up into a new plant, the remaining portion of the cell meanwhile decaying; this is the case in the remaining Liver-mosses, the Mosses, Ferns, Lycopodia, and Horsetails..........In all these Cryptogamia the reproductive cells are called *spores* or germinal grains.

"B. In the Phanerogamia the matter is differently arranged. The reproductive cells, which are here called pollen, are formed in peculiarly metamorphosed leaves, the stamens. But other organs besides the stamens are found, either in the blossom of the same plant, or of another individual of the same species. These consist essentially of hollow and generally pear-shaped bodies, which have a small opening at the upper end. A body of this kind is called the *germen*, and the orifice the *stigma*. In the cavity occur little protuberances, formed of cellular tissue, the seed-buds, to which the very inappropriate name of *ovules* was formerly given. In each of the seed-buds is one very large cell, called the *embryo-sac*. At the flowering period, the pollen falls upon the stigma, and then commences the development of the reproductive cells. Each one extends into a long filament, exactly as in the Cryptogamia, and in this form penetrates to the cavity of the germen, to enter one of the seed-buds, and finally into the embryo-sac. The extremity which has passed in now becomes filled with cells, and these develope forthwith into a perfect, though as yet simple and minute, plantule, the so-called *embryo* or *germ*. Simultaneously with the development of the pollen-cell into the embryo, the seed-bud is perfected into a seed, the germen into the fruit." (pp. 69-71.)

On this theory, then, the germ of the flowering-plant is considered as being derived from the pollen-grain, as that of the cryptogamic plant is from its spore; the *ovule*, or *seed-bud*, having merely the office of receiving it, and of cherishing it by the nutriment which it affords. Against this view (which, we may remark, was not altogether original with Schleiden, the analogy between the spore and the pollen-grain having been dwelt upon by many preceding physiologists,) the phenomena of hybridity have been urged; since the complete admixture of the characters of the two parents, which is usually found in the offspring, seemed not sufficiently accounted for, if the germ were derived from the male parent alone, and merely the food were supplied by the female. A remarkable fact stated by Mr. Thwaites (op. cit.), as occurring within his own knowledge, is still more opposed to this doctrine; namely, the presence, within one ovule, of two embryos, of which one exactly resembled its male and the other its female parent. If the germs be exclusively supplied by the male parent, it seems impossible to account for one of them having been developed into a characteristically-different form, whilst the other completely retained its original character, both of them having been originally similar, and alike subjected to the modifying influence of the food supplied by the ovule.

Still the doctrine of Schleiden might have maintained its ground, if the facts had been as he stated them; that is, if it could be proved that the embryo is developed within the end of the pollen-tube, and that this is always external to the embryo-sac. These points, however, have been disputed from the first; and the concurrence of three eminent observers, who have recently taken up the inquiry, and have pursued it independently of each other, furnishes a strength of testimony as to the incorrectness of Schleiden's view, which for the present seems irresistible. As all these have taken for the chief subject of their investigation the very same species of plant as that on which Schleiden's observations were made, namely, *Orchis morio*, it cannot be said (as it frequently may be) that their results are different because they have been looking at different objects. And, moreover, there is that in the description and figures given by them, which gives the clue to the misinterpretation which Schleiden has put upon less exact observations. We shall now give a condensed summary of the researches of Amici, Von Mohl, and Karl Müller; referring our readers, for translations of their original papers, to the 'Annales des Sciences Naturelles' for April, 1847, and Jan. 1848; and for a general account of their contents by Mr. Henfrey, to the 'Annals of Natural History,' Jan. 1848.

The first part of the inquiry concerns the *pollen-tube*. All observers are now agreed as to the descent of this tube along the style to the ovary, and its passage through the foramen left by the non-closure of the coats of the ovule before fertilization, until it reaches the nucleus. Instead, however, of penetrating the embryonal vesicle, or of pushing this before it so as to become surrounded by it (as the heart is surrounded by the pericardial sac), the pollen-tube merely comes in contact with it, and passes a little way *down its side*, running between its outer surface and the inner coat of the ovule. This, of course, can only be seen when a side view is obtained; and if the pollen-tube should happen to lie either between the embryo-sac and the observer's eye, or beneath the embryo-sac, it may easily be imagined to be enveloped by the embryo-sac, as Schleiden has

represented. The lower end of the pollen-tube dilates into a sort of bulbous expansion, and its contents become altered; for instead of the clear fluid in which granules are intermixed, such as is found in the upper part of the pollen-tube, we now find in it a coagulated grumous mass of a greenish-yellow colour. It is not surprising that Schleiden should have taken this for the origin of the embryo, as Mohl himself was inclined to do at one stage of the inquiry. We shall better understand its real office in the act of fertilization, after the history of the embryo-sac has been described.

The embryo-sac is at first a large cell, filled with "protoplasm," lying in the midst of the nucleus or central mass of the ovule; and whilst it is being prepared for fecundation it undergoes a considerable increase of size, pressing upon and obliterating the cells of the nucleus, and absorbing their contents into itself. Within the embryo-sac are seen the first traces of three cells, which begin to undergo development *before the access of the pollen-tubes;* and it is in these cells that the entire embryonic structure originates. After the contact of the pollen-tube, these cells undergo a rapid multiplication; one of them in particular, which has been named the germinal vesicle, appearing to be peculiarly energetic in giving origin to a new generation of cells, which continue multiplying by self-division, and thus form the embryonic structure.—Hence it appears certain, that we must regard the pollen-grain, not as furnishing by itself the germ of the plant, as Schleiden supposed, but as concurring to do so with the cells contained within the embryo-sac; the contents of the two being apparently intermingled by endosmose through the membrane of the latter. It has been thought by some that there is an absorption of the membranes of the pollen-tube and of the embryo-sac, where the two come into contact, so that their contents directly intermingle; but it is stated by Hofmeister,* who has made similar observations on the impregnation of the *Œnothereæ*, that at the time of fertilization, the pollen-tube and the embryo-sac are of such firm consistence, as to admit of being separated with a needle under the microscope; so that the fertilizing matter must pass through three membranes; viz. those of the pollen-tube, of the embryo-sac, and of the germinal vesicle itself.

We have thought it right to make this digression, partly for the sake of the readers of 'The Plant, a Biography,' who might otherwise be misled by the confident tone in which Professor Schleiden speaks on this subject; and partly that we may put the student of animal physiology in possession of the best-established facts in regard to the real nature of vegetable impregnation and the origin of the embryo,—points which have such an important bearing on the interpretation to be put on various phenomena witnessed in the study of the early history of the animal ovum. We now return to our author's Fourth Lecture, on the "Morphology of plants," in which his style seems to us less clear and happy than elsewhere. His object is to show, that in comparing different types of structure, we must disregard external form and even function, and must be guided chiefly by the origin and the history of development of the several parts; a principle which is now rapidly gaining acceptance amongst the philosophic students of all departments of natural history, but which is scarcely yet adapted for popular exposition, unless presented in the most simple and

* Botanische Zeitung, Nov. 5, 1847.

lucid form. To a reader of ordinary English habits of thought, the first part of this Lecture will be scarcely intelligible; the latter part, however, contains a sketch of the entire vegetable kingdom, in which the most prominent characters of its principal divisions are very graphically displayed in accordance with the view just expressed. Professor Schleiden considers that the highest and most perfect combinations of the flowering organs, which he regards as the culminating point of the whole, are to be found—among the Monocotyledonous plants—in the *Grasses;* and among the Dicotyledonous, in the *Compositæ.*

"If we would recognise a series, in the progress from the simplest to the most complex, we must evidently regard the Grasses and Compositæ as holding the highest station in existing vegetation. Remarkably enough, also, precisely these two families, by their number of species and individuals, constitute the most peculiar characteristic components of the existing flora; for, in the collective number of about 300 families of plants, the Grasses alone include one twentieth, the Compositæ a tenth, therefore both together almost a seventh, of the whole number of species known." (p. 103.)

The Fifth Lecture, quaintly entitled "About the Weather," presents a lucid summary of the chief facts known regarding the laws of climate, with an amusing interspersion of elements of risibility. Thus, when pointing out, at the commencement, the influence of weather on the mental and moral state of man, he tells us that "our ancestors knew and named a 'joy-month' (May), and in England, November is called 'the month of fog, misanthropy, and suicide.'" And then, as if the notion had not been disproved long ago by statistical evidence, he repeats the stale fiction—"It is a fact that most suicides happen there in this month." A little further on we have a contrast drawn between the sullen melancholy of a gentleman whose hay-crop has been destroyed by continued rain, and the blithe joyousness of the vine-grower who has been gladdened by favorable weather, and has made an advantageous sale of his crops. Other changes than those of the weather, however, may produce the same effects; the readers of our esteemed contemporary 'Punch' will remember the graphic delineations of the interior of a household, with "Consols at 90," and of the same with "Consols at 80," which appeared therein a few weeks since. We are presently afterwards told that "the gay Tahitans, the dull Fuegians, the formal Chinese, the roving Bedouins, the child-like Hindoos, the manly English, the abstracted Germans, the material Yankees,—all these, and the thousand other varieties of human nature, are fundamentally dependent on or promoted by the weather;"—a proposition which it is easier to advance than to prove. We have afterwards, by way of counterpoise, a due admixture of the tragic, in a description of a parching calm, and then of a fearful storm, to which an unlucky ship is subjected in its passage across the equatorial region; followed again by an amusing piece of quaintness. "In the German legends, we read of a cavern, in which the Dame Holle sits and brews the weather. That region of calms and storms is an actual Dame Holle's hole. The weather of the whole world is manufactured there." The lecturer then proceeds to give a truly scientific exposition of the causes of winds, the distribution of temperature, and the other conditions on which "the weather" is dependent; and from this we extract the following

account of what has been called by Professor Dove the law of the circulation of the winds, which constitutes the nearest approach yet made to a systematic acquaintance with their variations in these latitudes.

"According to the statements already made, there are but two wind-currents upon the earth, that blowing from the poles to the equator, and that returning to the poles. Let us imagine a place in the region of what I have called the changeable weather, say in Germany, and let us assume that this spot lies directly in the direction of the polar current. A north wind blows, the air is cold, the sky serene, and remaining so while the wind gradually changes, and at last appears as a true east wind, the dry, highly oxygenated polar air of which is so perilous to those whose lungs are affected. This wind blows until another replaces it, and this is none other than the equatorial current, which always begins as a south wind; and the meeting of this with the east produces the south-east wind, having an intermediate direction; in this the warm, moist air of the equatorial current is cooled down by the cold polar current, and constrained to deposit part of the water it contains in the form of clouds, snow, or rain. Gradually the equatorial current acquires the mastery; in the south wind it becomes warm and bright, and so remains till the equatorial current gradually diverges round to the west. The northern polar current alone can take its place at a change, and the mixture of this with the moist air of a north-west wind again gives rise to abundant atmospheric precipitation. These are the cold, damp days, in which those persons suffer so much who have nervous complaints." (p. 122.)

We have thus a scientific explanation of the fact known to most persons who are accustomed to attend to the direction of the wind,—that a change of wind rarely or never continues for any length of time, unless it take place *with the sun*, i. e. from east by south to west, and back again to the east by the north.

The Sixth and Seventh Lectures, headed "What does man live upon?" need not now detain us; as we shall take some other opportunity of discussing the subject of human food. We observe that Professor Schleiden now recognises the truth of Liebig's principles of vegetable nutrition, which at first, if we remember rightly, he severely criticised; still he considers that Liebig takes a somewhat exclusive view in laying so much stress on the inorganic materials furnished by the soil, and so little on the humus; and he remarks that—

"Liebig's chemical onesidedness, in this respect, will probably be mischievous to those agriculturists who cannot neutralize this fault by their own thorough knowledge of natural science; just as, on the other side, the absence of a thorough study of natural science, and crude empirical prejudice, have in these later times prevented many, particularly German agriculturists, from taking part in the improvements called forth by the progress of science." (p. 178.)

Professor Schleiden, like Liebig, has his peculiar view of the nature and causes of the potato-disease, and his remedy for it. Any suggestion that proceeds from him will doubtless be received with attention; but it would be absurd in us to pronounce upon its merits. From all that we have ourselves gathered, we should be disposed to regard the disease in the light of a true *epidemic,* not referable in its essential nature to any peculiarities of soil, climate, mode of cultivation, &c.; but dependent for its great development upon predispositions arising out of their agency.

The Eighth Lecture treats of the "Milky juices of plants;" this subject having been selected as an example of the variety of uses to which man renders plants subservient, independently of their forming the basis

of his nutrition, The scene is laid at the opening "in the brilliant arena of the polite world, the entrance to which is decorated by the celebrated obelisk of Luxor,—that field, where, in bloodless battles, the victories of fashion are decided,—albeit, the ground was originally consecrated to the 'humilité de Notre Dame;'" and the question discussed is the merit of the Mackintosh. This of course introduces the subject of caoutchouc, and the properties of the trees and plants which yield it. Thence the lecturer proceeds to the wourari poison, the nettle-plants (including the bread-fruit and cow-tree), and lastly, to the far-famed poison-trees of Java, whose genuine effects have been exaggerated by their connexion with the mephitic exhalations of the volcanic district in which they grow. An extremely picturesque account is given of the scenery and circumstances among which these various tribes display themselves; and their prominent characters are brought vividly before the mind.

In order to give an idea of the character and purposes of a natural classification, "A Sketch of the Cactus tribe" is introduced as the subject of the Ninth Lecture; a tribe which, in the totality of its relations, is certainly one of the most suitable that could have been selected; and of which the account here given is fertile in topics of interest. We must not, however, dwell longer upon it, but must notice the subject of the Tenth Lecture, namely, "the Geography of plants,"—a science, says Schleiden, "of a peculiar nature, still young and burdened with all the faults of youth, overflowing with the fulness of life, certain of a fair and powerful manhood, but still disorderly and obscure, gathering much that is at present unintelligible for use in riper years, and as yet dreaming much more than thinking." The starting-point of the Lecture is now shifted to London; and the scene is laid in the Travellers' Club House, of which the waiters are said to "catch flying, from the conversation of the guests, more geographical knowledge than if they had been for years industrious scholars of Ritter." We are then introduced to a party of three;—a sportsman, who details his exploits in deer-shooting in Scotland; a wanderer in Australia, who narrates how he saw a settler nearly annihilated by a monster of a kangaroo, seven feet high; and an amateur whale-fisher, who had been seeking for excitement in the North Seas. From these personages, who are intended to present us with a graphic portraiture of the entire variations which different regions of the earth present, as to their physical features and living inhabitants, we are made to pass, by a somewhat abrupt transition, in which Oliver Cromwell (!) is the connecting link, to certain abstract considerations regarding the mental and moral condition in which scientific pursuits should be carried on; and these, again, are made to introduce a sketch of the history of the geography of plants, the conception of which, as a branch of really scientific investigation, is due to Humboldt. We may take a future opportunity, in presenting our readers with a notice of 'Cosmos,' of giving a brief account of the manner in which this illustrious philosopher, comprehending the whole surface of the globe in one intelligent glance, made the geography of plants a part of the general theory of the earth, by showing how much their distribution is dependent upon the physical conditions which it presents.

The title of the Eleventh Lecture would lead us to expect an epitome of the "History of the Vegetable World," as developed by geological re-

search; the amount of information, however, which it communicates respecting the fossil flora is very small; various other topics being brought in, some of them, as it seems to us, rather inappropriately. Thus, at the commencement we have a general discussion upon cosmogonies, from which we may quote a passage highly characteristic of the tone prevalent amongst those who claim to be regarded as the "free and enlightened" thinkers of the present generation:

"While the haughty English High-Church, much more despicable than Popery in its most offensive extreme, fattens on the sweat and blood of millions of poor hungry Irishmen, she hunts down, with all the unworthy means that stand at her disposal, every scientific inquiry which appears to contradict her narrow view of the literal truth of the old Jewish poetry." (p. 272.)

Surely such language as this is misplaced, whatever ground the writer may think he has for his opinion. We gladly bring into contrast with it the beautifully expressed creed of our author himself:

"The naturalist cannot go beyond the simple expression, 'God is the Holy Author of all things, and His wisdom, His love, has created the world.' It is to him, as to every thinking man, a truth which may not be touched." (p. 273.)

The doctrine of successive repeated *creations* of new vegetable forms is held by Schleiden to be superfluous; since, if the first simple cell be given ("which came forth upon the earth once, at least, out of the strife of the inorganic elements"), he holds that—

"The whole wealth of the vegetable kingdom may have been formed by a gradual passage from it through varieties, sub-species, and species, and thus onward, beginning anew from each species,—in a space of time, indeed, of which we have no conception, for which, however, since there is nothing *real* wanting, we may provide at pleasure in our dreams." (p. 291.)

The latter part of the Lecture contains some very striking views on the modifying influence which man has exercised upon vegetation; and upon the duty of avoiding those exhausting and destructive systems of tillage, which, for the sake of immediate profit, have contributed, and are still tending, to future misery and abasement. Adverting to the more intelligent and less selfish spirit, of the gradual diffusion of which there are happily numerous and increasing manifestations, he concludes:

"We see in the gray, cloudy distance of the future a realm of peace and beauty on the earth and in nature; but to reach it must man long study in the school of nature, and, *before all*, free himself from the bonds of egotism."

The closing Lecture, on the "Æsthetics of the Vegetable World," commences with a discussion on the abstract nature of beauty, which is full of interest when once the train of thought is comprehended; but its character is altogether so very *German*, that we doubt whether it will not be put aside by many readers, almost in despair. The key-note to the whole is contained in the following passage, with which the Lecture opens:

"Inexplicable is the nature of beauty. Only in the feeling does the susceptible soul become conscious of it; and to the logically arranging, scientifically connecting, and theoretically deducing understanding, it remains ever a foreign, closed territory,

'Where all the wisdom of the wise man leaves him blind
There plays in free simplicity the child-like mind.'

When with our observations and experiments, with analyses, conclusions, and

proofs, we have unravelled nature into a plain, intelligible tissue of substances and forces, beauty and sublimity enter upon the field, unite the disjointed once more into a single whole, and mock our endeavours to comprehend the eternally incomprehensible. We explain it not, yet it is true; we comprehend it not, yet there it is. The pure heart speaks out unhesitatingly what the acutest intellect never finds:

"'The heavens declare the glory of God; and the firmament showeth his handywork. One day telleth another: and one night certifieth another.'

"No matter; that which we cannot comprehend, cannot explain, may yet perhaps be so far capable of arrangement and demonstration, that we may come to understand where and why the Incomprehensible necessarily enters into joint possession of our spiritual life. Though we cannot develope the nature of beauty in itself, yet it may be possible, perhaps, to discover what it signifies for us, mankind, under what shape it appears, and what its influencing elements are." (p. 312.)

The *physiognomical* character of different tribes of plants is then considered, with reference both to their participation in the composition of a landscape, and to the impression which their forms individually make upon our minds. We should gladly make many quotations from this part of the lecture, which abounds in striking descriptions and interesting suggestions; but the interest of these would be greatly diminished if taken apart from their connexion; and our space will not admit of any lengthened extract. We can, therefore, only refer our readers to the work itself, in which we can assure them they will find much to interest and inform;—its interest not being diminished but increased by the unusual style, the unexpected transitions, the bold and free range of thought, which characterise the composition;—and the information not being the less valuable, on account of the novelty of the dress in which it is clothed.

We must not omit to pay a just tribute of praise to Mr. Henfrey, for the force and spirit of his translation. His own botanical acquirements are a sufficient guarantee for the accuracy with which the scientific language of the author is rendered; and in the difficult task of transfusing into readable English the peculiar character of those more discursive portions which constitute a large portion of the original, he has on the whole succeeded extremely well. The illustrations are well executed; and the whole appearance of the book is appropriate to its subject.

Art. V.

Mémoires de l'Académie Royale de Médecine. Tomes XII and XIII.—*Paris*, 1846-8. 4to, pp. 625 and 727.

The 'Mémoires' of the Parisian Royal (lately baptised National) Academy of Medicine, occupy in French medical literature a position somewhat analogous to that held by the 'Medico-Chirurgical Transactions' in our own. Each volume, however, generally contains fewer communications; not so much probably from the paucity of the material, as from what to us would prove the insupportable verbosity of those admitted. Moreover, a considerable space is always occupied with the "Eloges" of deceased members; a species of composition in which the French take great pride, but which, to our appreciation, partakes of much sameness and vapidity. These "Eloges" are yet not without their uses, as supplying biographical

data; and we may mention that the present volumes contain notices of Larrey, Chervin, Chevreul, Jenner (a very indifferent performance), and, lastly, of the great official panegyrist himself, M. Pariset, from the pen of his successor, M. F. Dubois.

The first paper we purpose noticing is a prize essay upon the following subject, proposed by the Academy:

"Examine the cases in which Multiple Abscesses have been observed, and compare such cases in their different relations."

On this subject, the authors, MM. Castelnau and Ducrest, have produced a very creditable memoir. They much approve of the substitution by the Academy of the term *multiple* for the usual one, *metastatic;* and believe that had observers more generally eschewed this latter, far more precise results than we are now in possession of, would have been arrived at. The following is their definition:

> "Whenever two or more abscesses are developed simultaneously, or successively at very short intervals, without their existence being explicable, either by traumatic violence exerted upon the points where they appear, by the vicinity or extension of inflammatory action (as in phlegmonous erysipelas), by the separate burrowings of pus from a single centre, or by the infiltration of fluids into the tissues (as in abscesses from congestion, urinary abscess, &c.),—such abscesses are considered by us as examples of multiple abscess." (tom. xii, p. 3.)

The various diseases in which these abscesses have been observed to manifest themselves, are passed in detailed review. The buboes of *plague* are familiarly known; and in this disease there have been also observed suppuration of the parotid gland and of the tumours termed *charbons.* Almost all historians of epidemics of *typhus* relate examples of purulent deposits in different parts of the body. *Yellow fever* is very rarely, if ever, attended with such; a circumstance explained by its paludal origin,—fevers so arising seldom giving rise to abscess. *Typhoid fever* occasionally induces multiple abscess, a case of which occurred to one of the authors. A girl, aged 15, after suffering all the symptoms of well-marked typhoid, was attacked, about the 30th day, with purulent otitis and parotitis; and during the next two months, fluctuating tumours formed successively over almost every joint and every part of the trunk. She recovered. Sydenham and Boerhaave testify to both the occurrence and the danger of these abscesses in *variola.* Mr. Ancell reports a case in the 'Med.-Chir. Transactions' (vol. xxiii, 1838); and the authors quote another from Andral's 'Clinique Médicale,' in which, death occurring on the 14th day, numerous abscesses were discovered in the lungs, the muscles of the neck being also completely dissected out by purulent infiltration. Several authors mention abscess as an occasional complication of *scarlatina;* but this is not the case in respect to *rubeola.* M. Duplay (Arch. Générales, tom. xxix) shows that parotid abscess, without being frequent in *cholera,* yet occasionally occurs. In *syphilis,* other collections of matter are sometimes developed, beside buboes. Numerous writers have signalized the complication of *erysipelas* by abscess; and the present authors believe this to be the case more frequently in the erysipelas of children than in that of adults (Landouzy, Gaz. Méd., 1835), and furnish an interesting case. An infant, 8 or 10 days old, was attacked with erysipelas which traversed the whole body. No less than nine abscesses followed in succession, located

over the scalp, the scapula, the hand, the thigh, and the lumbar and sacral regions. Some of them were opened, and the contents of others were absorbed, the child recovering. Besides the slowly-forming abscesses of the lymphatic glands, characteristic of *scrofula*, there are others pursuing quite a special course, upon which authors have but little insisted.

"In a child, presenting at the time nothing particular, a portion of the skin becomes raised and reddish, but with little or no pain. Next day the tumour is larger, and evidently fluctuating, and at the end of three or four days it suppurates or becomes suddenly dispersed. In the first case, contrary to what takes place in scrofulous abscess of the glands, cicatrization promptly follows; while in the second, the tumour manifests itself in some other part, and pursues exactly the same course. In this way, from ten to fifteen abscesses may show themselves one after another, without the health of the patient being in anywise injured, but without our being able to say it is ameliorated thereby, although it certainly has seemed to us that these evacuations have proved more useful than injurious." (tom. xii, p. 21.)

Under the name of milk-abscesses (dépôts laiteux) the older physicians designated a large category of multiple abscesses occurring in the *puerperal state;* examples of which every one has seen, since the writings of Dance and others have directed so much attention to the subject. Obstinate *cutaneous eruptions* may become thus complicated; and a case is given, in which a young man could obtain no relief from an inveterate prurigo, until two or three abscesses had formed in different parts. Of all diseases, *glanders* is the one which most constantly gives rise to multiple abscess. *Wounds*, since the time of Ambrose Paré, have excited much attention, as the cause of these collections, and in later times have become far too exclusively so regarded. *Phlebitis*, as a cause of abscess, has been increasingly estimated since the time of Hunter. Some surgeons are of opinion that these abscesses may occasionally occur *spontaneously* in the midst of health, and unpreceded by traumatic lesion, or by any of the above-mentioned diseases.

In reference to the various *doctrines* which have prevailed respecting multiple abscesses, we must divide these into two categories, *medical* and *surgical abscesses*. The former are explained by Hippocrates, as resulting from the defective power of the natural emunctories in the expulsion of noxious principles capable of producing disease. Among his successors, critical abscesses have been regarded as the results of morbific matter there presenting itself, or as the change of the seat of irritation or sympathy, just as the humorist or solidist schools have prevailed. It is, however, to surgical abscesses that the especial attention of most observers has been directed. Attributed by the older surgeons to the use of poisoned arms, Ambrose Paré first taught that they were due to a changed condition of the fluids, produced by some unknown alteration in the atmosphere. Morgagni somewhat obscurely hinted at the doctrine of the reabsorption of pus, a doctrine afterwards logically worked out by Quesnay; but it was not until the time of Hunter, that the fact of the existence of pus in the blood was formally established, and the influence and mode of action of phlebitis pointed out. The ignorance in France of the writings of the great English surgeon gave rise to various other theories; until Velpeau again demonstrated the presence of pus in the blood, thus giving a new support to the doctrine of *purulent infection*, admitted by Dance, Cruveilhier, and Blandin.

"This theory acquires great importance from its precision; but if the excellent works produced by its partisans strike us with admiration, we cannot but feel astonished that they have not more frequently resorted to the means of demonstration which they had at their disposal. It would seem, in fact, that the first thing to be done, when multiple abscesses were attributed to the presence of pus in the blood, was to ascertain experimentally whether the introduction of this substance acted in the manner supposed, and then, in case of the affirmative, to become assured whether such introduction was naturally possible. Now, if the authors who have sustained this theory have done much to demonstrate the possibility of the introduction, they have done but very little to exhibit its effects; and as the means of conviction which they have chosen is difficult of management and obscure, or at least, very delicate in its results, it has come to pass that many minds have not found in it sufficient characteristics of evidence, and have remained in doubt. Some even, founding their opinion upon very defective experiments, have declared that the falsity of the doctrine might be demonstrated by the very means which best prove its reality. This, we believe, will be seen in a very distinct manner by an analysis of former experiments, and a comparison of these with our own." (tom. xii, p. 32.)

Judging from the confidence with which negative results have been stated, it would have been expected that these had been founded upon numerous carefully conducted experiments; whereas a diligent search has revealed to the authors few instances in which pus has been injected into the veins; and on critically examining even these, it is found that in nearly all, the animals have been killed before a sufficient time for the development of abscess had been allowed to elapse. When they have lived longer, abscesses, or the ecchymoses which precede them, have been observed. Altogether, taking into account that several of these experiments had been instituted without any reference to the formation of these abscesses, and that others, specially so undertaken, were neither conducted with sufficient care, nor stated with sufficient precision, the authors believe that a case has been made out for the institution of a new series possessing these guarantees.

Pus was injected into the saphena vein in several dogs; this vein being chosen because it is superficial and remote from important organs. The whole amount was not thrown in at once (with one exception, when the dog died in 30 hours), but in divided quantities, at intervals of from a few minutes to several days. The pus was obtained from various sources, and frequently mixed with a little water; and from a quarter of a gramme to 6 grammes were thrown in at a time, the total quantity in any one case not exceeding 49 grammes. The dogs were never killed, but were allowed to run about a yard, food and drink being placed at their disposal. Of the seven dogs, two recovered, and five died; the histories of all the experiments are given in great detail.

"It will even now be seen how far these results differ from those which have been generally obtained; for not only have the majority of animals not recovered, but those succumbing have presented symptoms and lesions little in accordance with those observed by other experimenters. Thus, in all the animals that died, we found either multiple abscesses completely formed, when the duration of life had been long enough, or the lesions characteristic of the first stage of such abscesses, when they had succumbed too soon; or, again, when they had lived for an intermediate period, the two lesions were observed in the state of union and transition." (tom. xii, p. 78.)

The animals which died early presented only *ecchymoses*, a term which

is employed by the authors, as by MM. Trousseau and Dupuy, only for want of a better, as in fact there is no effusion of blood in the little red centres which precede the abscess; and, perhaps, a better term would be *ramollissement rouge*, or *ramollissement ecchymoide*. As to the *seat* of the abscess, this was sometimes on the surface, at others within the viscera, and generally both. The lungs were the organs oftenest affected, then the spleen, and then the kidneys. Analogous to these, without being strictly abscesses, are the purulent collections which were sometimes found in the joints, the pleura, or the peritoneum. In other cases, serous or sanguineo-serous collections offered their points of analogy.

The appearances after death were thus remarkably similar to those recorded by the best authors, as being observable in metastatic abscess occurring in man; and besides this, the phenomena witnessed during life bore a closeness of resemblance hardly to have been anticipated in such dissimilar animals. Thus, *shivering* was a very frequent symptom; *vomiting* and *purging*, at least the former, being no less so. The amount of *urine* was diminished; great *thirst* prevailed, and *anorexia* was present. *Prostration of strength* was observable in all, though at different periods; and in most the *respiration* was accelerated.

A proposition which has been laid down as favorable to the doctrine now advocated, excited great doubts in the authors' minds as to the legitimacy of the conclusion. Every foreign body introduced in its natural state into the venous system, says Cruveilhier, induces, when its elimination by the emunctories is impossible, *visceral abscesses exactly similar* to those which succeed to wounds and surgical operations. "Now, is it not evident, that if the results, apparently so conclusive, deducible from our experiments, can be induced by the introduction of any foreign body into the venous system, there is nothing specially to be stated in regard to pus?" To set the matter at rest, a new series of experiments was devised; and their results compared with the very numerous ones bearing upon this subject already on record. Very various substances were employed, as milk, urine, semen, putrefying substances, metallic mercury, corrosive sublimate, &c.; and the general result of the analysis of these new experiments, and of those already recorded, is thus stated:

"Metallic mercury, powder of gold, and solution of corrosive sublimate, are the only foreign bodies, besides pus, which have been found capable of causing multiple abscesses. Those resulting from mercury have little analogy to those produced by pus, differing from them both in their small size and in the absence of fluid contents. They form, in fact, a number of minute granulations, having no disposition to aggregation into larger masses, each of these inclosing a brilliant little globule of mercury. A general remark may be made upon abscesses induced otherwise than by pus; viz. that their seat is almost constantly *solely in the lungs*, while the deposits resulting from pus may occupy various other parts of the body, and may even be met with in these while the lungs continue quite exempt. On analysing the symptoms produced in the two cases, we may lay down this general proposition, that there is no *single symptom* induced by purulent intoxication, which may not be also caused by the injection of some of these substances; but that this is not the case as regards the *combination of symptoms*. . . . . . Each category of foreign bodies has its own mode of action; and even when this does give rise to the formation of multiple abscesses, these do not resemble those caused by pus; so that we can no longer admit the proposition cited from Cruveilhier." (tom. xii, pp. 113, 115.)

Pus, then, being the only substance competent to determine abscesses having every feature in common with those termed by the authors *surgical*, it might be inferred that these last owed their origin to the introduction of pus into the circulation. The opponents of this doctrine urge the two following objections: first, that "*the absorption of pus from the surface of a wound is unphysiological and impossible;*" and, secondly, that "*pus secreted by inflamed veins is always separated from the general circulation by coagula acting as plugs.*" The authors, taking this last objection first, protest against its exactitude; and cite, in corroboration of their opinion, a case rendered highly interesting by the extraordinary care bestowed upon the autopsy. A man, æt. 21, entered the hospital suffering from syphilis; but this did not appear to have much affected his general health. He was bled, and well-marked phlebitis was soon after set up. Symptoms of purulent infection came on, and he died in a fortnight. A skillful anatomist examined the veins with the minutest care; the basilic and its collaterals were found firmly plugged, so that several hours' dissection were required before veins sufficiently pervious for the passage of pus could be distinctly demonstrated; many of the deep-seated and superficial veins were filled with pus, and various abscesses were discovered.

But the authors also protest against the defect of actual pathological proof being assumed as contradicting the mixture of pus with the circulating blood in such cases; which they contend must take place on the following grounds:—1. Pus is not a direct transformation of the blood, but a product of secretion; or, at least, the agency of the solids is necessary for its elaboration (see Bérard, Dict. de Méd., vol. xxvi, for a *résumé* of all the facts). 2. The absorption of a purulent collection is a very rare occurrence. 3. It is almost as rare for an abscess in a deep-seated vein to open externally. This is more common in superficial phlebitis, and then multiple abscesses do not occur. 4. No one denies that pus has been seen in a free state in the circulation.—From these facts it necessarily results, that the pus from phlebitis passes into the circulation; for (1) if pus is found in the circulation, and is not the result of a direct transformation, it must necessarily have come from elsewhere; and (2) if the pus of a phlebitis does not disappear by absorption, or become discharged externally, it must escape by the circulation.

Admitting, then, the passage of the pus of a phlebitis into the blood, we have to inquire whether *this is always accomplished in the same manner;* and to this end the authors enter into the examination of two questions: first, whether *inflammation* of the *arteries* and *lymphatics* gives rise to the same results as phlebitis? and, secondly, can absorption or resorption of pus take place from the surface of purulent deposits? In respect to the first of these, it may be remarked, that multiple abscesses have been rarely observed even when arteritis has been violent, this probably arising from the usually solid condition of the product of inflammation; while, in respect to the lymphatics, our pathological observations are insufficient, for, although purulent infiltration of these vessels has, in several cases, been the only detectible cause of multiple abscess, yet the inflammation of some venous branch may have given rise to this.

In replying to the question of *absorption* or *resorption from a pus-generating surface*, we may appeal to reasoning based upon physiology, as well as to pathological anatomy. If *absorption* of pus is rare in a closed

cavity, it should be still more so from a free surface, where nothing prevents its exit; that surface, too, being the same that secretes it. Moreover, pus *absorbed* would not re-enter the circulation in its unchanged state. As to the statement made by Marechal and others, that *resorption* takes place in consequence of the entrance of pus into the open mouths of veins during the aspiratory movement in inspiration, it is to be observed that such movement does not influence the veins of the extremities, and if it did, these are, prior to the formation of pus, occluded by coagula.

But having established that *purulent infection is the cause of surgical multiple abscess, are we to attribute to this same cause all other multiple abscesses, or only a portion of these?* The authors thus state their reasons for limiting the application.

"The only ones that we think should be also referred to purulent infection, are the *puerperal abscesses;* and for the following reasons. If we compare the different categories of abscesses that we have admitted, it will be found that they differ in respect to their seat. In all the general diseases which we have enumerated, the number of such abscesses is inconsiderable; they are exactly circumscribed, and occupy neither the viscera nor the joints. Surgical and puerperal abscesses possess characters quite opposite to these, and agree with each other in this respect. If, too, we examine their course and concomitant phenomena, differences no less striking are observable. Thus, while in other diseases, these abscesses occur one by one, sometimes at very long intervals, and produce by their presence no change in the features of the principal disease,— being in fact mere epiphenomena, of which the economy, so to speak, takes but a very secondary notice,—in the purulent infection, the production of abscess is the important act of the organism, and if not the capital lesion, at least one highly characteristic of the disease, just as is the evolution of pustules in variola, or the production of intestinal ulceration in typhoid fever. It is obvious that, in other general diseases, the nature of the malady does not lead to the production of pus, which is but an accidental result; while in purulent infection, such production impresses the characteristic seal upon the disease—hence it has been called purulent fever, and pyogenetic or purulent diathesis.

"...........In respect to *erysipelas,* an important distinction is to be observed. In some cases the abscesses pursue exactly the same course as those observed in severe fevers; and in such must be referred to the original cause productive of the disease itself. In others, which have been only imperfectly observed, the abscesses much more nearly resemble the surgical ones, and then there is every reason to believe them due to purulent infection, whether we admit with Ribes, that the erysipelatous inflammation has its seat in the venous capillaries, and there terminates by suppuration, or believe that the erysipelas has become complicated by a true phlebitis.

"The same reasoning will still more strictly apply to the multiple abscesses following *wounds from dissection;* for we know, on the one hand, that the injection of putrefying substances into the veins of animals produces death without causing abscess, and, on the other, that it is in the same manner the phenomena usually take place in man. It is therefore to be supposed that if multiple abscesses analogous to those observed in purulent infection do sometimes occur, it is to such purulent infection that they must be referred." (tom. xii, pp. 136-7.)

The following are the general conclusions arrived at by the authors:

"1. Multiple abscesses are due to a changed condition of the blood, which is most frequently, and perhaps always, produced by the presence of a foreign principle in this fluid. 2. In those abscesses which are developed in the puerperal state, after traumatic lesions, surgical operations, or phlebitis, this principle is pus. 3. When abscesses are developed in certain other diseases, the principle is that

which gives rise to the disease itself. 4. The progress, prognosis, and treatment of these abscesses entirely depend upon the nature of the cause that has produced them." (tom. xii, p. 151.)

The Pathological Anatomy of Cholera; by Victor Bally.

In this manner may be translated the title of M. Bally's paper—"*Anatomie de la Choladrée Lymphatique ou Hydrocholadrée.*" He derives the term from χολας, *intestinum*, believing the disease to consist essentially in the loss of the fluid portions of the blood from the surface of the alimentary canal. His observations are founded upon a large number of dissections performed at the Hôtel Dieu during the epidemic of 1832, of all of which he preserved faithful records.

As the disease has too very distinct periods, the *cyanic* and the *reactionary*, two anatomical tables of appearances are required, the one exhibiting what occurs in *rapid deaths*, the other the consequences of *reaction*, for which the term *metamorphism*, employed by geologists, might be substituted. The insufficient observation of the distinction between these two periods has given rise to much confusion in the descriptions.

"The *cyanic period*, which constitutes the true *choladrée lymphatique*, only lasts from twelve to thirty-six hours; while that which succeeds it has an unlimited duration, and is truly a new disease. The poison which fermented in the blood, producing a dissolved state of that fluid, is exhausted, expelled, or neutralized; and the dissolved condition entirely ceases. Everything in the organism tends to re-establish the physiological condition, but by means of extraordinary procedures. The energy of the curative measures, the whole of the therapeutical combinations, should be directed to the neutralization of the poison, and the re-establishment of those bonds which, in the state of health, unite the molecules of the blood together." (tom. xii, p. 156.)

1. *The surface of the body.* The *cyanosis*, which may occur at very different periods prior to death, is not the result of engorgement, but of a deprivation of the watery parts of the blood. It is very variable in extent and intensity, and difficult of exact pictorial delineation. It is rare to find it quite uniform and general. In the algide form of cholera, it is by no means rare to find portions of the body having after death a higher *temperature* than they possessed during life. The *cadaveric odour* was long in becoming developed; a strong smell from some of the remaining alvine discharges, resembling the odour of semen, or the pollen of the chesnut-flower, was, however, observable. This smell was often strong and remarkable; and although it was also very perceptible on opening the intestines, it in nowise resembled that of fæcal matters. The fæces, too, were long retained without undergoing putrefaction. Otto of Breslau exposed the bodies of an old man and young woman dying of cholera for three days to the rays of the sun, and yet was the *rigidity* as great as at first, no putrefaction having occurred. The *emaciation* was as remarkable during the short space of time that elapses in cholera, as it is in the longer one of phthisis; and the bodies of the patients were lifted by the dissecting-room attendants with astonishing facility. The very singular condition of the skin was sometimes observed, termed by the author *empâtement*. In well-marked cyanosis, it might be seen towards the decline of life; but then the skin, if pinched up, would slowly, by its elasticity, resume its form. After death, however, it remained as placed, just as would so much moulded clay. The skin of the fingers was thus

thrown into plaits without the mechanical intervention of any foreign body. The author explains the occurrence by the removal of the aqueous portions of the blood from the cutaneous capillaries, leaving only a pasty plastering on their surface; and he believes this passive condition of the skin to act as a preservative against the communication of the disease by contact—differing so entirely as it does from the turgescence of the surface observable in yellow fever. The skin of the forehead, which was violaceous, pasty, and plaited prior to death, had frequently recovered its natural colour when the body was brought in for dissection. In this disease, says A. Beclard, the living resemble the dead, and the dead the living. In respect to the *eyes*, M. Bally observes:

> "Who does not recollect the frightful aspect of the unfortunate beings suffering under the algide period? How astonished and stupefied their air, what uncertainty in their looks? Was ever a more painful spectacle seen, than the globes of the eye coloured by numberless delicate arborizations, often giving rise to bloody patches; or those horrible stigmata blackening the sclerotica exposed to view by the convulsive expansion of the eyelids? The sclerotica, too, acquired a demi-transparency due to its partial desiccation, which allowed the blackness of the choroid to be seen through it. During the apogee of this period, a rapid disappearance of the fat of the orbit, and a notable resorption of the aqueous humour of the two chambers and of the interior of the tissues, took place. At the same time, the muscles, finding no longer the same resistance in a globe so diminished in volume, drew back the organ into the very depths of the orbit. It might be said, and the remark would apply to the entire organism, that the whole of the powers and actions of the economy exerted no longer any other effect than that of emaciating the body." (tom. xii, p. 166.)

The *conjunctiva* is sometimes entirely reddened, just as is observed in yellow fever, only in this last disease the black spots are not seen. The *iris* becomes dull, and loses all harmony in its movements, being drawn in a great variety of directions, and enlarged either transversely or vertically, instead of circularly, "whence results the fact that this organ, as in the feline race, acts irregularly and unequally; and indeed some portion of its fibres may be considered as attacked with partial paralysis, or perhaps with fibrillary convulsion, as is observed in the muscles." The altered condition of the circulation of the eye, the irregularity of its movements, and the change in its bulk, explain the peculiar look, the loss of sight, and the hallucinations, of cholera patients.

2. *The air-passages*. The most careful examination of all the component parts of the *larynx* did not explain the "*veiled voice*" perceptible in this disease, unless this arose from the marked dryness of the parts. It is probably due to loss of power; other exhausting diseases, as hemorrhage, producing the same effect, though perhaps none so rapidly. The *trachea* was frequently found injected, as were sometimes the *bronchi* also; and occasionally a granular or follicular eruption, such as that seen in the digestive canal, was found in the air-passages. The parenchyma of the *lungs* was unaltered; crepitating, though less forcibly than natural, under the fingers. If the bodies had remained long in the dissecting-room, hypostatic *engorgement* was observed. Sub-pleural ecchymoses were found at the surface of the lungs. The *pleura* itself was always (during the cyanic period) without traces of inflammation, and sticky, probably from the absorption of the more subtile portion of its lubrifying material.

3. *Circulating apparatus*. Whenever the author found a patient in the agony of death, he always, with an unjustifiable subserviency of the rights

of humanity to those of science, ausculted him, and found that the heart continued audible to the very last breath of life, even when the arteries seemed empty and destitute of all oscillation. The *left ventricle* had usually expelled all its contents; and when it had not, the fluid was found viscid as elsewhere. The *right* side of the heart was filled with brownish or whitish coagula, or contained dark, viscid, blood. In no other disease does the organ better maintain its *consistence*. Numerous small ecchymoses or petechial spots were often observed; and in one case the entire surface of the right side was covered with suffused blood, which, lying immediately beneath the adherent layer of the pericardium, did not penetrate into the muscular substance. The large *vessels* leading from the heart likewise exhibited these ecchymoses, &c. The vessels of the organ also, especially the veins, were more developed than in the normal state. The *pericardium* contained little or no fluid, and its external surface was viscous. When the bodies were opened shortly after death, an augmented *temperature* of the heart and other viscera (which in one case lasted twelve hours) was observed. The *arteries* were not found inflamed, and they contained much less blood than usual. In the *vena cava* and *iliaca* the blood was found black, viscid, and scarcely fluid; so as to offer, in the latter periods of life, a considerable obstruction to the circulation. This same condition of the blood, which has been well compared to diluted currant jelly, has been observed more or less to prevail in other veins also. According to Dr. Rochoux, who has made very elaborate comparisons, the least black blood in cholera is blacker than the blackest blood of other diseases. In careful examinations of the *lymphatic vessels*, these were found uninflamed, but *empty*.

4. *Alimentary canal.* The *mouth* is dry, and in no other disease do the lips remain so separated after death, owing to the rigidity consequent on prior convulsive action. The *teeth* lose their whiteness and brilliancy, acquiring a bluish or reddish cast; so that, owing to their unsaleableness, they are never drawn by the dissecting-room attendants. The *tongue*, so extremely cold during life, is less so after death, and is smaller in size than natural. The fungiform papillæ at the base are much developed. The *salivary glands*, though unaffected by morbid processes, yet cease to perform their natural functions,—this being one of the causes of the intense thirst suffered by the patient. The follicles of the *œsophagus* are sometimes developed, especially towards the cardiac orifice.—The appearances observed in the *stomach* are described very minutely. Some portions of the organ were observed of a dull white, just as in *ramollissement*, the texture, however, remaining firm. In other cases, spots of *ramollissement* did exist. The abnormal development of the capillary vessels filled with blackened blood, has given rise to the erroneous belief of the existence of gangrene. Gastritis, too, except to a very limited extent in exceptional cases, is not found during the cyanic period. Three groups of lesions, independently of altered colour, were observed:—1. Granules with no contents. 2. Pyriform granulations, containing a white, albuminous, purulent-looking substance, this being sometimes fluid, and oftener so thick as not to pass out even after the incision of the granule. 3. The mucous membrane was penetrated, and especially at the great *cul-de-sac* of the stomach, by numbers of minute holes or depression visible to the naked eye. When death took place in the algide period,

the external appearance of the *intestinal canal* was most remarkable, so deeply coloured was it by the network of vessels—ecchymosis sometimes resulting from the great afflux of blood.

"Not that the capillaries remained filled. They were so when an unusual, extraordinary, and prodigious afflux converged towards this point; and they remained coloured and varnished, by the red portion of the blood separated from the colourless portion. They constituted a new order of organs, usurping functions which were not their own, and invading, in the full force of the expression, those of other organs. The intestine thus became transformed into a reparatory and secreting agent, which exhaled and allowed the transudation of white blood; and if globules became mixed with this *intestinal sweat*, then the albuminous lymph was rose-coloured, the villi ceased to absorb, and the functions of the other secreting organs of the body were no longer performed." (tom. xii, p. 199.)

In the interior of the canal, partial inflammation occasionally existed, imbibition and coloration often also simulating this. The granulations or *utriculi* found on the mucous membrane attracted much of the author's attention. M. Serres was so struck by their analogy to the papulæ observed in itch, as to term this disease a *psorenterie*. These granulations, unlike to those of Brunner's glands, are found nearer to the cæcum than to the duodenum. They often have an umbilicus discernible to the naked eye; and with a glass, sometimes two or three of these may be seen. Some of them contain a plastic white matter; and they vary from a microscopic size to that of a small pea, destroying the velvety feeling which the mucous membrane should impart to the finger. These bodies were best seen in patients who died rapidly and were speedily examined; for if the necroscopy were long delayed, they diminished or even disappeared, although they have been seen thirty-six hours after death. They were sometimes found occupying the whole alimentary canal, from the pharynx to the anus; being, however, rarely seen in the pharynx and œsophagus, frequently in the stomach and duodenum, always in the ileum, and often in the cæcum, its appendix, and the colon. Contrary to the experience of Sandras in Poland, in no autopsy did the author observe any alteration in Peyer's glands, save perhaps their being somewhat in relief.

Deferring to a future opportunity a full account of the *intestinal secretions*, the author now only alludes to their physical characters. He cannot understand how cholera, capable of producing cyanosis, can exist without the exudation of whitish matters by the intestinal surface; and believes that in those cases in which no evacuations take place, the canal would be still found to be filled with them. This part has acquired new emunctory functions, and separates the aqueous portions of the blood. The appearance of the rice-water stools is too familiarly known to render it desirable to quote his description.

Like other epidemic diseases, such as typhus, plague, yellow fever, &c., the cholera is remarkable, short as is its duration, for the development of the *ascaris lumbricoides*. Dr. Brière, who died in 1832, had been only ill for seven hours, and had for some days prior to the attack been taking the bichloride of mercury, which is a powerful anthelmintic; and yet several lumbrici were found.

In respect to the other parts of the digestive apparatus, the *spleen* was found to have undergone a rapid atrophy; this was also observed by Magendie and Serres, although in other epidemics this organ has been

found congested. The author believes that the Anglo-Indian practitioners have attached too much importance to altered conditions of the *liver*, which he regards as secondary only. The *gall-bladder*, in spite of the violent efforts at vomiting, is never found empty; its contents possess great variety of colour.

5. *Urinary apparatus.* The secretion of urine is suspended, and M. Bally is disposed to regard the plastic mucus found in the kidneys, bladder, and arteries, as the residue of urine, whose watery parts have been removed—resembling as it does the deposits termed critical. The kidneys were found contracted; and if the patient died prior to reaction, the bladder was sometimes reduced to the size of a walnut, still containing perhaps a few drops of urine.

6. *Automatic movements.* At the instant of the extinction of life a rapid movement of the lips was observed (*marmotage*), exactly resembling that which takes place in some persons while reciting their prayers. It continued a short time *after* death, and at the same time movements of the forearm were observed. These first produced pronation, which was slowly but with complete regularity followed by supination—such alternation continuing even for several minutes. If great care in observation had not been observed, it might easily have been believed that these meteoric motions of the lips and arms were directed by consciousness. Frequently, a vermicular or oscillatory movement of the muscles of the thorax was discernible.

7. *The nervous system.* In several cases the sinuses of the brain were found remarkably empty; and the veins of the pia mater were swollen up though containing but little blood. The membranes of the brain were not found in the congested state described by some Indian practitioners; both the arachnoid and pia mater retained their transparency, the former containing no fluid, and manifesting somewhat of the stickiness perceived in the pleura. The ventricles, too, contained less fluid than in the normal state, and the substance of the brain did not exhibit bloody points on section. The spinal marrow was examined in almost all cases, and found in a natural condition, its veins, however, being swollen and pursed up, without any turgescence from blood, and containing (the author supposes) rarefied air. The various nerves and ganglia were examined, without furnishing any particular results.

We have given this summary of M. Bally's researches without comment, as an interesting contribution to our knowledge of the Morbid Anatomy of Cholera; but it must be constantly borne in mind, in estimating the value of any such collection of observations, that the epidemic presents itself in a great variety of forms; and that its nature is often modified by the constitution of the place and the people attacked.

The principal portion of the twelfth volume of the Mémoires is occupied with essays contributed by MM. Michea and Baillarger, in answer to the subject proposed by the Academy in 1844; namely, *Hallucinations, their causes, and the diseases that they characterise.*

The first of these papers is an extract from the answer delivered in by M. Michea; who defines and classifies hallucinations in the following manner.

"They are perceptions produced in the absence of their *habitual* stimuli, without

the objects which they represent exerting any kind of impression on the organs of sense.

"Do they exclusively originate in a disturbed condition of the sensorial organs, their seat being the peripheric portion of the nervous system, as F. Plater, Sauvages, and Darwin affirm; or is the perceptive centre, the brain, their only source, as maintained by Gall and Esquirol? Although erroneous, if taken in an absolute sense, both these opinions have some truth in them. Cases in which hallucinations of sight have occurred in the blind, and those of hearing in the deaf, while pathological anatomy has revealed a complete disorganization of a part or of the entire tract of the sensorial nerve, are not very rare in the annals of science. On the other hand, hallucinations may be produced by irritating a sensorial nerve, either in its course or its expansion. Marcellus Donatus tells us of a person whose eyes were quite normal, and who never experienced phantasmic perceptions, save when she opened the left eye while she closed the right. This fact alone would suffice to prove that certain hallucinations are in immediate dependence upon the peripheric nervous system, at least as far as regards their origin; but it is in the encephalon itself that they are elaborated, constituted, and invested with a characteristic and distinct form. In no case can the peripheric nervous system produce these phenomena without the concurrence of the brain, while this latter may very well dispense with the co-operation of the former. Thus, then, we may divide hallucinations into *symptomatic* and *essential,* into *sensorial* and *encephalic*........... In our view, the essence of hallucination consists in the metamorphosis of the sensory impression, not into a sensation or true perception, but into a phenomenon intermediate between this psychological act and an idea or pure conception. It is intermediate between these two states of consciousness; for an hallucination is less than a sensation or perception, since it represents a body without this actually presenting itself to the senses; and it is more than an idea or pure conception, since it furnishes a representation which the mind refers to something exterior, and which has for it the same value as that produced by a material object." (tom. xii, p. 243.)

After citing several cases for the purpose of exhibiting the extent to which perception, memory, and imagination may become, either conjointly or separately, affected in these hallucinations, the author observes, that they are capable of a division into active and passive:

"The association of ideas in the normal state is effected in two modes. Sometimes it takes place in a spontaneous manner, without any effort of the mind; and sometimes with reflection, i. e. under the influence of the will. What man is there who is not incessantly liable, however little he may allow his imagination to wander, to the access of thoughts which he did not seek, and which he was even far from expecting? On the other hand, in the continued current of ideas, to use the expression of Dugald Stewart, which passes through our mind, any one may choose, as he likes, some one of such ideas, retain it, and make it the object of his attention. Now, just the same thing happens as regards hallucinations; which may be sometimes *spontaneous* or *passive,* sometimes *voluntary* or *active.* Those which are developed without any effort of the mind, which come upon consciousness when unexpected, are the most common. Those which the mind evokes, directs, and commands are rare." (tom. xii, p. 250.)

M. Aubanel has endeavoured to set aside the distinction, so carefully established by Esquirol, between vicious perceptions of real objects, or *illusions of the senses,* and *hallucinations.* He believes them to be identical psychological acts:—1, because in the one, as in the other, there exists a delirium, the result of which is a false sensation; 2, the acts are often united, and may succeed or replace each other; and, 3, their intimate nature is the same. M. Michea, however, observes that illusions differ from hallucinations in this essential point,—that, while in the former the

object of error is actually presented to the senses, but is viciously perceived,—in the latter, either this object is purely fanciful, or, if it is real, it comes not within the sphere of the sensorial surfaces, and is for them as if it existed not. Moreover, the phenomena possess very different degrees of energy and intensity; and their conjoint or successive existence denotes no more than analogy.

However great the analogy between *dreams* and *hallucinations*, the conditions under which they are manifested are too different to allow of the admission of an identity. *Dreams* imply sleep, during which the mind "retires within itself," quitting the organs of the senses, which fall into inactivity. The antagonism between the external and internal world ceases, and no means of contrasting reality with appearance, the phantoms of the imagination with the perception of true objects, exists. The mind can no longer govern the connexion of its ideas, or modify their direction; it is no longer mistress of the will. For *hallucination* the waking state is a necessary condition; and the mind, so far from retreating within itself, expands upon the organs of sense, which possess their full activity. It may oppose the external to the internal world, reality to appearance; and may terminate, regulate, or change certain combinations of thought; and, save in the case of insanity, is possessed of free action. In *dreams*, time and space are confounded, without causing the least surprise; men and things of the most opposed nature are commingled, without the absence of transitions being perceived, or any question of singularity being raised. In *hallucination*, the fantastic sensations always excite astonishment, sometimes fear; and explanations of the strange phenomena are eagerly sought, according to the intelligence of the epoch or of the individual, in the power of magic, sorcery, physical laws, physiology, or pathology. Moreover, the *subjective perceptions* are very different. In *dreams* they are remarkable for presenting a rapid succession of forms, a coherent series, which form a picture, having all its personages and its perspectives, a drama with all its incidents,—but which have nothing fixed in them, appearing and then disappearing, to give way to other scenes. *Hallucination* generally manifests itself by the presence of isolated phantoms, which remain the same during its continuance. In *dreams* it is rare for the same objects to present themselves several times in succession, except in the case of somnambulism; and the images are soon effaced and forgotten. So, too, they may be considered as almost normal physiological incidents, which occur to most persons; while *hallucinations* are anormal and pathological, and exclusively affect certain individuals. Although the subjective phenomena of hallucination approach nearer to natural or artificial *somnambulism*, yet in the one there is always consciousness of the phenomenon, whatever time may have elapsed after its cessation; while, in the other, this is extinguished on waking. Hallucinations may manifest themselves during the transition from waking to sleep, and from sleep to waking; and the object of the dream may become a nearly faithful representation of that of the hallucination; and, on the other hand, the object of the hallucination may be an exact copy of that of the dream.

There is often much difficulty in distinguishing between *idiopathic* hallucination, derived from a lesion of the nervous centres, and *symptomatic* or *consecutive*, depending upon a disturbance of the peripheric

portions. The distinction is of importance as regards prognosis; symptomatic hallucination possessing a much less unfavorable character than idiopathic, inasmuch as the disturbance of the brain is merely slight and accessory.

"An hallucination should be regarded as *symptomatic*, when it unites several or all the following conditions:—1. When it is limited to a single sense. 2. When it affects only one of the symmetrical organs of that sense, as a single eye or ear. 3. When it disappears or diminishes in a very short time, under the influence of narcotic substances applied to the organ of sense itself. 4. When it relates only to inanimate objects, or at least to objects which have no relation to living and intelligent beings." (tom. xii, p. 261.)

The first three are obvious inferences; and the last is derived from the facts of experimental physiology, from which it appears that, when any decided modification of the sensorial expansions has taken place, the false sensation exhibits itself in the form of confused luminous lineaments, inarticulate sounds, or odours belonging to the inorganic world, as the case may be; while, in insanity, the false perceptions almost always consist of articulate sounds, forms of men or animals, animal odours, &c.

The author furnishes numerous references to cases, exhibiting the *causes* of hallucinations. They may be either *material* or *psychological*. Among the first are the abuse of alcohol, change of temperature, various medicinal substances, mechanical irritation of the organs of the senses, concussion of the brain, hereditary tendency, worms, prolonged privation of light, the period of life (35 to 45), and female sexuality. Among the latter are the too long duration of sensations, too forcible impressions, concentrated attention, isolation, remorse, terror, and various depressing passions.

Although the existence of hallucination is compatible with complete integrity of reason, it is in most cases pathognomonic of insanity, in some instances preceding and causing the loss of reason, in others occurring only consecutively as the effect of insanity. It is a pathognomonic symptom of insanity, but not a constant or necessary one; occurring in about one half of the subjects of that disease, and oftener in monomania and mania than in dementia and stupidity.

Hallucinations of *hearing* are the commonest; then, in succession, those of sight, touch, taste, and smell. Several senses may be simultaneously affected, the combination of that of hearing and sight being the commonest; triple and quadruple combinations are rare, and the quintuple never met with. False perceptions have a wider range of action than true ones; for, not only do they embrace all objective realities, but also give form to all the caprices of thought, the fancies of the imagination, and all the chimeras of the subjective world.

Hallucinations may present themselves as symptoms of hysteria, ecstasy, epilepsy, typhoid fever, cerebral congestion, meningitis, encephalitis, &c.; and, much more rarely, of intermittent fever, pneumonia, scorbutus, and gout. References to such cases are given.

The paper furnished by M. Baillarger, in reply to the same question, is a very diffuse one, occupying some two or three hundred pages; but the views of its author may be exhibited in a moderate space. He observes

that the study of hallucinations naturally divides itself into a physiological and a pathological portion; and on the present occasion he entirely confines himself to the illustration of the "*Physiology of hallucinations*."

The phenomena of hallucinations. M. Baillarger, believing that the phenomena can be best studied and explained by exhibiting them in all their varieties, supplies full details of these by the relation of a very great number of cases, which have fallen under his own notice, or which have been already recorded by others. These form a large repertory of classified facts, well suited for reference; but all we can now notice is the *résumé* with which he terminates this part of the subject, premising that he constitutes two general divisions of hallucinations,—intellectual and sensorial.

"1. The hallucinations of *hearing* are the most frequent and oftenest complex.—2. The simplest consist of scarcely else than different noises, or repetitions of the same words or phrases.—3. When they are more complex, they, so to speak, reflect the preoccupations of the patient.—4. Sometimes it appears to the patient as if his thoughts or words were repeated aloud.—5. In most cases the invisible interlocutors speak to the patient in the second person, but in rare cases they speak of him; he being then present as a simple auditor of a conversation, of which he is the subject.—6. The number of the voices varies from one to ten or twelve. When there are two, one of these often seems to counsel the patient for good, the other for evil, the hallucination then reflecting the combats of conscience.—7. The patients often enter into long conversations with the invisible persons. Sometimes they give both question and answer, but in different tones of voice,—the one being the natural one, the other a guttural, which renders it difficult to understand them.—8. The patients have sometimes the power of reproducing their false perceptions at will.—9. In the case of the patient knowing several languages, the voices generally employ that with which the patient is most familiar.—10. It is far from being proved that the intellectual power of the patient is ever superior during the existence of the hallucination, compared with its normal condition in the individual.—11. The false perceptions of hearing are not infrequently the reproduction of prior vivid sensations.—12. The voices heard are sometimes loud and deep, or they may consist in murmurings or whispers. Sometimes they make no noise, and are then the 'secret or internal voices.' The patients, too, hear 'thoughts' at a distance, and converse 'soul with soul.'—13. The directions whence the voices come are very various, and the distance sometimes very great, so that these patients frequently speak of employing a speaking-trumpet.—14. They are sometimes heard only by one ear; and at others by other parts of the body, as the head, chest, but especially the *epigastrium*. In this last case the existence of a vivid sensibility of the part, and a kind of ventriloquism which the patient sometimes produces by speaking with his mouth shut, appear to be the probable reasons for this locality being selected.—15. Hallucinations are almost always found in the insane deaf.—16. The study of the phenomena in the false perceptions of the organ of *sight* is much simpler.—17. They sometimes constantly reproduce the same object, but generally very different ones.—18. The images are sometimes so vivid that the patient can indicate the minutest details; but at other times they seem as if seen through a veil or gauze.—19. The personages seen may be at rest or in motion. These transformations succeed rapidly.—20. The images persist for a variable time, and before disappearing seem to become vaporous, commingling with the air, portions of them long continuing visible.—21. They are sometimes visible by night or day indifferently; and oftentimes they appear only during darkness, being dissipated by light. In some patients, the closure of the eyelids suffices to produce them.—22. The blind sometimes suffer from hallucinations of vision.—23. In respect to the hallucinations of smell, taste, and touch, we are unable to separate the intellectual and sensorial phenomena.—24. Those of smell and taste are especially

observed in acute disease, and are distinguished with difficulty from illusions of the same senses.—25. Patients suffering from hallucinations of touch feel as if struck or seized by invisible hands, as if insects were crawling on the surface, &c.—26. It is almost impossible to distinguish between illusions and hallucinations of general sensibility, upon which almost all the false perceptions of hypochondriacs depend. The pains still supposed to be felt in limbs which have been amputated, afford the most striking example of this kind of hallucination.—27. False perceptions, having reference to the genital organs, are very common in women; and to these may be referred all that has been said of incubi, succubi, &c.—28. Hallucinations which cause the patients to believe that their bodies enlarge or swell, or that they are raised in the air, explain, to some extent, the delusions of lycanthropes and witches. —29. The hallucinations of touch are sometimes only the reproduction of prior vivid sensations.—30. When false perceptions by several senses exist simultaneously, those of one sense especially predominate over the others.—31. It is especially in acute disease (as delirium tremens and febrile delirium) that simultaneous hallucinations of several senses are observed, the relations existing between which are explicable by the association of ideas." (tom. xii, p. 367.)

The nature of hallucinations. While some have contended that hallucinations are purely intellectual phenomena, totally independent of the sensorial apparatus, others believe they can never occur without the participation of the organs of the senses. The first have regarded hallucination only as an idea, reproduced by the memory and associated by the imagination; while the latter admit that there are sensorial impressions, as real as in the normal condition. If, indeed, hallucinations were studied only among the insane, and especially if we only took into account the false perceptions of the organ of hearing in chronic insanity, we should frequently be disposed to consider the phenomenon as purely psychical; but, on the other hand, the analysis of cases of hallucination of vision, in persons of sane mind, leads almost necessarily to the admission of the activity of the sensorial apparatus. The discrepancy of opinion is explicable by the different circumstances under which observations have been made; and probably, if the *ensemble* of these cases could have been considered, the conclusion that the nature of hallucination is not always the same in all cases would long since have been arrived at. The author therefore believes that a distinction may be made, such as is admitted already by metaphysical, if not by medical writers. Hallucinations may be composed of two elements, and may result from the combined action of the imagination and the organs of sense (*the psycho-sensorial*); or they may be due only to the involuntary action of the memory and imagination, and wanting the sensorial element, are incomplete, and may then be termed *psychical*. The proofs of the *participation of the organs of sense* in the first of these classes are derived—first, from the conviction of the reality of the impressions felt by persons who were temporarily liable to hallucinations, and who were well qualified to observe their phenomena accurately, as Burdach, Müller, Bostock, Gruthuisen, &c.; and secondly, from the testimony of the insane, and of those who have recovered from insanity, who distinguish most accurately between the false perceptions in question, and the inward ones with which they may be associated; their actions proving meanwhile the faith they have in the reality of such false perceptions. Several cases are given in long detail, exhibiting the effects of purely psychical hallucinations, in which the patient seems to hear rather the thoughts than the words; and, among others, that of a patient

who heard voices from his epigastrium, and easily distinguished these from other external voices, also occasionally heard. "Indeed, these false epigastric perceptions, so frequent in ecstasy and somnambulism, appear to belong without exception to the purely psychical hallucinations." Sometimes the patient's entire intellectual power seems to him to be exerted from this point, as occurred to Van Helmont after a small dose of aconite. The examination of what takes place in *dreams*, regarding the sensation of hearing, confirms the distinction laid down. A conversation is remembered, but not the voice; or rather, we feel certain that no such voice existed, and that no sensorial impression was perceived.

Conditions favorable to the production of hallucination. These conditions are,—1. The involuntary exercise of the judgment and imagination.—2. The suspension of external impressions.—3. The internal excitement of the sensorial apparatus. In respect to the first two of these, the author believes that the following propositions are demonstrable:

"1. That all the conditions characterised by the involuntary exercise of the memory and imagination, and by the more or less complete suspension of external impressions, are generally accompanied by psycho-sensorial hallucinations.—2. That the voluntary activity of the mind, so far from directly provoking the false sensorial perceptions, suspends them when they exist.—3. That the causes which induce the involuntary exercise of certain faculties and the suspension of external impressions, are at the same time those which induce hallucinations.—4. That the patients, during the continuance of these false perceptions, are in a special condition, characterised, besides the hallucinations, by the involuntary exercise of the memory and imagination, the suspension of external impressions, and the more or less complete inertia of the will." (tom. xii, p. 429.)

The conditions indicated in this *first* proposition are the state intermediate between sleep and waking, dreaming, and the form of insanity characterised by melancholia with stupor. The case of Nicolai is cited in proof that the active exercise of the faculties is an obstacle to the production of hallucinations; for distinct as these were in his case, he had no power of reproducing them by any amount of mental tension: and the observation of numerous other cases shows that such images are especially liable to occur just as active occupations are ceasing. Great, in the *second* place, as is the obstacle offered by attention to the *production* of hallucination, it is yet more easy to demonstrate its power in *suspending* it when present. The author has long remarked with what ease such temporary suspension may be so induced in the insane. In respect to the operation of the causes, as stated in the *third* proposition, this takes place in two modes. Thus the depressing passions and certain medicinal agents, as opium, datura, and especially Indian hemp, exert a direct action; while the exciting passions, and most stimuli, as alcohol, &c., do not directly give rise to hallucinations, inasmuch as these do not appear during the stage of excitement, but in that of collapse which follows this. As regards the special state in which the subjects of hallucination are found, Esquirol compares it to that of sane men engaged in profound meditation. But, in fact, nothing can well more differ from this; the only points of analogy between the two conditions being an isolation from the external world, and a suspension of external impressions. The attention of the meditating man, concentrated upon some point, is the highest degree of intellectual activity; while in hallucination there is no attention

whatever, and the exercise of any faculty is involuntary. In the one the tension of the spring is extreme, in the other it is utterly relaxed. One of the most frequent symptoms of the incubation of insanity, is the frequent occurrence of reverie, into which the patient falls again and again after being roused from it. And so is it with the subject of hallucinations, who has again the false perceptions, as soon as the stimulus, which for a while had suspended them, is diminished in activity. In slight cases, this suspension of consciousness may not be present; patients sometimes calling external impressions to their aid, to assist them in struggling against the hallucination. In all cases, however, we have the *involuntary exercise* of *memory* and *imagination, and the more or less complete suspension of external impressions,* as the primary conditions favouring the production of the phenomenon.

The *third* condition (*internal excitement of the sensorial apparatus*) is requisite for the production of complete hallucinations; the first two being also present in *dreams*, which are not necessarily accompanied by hallucinations. It may be inferred that a special excitement of the sensorial apparatus exists, from the following facts:—1. Intellectual phenomena are often produced without sensorial, showing that these last depend upon a distinct cause. 2. Predisposition of this or that sense to hallucination proves that this cause may act separately on one or more senses. 3. Facts directly prove that the excitement of the sensorial apparatus concurs in the production of hallucinations. 4. The causes which induce hallucination are at the same time purely sensorial phenomena. 5. These phenomena often precede or accompany hallucination. 6. In some patients who have suffered from psycho-sensorial hallucination, the sensorial element ceases, the psychical alone persisting.

Mode of production. Having admitted the compound nature of psycho-sensorial hallucinations, the question presents itself—Are they produced from without inwards, or the reverse; and is the point of departure of the phenomenon seated in the sensorial apparatus or in the brain? The author agrees with those who believe that hallucination differs from normal sensation, inasmuch as its origin is always in the intellect; and so far from the imagination being excited by impressions proceeding from the organs of sense, the sensorial apparatus is on the contrary excited by it,—the false perception taking place from *within outwards*. But admitting this as the point of departure of the hallucinations, the *mode* of *production* of the false perceptions only becomes the more difficult to conceive, easy as this would be did the organs of sense form the point of departure; and here we are obliged to confess our ignorance. A *psycho-sensorial hallucination* then may be defined as follows: "*A sensorial perception independent of all external excitement of the organs of sense, and having its point of departure in the involuntary exercise of the memory and imagination.*"

"We do not say that the phenomenon is a false perception, but really a false sensorial perception, thus affirming the reality of the sensation. This sensorial perception is independent of all external excitement of the sensorial apparatus, which distinguishes it from illusions of the senses. We add, that it has its point of departure in the involuntary exercise of the imagination and the memory, and so separate it from sensations which certain external and internal excitations of the organs of sense produce, such as luminous spectra, humming in the ears, &c. In

admitting the intervention of the intellectual powers, we admit the complex nature of the hallucinations, and their psychical and sensorial elements." (p. 470.)

Psychical hallucinations are produced under the same conditions as the more complex ones, save that the internal excitement of the sensorial apparatus is no longer necessary. They indicate less disturbance than the former, are more easily produced; more continuous, more easily suspended and renewed.

The influence of the state intermediate between waking and sleep, in the production and progress of hallucinations. This is a separate paper by the same author, forming a small contribution to the pathology of the subject. He details at considerable length the cases of about twenty lunatics, and corroborates his conclusions by reference to some other cases published by various authors, who have reported them without reference to the views he entertains. These data are, however, few in number, owing to the imperfect manner in which most of the recorded cases are detailed.

The result of the combined observations is to exhibit the marked influence which the condition between waking and sleeping exerts in the production of hallucination, in persons predisposed to or suffering from insanity. Various authors, as Foderé, Calmeil, &c., have noticed the influence of the night in producing or aggravating paroxysms, without specifying this particular condition. Hallucinations so arising are not to be confounded with those which may occur in our dreams during actual sleep. They affect the imagination much more vividly, and their occurrence is of far graver import. Hallucinations occurring during dreams are doubtless signs of cerebral excitement: but they may long exist without giving rise to any fears of insanity; while if those in question, which more nearly approach to hallucinations of the waking hours, long persist, they become at last continuous, and lead to insanity, at least generally. The symptom becomes of great importance in persons whose relatives have been insane, or who manifest other predispositions to insanity; and still more important is it, as one of the premonitory symptoms of the attack itself. The time which elapses before they become continuous is very variable; and sometimes after having done so, they become again intermittent before disappearing. It sometimes happens that the insane, having one or more senses hallucinated during the day, have a different one so affected in this intermediate state; and so distressing to some patients are the sensations which precede sleep, that they delay the hour of rest as long as possible. Since his attention has been directed to this subject, the author has observed that many patients suffering from severe nervous affections, do so most acutely just at the time of going to sleep; and he believes this subject is well worth examination as regards epilepsy, the attacks of which so often occur at night. The most common case is to meet with these hallucinations only just before going to sleep. It is rare to find them *only* in just waking from it; but more common to meet with them both immediately before and after sleep.

Dr. Baillarger makes the following remarks upon the *medico-legal* bearing of the subject.

"A German peasant, waking up in the middle of the night, struck a blow with an axe at a phantom he saw before him. In this manner he killed his wife, with whom he had always previously lived on the best terms. Up to this time he had

exhibited no symptoms of insanity, nor did he manifest any after the event. This case, of so interesting a nature to the medical legist, gave rise to a prolonged consultation, of which Marc (De la Folie, 1840) has given a full translation, considering it as a model of what these investigations should be. The consultants, after referring to the kind of delirium which often supervenes at the moment of sleep and waking, declared their opinion that this murder was committed during that condition. It is a singular thing that this opinion, maintained by the most celebrated physiologists, was not attempted to be supported by any facts of mental pathology. Neither Hoffbauer nor Marc, in the chapters devoted to the examination of this intermediate state, in its medico-legal bearings, allude to any one fact observed among the insane. It is the silence of authors so learned, which has especially induced me to publish the present cases. They will be referred to in those instances in which hallucination is alleged as an excuse for the commission of a murder during the night by a man who in the morning may be perfectly sane. We have seen that in one case hallucinations during the passage from waking to sleep, became continuous from the very first time during only a portion of the night, no traces of such transitory insanity remaining in the morning. We should always endeavour to learn whether the person charged with the murder had laid down and had commenced slumbering, or whether it was at the moment of waking that the hallucinations commenced." (tom. xii, p. 516.)

On the Bile and its Diseases; by Dr. V. A. FAUCONNEAU-DUFRESNE.

Nearly the whole of the Thirteenth Volume of the Mémoires is occupied by this elaborate prize essay, which is certainly the most complete monograph extant upon the subject to which it relates. We are unwilling to give such an imperfect account of it as our present limits would permit; and we shall therefore postpone our analysis of it until our next Number.

On Fracture of the Neck of the Femur, accompanied by Penetration into the Spongy Tissue of the Trochanter; by M. Robert, Surgeon of the Beaujon.

Mode of production. The author observes, that although several authors since the time of Desault have alluded to this species of fracture of the cervix, none, except Smith, (Dub. Journ. vol. xviii,) have given any complete description of it. For the production of fracture with penetration, it is necessary that the bone should be conical, and decrease in density of structure from the apex to the base of the cone; and that the fracturing force should so act, as to bring the two extremities of the bone violently against each other. 1, The neck of the femur consists of two truncated cones joined together at their apices; and 2, the cancellated structure is so disposed as to become very dense as it approaches the head of the bone, but sparing in quantity, and more fragile in texture, towards the trochanter; 3, the cause of the accident is a fall on the trochanter, or on the outer part of the thigh. The trochanter is so disposed that a prolongation of the axis of the cervix does not fall upon the middle of its external surface, but upon the ridge that separates the free posterior portion from the anterior half which is blended with the base of the cervix. In falls upon the trochanter, the cervix is compressed between two forces acting in different directions; viz. the weight of the body acting along the axis of the cervix, and the force of the fall transmitted along that of the trochanter—the bone becoming bent and broken at the point of intersection, i. e. the junction of the base of the cervix with the trochanter. A secondary effect of the violence is to produce penetration.

Anatomical characters. Penetration is a frequent complication of *extra-capsular fracture.* Of 14 cases in which the *age* was recorded, 2 were between 50 and 60, 4 between 60 and 70, 5 between 70 and 80, and 3 between 80 and 90. It generally occurs in robust persons, in whom the extreme fragility which favours simple fracture does not exist. The penetration may be incomplete or complete. It is the *incomplete* variety that is usually met with. The head of the femur is carried more or less backwards, so as to be almost opposite the small trochanter, while the anterior surface of the neck forms an angle with the body of the bone, projecting forwards. Sometimes beyond the level of the trochanter, a cutting ridge, doing much harm to the soft parts, is formed by the base of the neck. Behind, the base is more or less thrust into the spongy tissue of the trochanter, the neck being shortened, and the head applied to the inter-trochanteric ridge. The vertical direction of the neck is also altered, becoming horizontal, or inclined downwards; at other times it undergoes a degree of torsion on its axis. In *complete penetration* the entire surface of the base is plunged in, sometimes extending only to a very slight distance, and at others completely traversing the entire thickness of the bone. In these last cases there is little deviation of the axis of the cervix —the shortening here being only due to the amount of penetration.

Complications. Few extra-capsular fractures occur without *fracture of the great trochanter,* an accident which so rarely occurs when the fracture takes place nearer the head of the femur. It does not exist in any of the 23 examples of *intra-capsular* fracture in the Dupuytren museum. It is generally the posterior portion which is fractured, this being the thinnest part, and least able to offer resistance. Generally the fragments are kept together by the fibrous parts surrounding them; but if the penetration be considerable, they may be separated as if by a wedge (Travers). The membrane lining the spongy tissue becomes lacerated, and may produce considerable *hemorrhage,* the blood infiltrating into, and ecchymosing the soft parts at the exterior of the thigh.

Symptoms. The most important are shortening of the limb, and the rotation of the foot outwards. 1. *Shortening* is an essential symptom, being produced by the diminution of the length and obliquity of the cervix; but except in the rare cases in which the head of the femur is excessively depressed, and the cervix at an acute angle to the body of the bone, it takes place to no great extent, varying in 13 cases from 1 to 3 centimetres. It is always, however, to be borne in mind that the comparative measurement of the two limbs is more difficult than is generally supposed, especially as regards slight differences. 2. *Rotation outwards* was noticed in 15 out of 16 cases, but it is usually less marked than in ordinary fracture of the cervix. 3. *Pain* is usually much *more severe* than in simple fracture of the cervix, and especially the intra-capsular. It is felt in the trochanteric region, is increased on the slightest movement or pressure, and is much more persistent than is that from contusion. 4. *Ecchymosis* to a remarkable degree is manifested at the outer side of the limb a few days after the accident. 5. *Swelling over the trochanter.* This part is found more voluminous than normal, and M. Herviez de Chegoin attributes this to the enlargement of the trochanter by the penetration of the cervix between the fragments. But it is rare to find so complete an interposition; and the enlargement probably arises

from the swelling of the periosteum and soft parts around the trochanter, and later, also, from the osseous productions which are found in the periosteum. 6. *Possibility of walking.* Desault attributed the fact of the patient being able sometimes to raise himself, and walk after the accident, to the existence of penetration; but the same power is sometimes observed where there is interlocking of the fragments, or preservation of the periosteum, though no penetration occurs; while, when this last is present, if the pain is very severe, all such power may be wanting. 7. *Power of raising the limb.* M. Herviez de Chegoin, in his memoir on this subject (Journ. de Méd., tom. 72), states this as a symptom, and the author has verified it; but in some cases the pain is too great, and in others the penetration too slight, to admit of it. 8. *Resistance to extension.* In ordinary fracture of the cervix, the shortening is removed by traction, again to reappear when this is discontinued; but in this case it is not so, and if the penetration be considerable, it will resist all efforts; a case in proof of which is given, which was mistaken and treated for a dislocation. A caution is here offered, worth noticing.

"This would be a useful aid in the diagnosis of the varieties of the fracture of the cervix, did it not present as many inconveniences as advantages. In fact, there are fractures which take place very near the head of the femur, and in which it is by no means rare to find the periosteum remaining uninjured, especially at the anterior part of the cervix; and it is evident that extension applied to such, however moderate, must have the effect of destroying the remaining periosteum, and deprive the fracture of a precious means of consolidation. There are also fractures with incomplete and slight penetration, which we cannot submit to such traction, without destroying the contact of the fragments, and rendering the formation of callus impossible. I think, then, we should reserve this means of exploration for those rare cases of fracture whose equivocal symptoms might lead to their being mistaken for dislocations." (tom. xiii, p. 505.)

Diagnosis. The simultaneous occurrence of *slight shortening* and *rotation outwards,* with *permanent pain* over the region of the trochanter, rarely allows of this being mistaken for other fractures of the cervix; for while in *intracapsular* fracture the pain is very slight, and referred rather to the groin than the trochanter, in the fracture at the *base of the cervix* the shortening and rotation are much more marked. When the other symptoms are obscure, the *severity and persistence of the pain* enable us to pronounce an opinion; and M. Gimelle, who, as surgeon to the Invalides, sees many of these cases, believes that this symptom alone suffices in some cases for diagnosis.

Prognosis. Fractures with penetration are followed by permanent shortening and eversion of the limb; but then they consolidate more securely and more rapidly than do the other forms of this accident. In from 40 to 50 days the patient is enabled to bear somewhat on the limb.

Treatment. The treatment of fractures of the cervix femoris has given rise to a greater difference of opinion than that of most others. This has arisen in a great measure from surgeons having too exclusively regarded the shortening and eversion, without considering whether these always arose from the same anatomical lesions, or called for the same mode of management. While the displacement of ordinary fracture so much depends upon muscular action, it here arises from a mechanical disposition which is not to be disturbed. The indication, then, is to abstain from all

traction, to retain the patient as immoveable as possible, and apply a continued lateral compression from without inwards. The author lays the patient on his back, with a cushion under the ham, and passes a tight bandage around the haunches.

Case of Vesical Calculus formed around a Cobbler's Awl, and removed by Lithotomy; by Dr. Fleury.

The subject of this case was a healthy countryman, who, though he had long suffered intensely from an irritable state of the bladder and urethra, retained at the time of admission most of the signs of good health. A stone having been felt, and the incision for its removal completed, its extraction was found to be very difficult. On introducing the finger into the opening, a pointed body was felt, which was believed to be the apex of the stone; and the forceps then seized a large calculus and crushed it, the pointed body still remaining implanted in the walls of the bladder near its neck. By repeated manœuvres this was at last dislodged and removed, and proved to be a portion of a cobbler's awl, eight centimetres long, all parts of which, except the two extremities, were covered with incrustation, the forceps having crushed that portion formed around the centre. The patient, with every appearance of a man speaking the truth, repeatedly denied having ever introduced this instrument into the urethra; and could only explain the occurrence by the fact of his companions, while he was asleep, some eighteen years ago, having introduced a sharp instrument into the *throat*, which slipped from their hands.

Seven days after the operation, air was observed to escape from the wound and the penis; and the patient stated that he had observed the same thing long prior to its performance, his urine also frothing like beer. On examination, a small aperture in the rectum, just above the anus, was found, communicating with the perineal wound. The patient was discharged cured six weeks after the operation. In reference to the history given of his case by the patient, Dr. Fleury observes:

"It would indeed be astonishing if a metallic body of this length could traverse the intestinal canal and perforate the recto-vesical partition without causing accidents. What, too, became of it during all these years? The problem is so insoluble, that it seems more rational to admit that the awl entered the urinary passages directly, ulcerated the walls of the bladder, and gave rise to the fistula which still exists." (tom. xiii, p. 536.)

On the Employment of Prolonged Baths with Continuous Irrigation, in the treatment of Acute Insanity; by M. Brierre de Boismont.

In this paper M. Brierre gives fifteen cases in considerable detail, as exemplifying the importance of the results he believes he has arrived at. The entire number of facts, however, upon which his conclusions are based, amount to 72, which are thus distributed:

35 cases of acute mania, of which 33 recovered.

11 of delirium tremens, all of which were rapidly cured.

10 of monomania with agitation, some having a tendency to suicide, and all cured.

10 of manial excitement, differing from mania by shades acknowledged by observers; 7 cured and 2 only ameliorated.

6 various forms of mania, all of which yielded, except one chronic case.

Sex and age. Of the 72 cases, 30 were men, and 42 women; only two patients were less than 20, and twelve more than 50 years of age. The disease on admission had lasted from 1 to 15 days, and in the majority from 3 to 4 days.

The duration of treatment varied from 1 to 14 days; but after 8 or 10 days, the baths were usually administered less regularly. 17 patients took them for eight days without omission; 10 for three days; 9 for four days; 8 for six days; 5 for five days; 5 for ten days; 2 for two days; 3 for 12 days; 2 for one day; 1 for nine days; and 1 for fourteen days.

The prognosis of acute insanity is almost always favorable when it is recent, if there have not been prior attacks, or if its form be not intermittent. Although hereditariness does not retard the cure, it gives rise to great fear of relapse.

The *baths* should be taken in a half-dark place, as the sunlight and the sight of surrounding objects increases excitement; and the treatment is the more efficacious if the patients are bathed separately. The *duration* of the baths is from ten to twelve, or even fifteen hours, while in some cases it does not exceed seven or eight hours. The *temperature* should be from 82° to 86° F., a little lower in summer; the bath varying from 64° to 68°, when the patient quits it. It is not unusual to find that the patient, after being in the bath for seven or eight hours, has become much quieter; and reason is sometimes re-established after the very first bath. Generally, however, after some hours, and especially towards midnight, the agitation returns. Sometimes patients are violent during the whole period, striking themselves against the bath. Such must be restrained by waistcoats and garters which make no injurious compression. The covered *baignoires de force* employed at Charenton and Salpêtrière are convenient, or a cover may be adapted to an ordinary bath, so adapted as to allow the head to project. The *irrigations,* which are simultaneously employed with the baths, consist of a small stream of water, at a temperature of 58° F., passed through a tube having a diameter of about a centimetre. They are very efficacious for subduing delirium, and sometimes even for curing it.

The author observes that there is always a danger of a means which has been found efficacious being elevated into the rank of an exclusive one. But although he has repeatedly seen this one alone cure insanity, he does not confine himself to its use. Thus, when the patient is very much excited, and his pulse rapid, he resorts to *low diet* and *purgatives.* After trying many of these last, he believes that calomel is the best and most easily given, its use being admissible for several successive days, as it rarely causes salivation. So, too, in plethoric patients suffering from suppressed discharges, or accustomed to bleeding, he resorts to *venesection,* though with very great caution.

In consequence of M. Brierre's well-established reputation, we give the above notice of his communication; but at the same time cannot but express our doubts as to the accuracy of the statement in respect to the proportion of cures, if aught else but temporary alleviation is intended by his term. Moreover, when we consider the duration of the time during which the patients are kept in the baths, the force sometimes required to retain them there, and the state of suffering in which they seem to be on quitting them, we strongly doubt whether the practice is worthy of imitation.

On Catheterism in the treatment of Dysphagia produced by Simple Stricture of the Œsophagus; by M. Trousseau.

M. Mondière, in a valuable series of papers (Arch. Gén., 1831-3) upon diseases of the œsophagus, opposed sound arguments to the fears raised by Boyer and other surgeons, in reference to catheterism in stricture of this tube; and M. Gendron published cases in 1837 (Journ. de Connais. Méd.-Chir.) well calculated to confirm these views. M. Bretonneau has also resorted to the operation, and it was in a patient under his care that M. Trousseau first had an opportunity (1845) of observing the value of the proceeding. The case occurred in the person of a woman of about 30, who had long tried antispasmodics and the usual remedies in vain. Fifteen days of catheterism restored the calibre of the canal, and the cure has continued complete. Not long after this he was called to a lady more than 60 years of age, who, having suffered from an angina ten years since, had gradually found a difficulty of swallowing increasing upon her, so that at last she could only swallow fluids, and that with great deliberation, since the least haste induced dreadful suffocative paroxysms. There was no pain, but a slight sense of uneasiness just below the larynx. An instrument was passed in this case twice a day for a fortnight, and in a week deglutition of fluid had become easier, while in a month small quantities of solid bodies were swallowed without great difficulty. The instrument was employed seldomer, but yet was not quite abandoned for some months, when a sponge as large as a pigeon's egg could be easily passed through. The patient recovered her strength and flesh, and could eat without difficulty.

Another case is given of more recent origin, and more rapid cure. A young woman perceived, a fortnight after the occurrence of a severe diphtheritis, a difficulty of swallowing just opposite the larynx, so that even drinks could only be swallowed with great care. Catheterism was resorted to, and although the obstacle offered some resistance, yet the very first day after she swallowed more easily, and in a fortnight solids passed so readily, that the instrument was then used only on alternate days for three weeks. M. Gendron has communicated to the author a case occurring in a young man quite recently, in whom simple catheterism having produced but little benefit, the nitrate of silver was resorted to with the best success.

The operation is simple enough. To the end of a slender whalebone, terminating in a small oval button, is to be attached by sealing-wax a piece of fine and dry sponge, which is afterwards to be moistened and cut to a proper form. For greater security, this is also fixed by a piece of packthread, the ends of which are left long enough to remain within reach. The sponge, well smeared with white of egg, is conveyed to the isthmus faucium, and then, by pulling at the two ends of the packthread, the end of the whalebone is so bent as to be easily directed into the œsophagus. Without this little manœuvre, contrived by M. Bretonneau, great difficulty may be found in engaging the instrument in the canal. During this time the head is held firmly back, and as soon as the whalebone has entered the passage, the threads are loosened, and it proceeds on to the obstacle; and as this is almost always found opposite the larynx, it should be passed as quickly as possible with the one hand, while the larynx is supported with the other. If the passage were made slowly, an

intolerable feeling of suffocation would result. As soon as the obstacle is passed, which it is easily felt to be, we draw up the sponge, to repass it a second or third time—the whole occupying only a few seconds. The size of the sponge is gradually increased, until one as large as the ordinary alimentary bolus is employed.

On Pellagrinous Paralysis; by M. Baillarger.

The pellagra has been so diligently studied of late years, that it is now well ascertained to be very prevalent in Lombardy, while it is also found in some parts of Spain and the south of France, attacking chiefly the poorest portions of the peasantry. Appearing at first as a slight erythema of the hands, it may become eventually complicated with the most alarming and fatal symptoms. An uncontrollable diarrhœa may wear out the patient; while, at other times, meningitis, mania, melancholy, suicide, or dementia are among the dreadful consequences.

This variety of symptoms explains the different names which have been given to the disease; and, among the rest, is an order of symptoms which have been termed by Aldalli *scorbutic paralysis.* This pellagrinous paralysis attacks all the limbs simultaneously; its progress is slow and gradual, and it terminates in dementia. In these respects, its resemblance to the *general paralysis of the insane* is obvious; but there are symptoms said to be wanting in it which prevent our regarding the two as identical, viz. the *embarrassment of speech* and the *ambitious delirium.* This last exists, it is true, in the paralysis of the insane, in every possible degree, from mere self-contentment to the most brilliant illusion, accompanied with the greatest difficulty of enunciation and nearly entire privation of movement. The author, having recently had an opportunity of observing the disease in the hospitals of Lombardy, resolved to assure himself respecting its identity. He has collected twelve cases, only the chief particulars of which, to save space, are introduced into this memoir. He met with one in which embarrassment of speech and ideas of grandeur prevailed to some extent; and he observed other symptoms, such as forcible grinding of the teeth, and the automatic movements of the lips and jaws, which he has since verified in the general paralysis of the insane. The two diseases also much resemble each other in some of their predisposing causes; but it is in their *hereditariness* that they do so especially; for, just as the author has before shown, in respect to insanity, that this disease is transmitted oftener by the mother than by the father, and oftenest by the mother to the daughter and by the father to the son, so Calderini has shown that the same law operates in respect to pellagra.

Of the *disposition to suicide,* so very common in pellagra, Dr. Baillarger thus speaks:

"This symptom belongs almost exclusively to pellagrinous insanity, and admits of the following explanation. The disposition to suicide is never observed so frequently in any form of insanity, as in that described by Esquirol as acute dementia, and by Georget as *stupidité.* It exists among at least a third of such patients: and, in determining the probable number of suicides among a given number of insane, we require before all things to know the frequency of acute dementia. In our asylums we find nearly 3 per 100 of this form, while M. Pelto, of Venice, states that the proportion in the pellagrinous insane is 30 per 100. Strombio says, that pellagrinous patients kill themselves without having manifested any signs of fury, and this is just what takes place in acute dementia. These

silent, inert, motionless, and apparently stupid patients endeavour to kill themselves without having manifested the slightest exaltation, the act being apparently purely automatic.

"The great prevalence of acute dementia in this disease is also easily explicable, for *stupidité* is observed especially in patients of enfeebled constitution. It has been observed as a consequence of loss of blood and of abstinence, and Sydenham signalises it as one of the consequences of prolonged intermittent fever; so that we may easily conceive how pellagrinous patients, arrived at the second or third stage of the disease, are so liable to this species of insanity." (tom. xiii, p. 719.)

In the Italian hospitals the same remarkable connexion between general paralysis (non-pellagrinous) and ambitious delirium was observed as elsewhere. It was found equally among ignorant peasants, careless of the future, and in men who have been engaged in the full pursuit of honours and fortune.

On the Health of the Workpeople employed in the Government Tobacco Manufactories.

The Twelfth Volume of the Mémoires contains an interesting Report, from the pen of M. Melier, upon the above subject, in answer to an application made to the Academy by the late Government. He personally visited the manufactories, examined into the anti-hygienic influences of the various stages of the manufacture, and investigated the actual state of the men employed. We have no space to detail the account of the process of the manufacture which he gives, and can only generally state the conclusions at which he arrives as to its influence upon health. The condition of the 5000 or 6000 operatives employed seems to contradict alike the exaggerated statements of Rammazini, of the injurious character of this employment (the introduction of steam, however, since his time, has much alleviated the laboriousness of the occupation), and those of Parent-Duchatelet, who declared it an utterly innocuous, if not even a salutary occupation. It requires, indeed, the evidence of facts to destroy the influence of *a priori* conclusions, that the living amidst the vapours and dust of tobacco, and the manufacturing a substance containing so deadly a poison as *nicotine* (experiments as to the effects of which on animals are here related), must be very injurious to health and preventive of longevity.

A *seasoning* at least seems requisite, and a large proportion of those who attempt the employment, being assailed during the first week or two with cephalalgia, nausea, anorexia, sleeplessness, and diarrhœa, find that they are obliged to abandon the occupation. Those, too, who have to be exposed to the worst emanations during fermentation, or undergo the hard-work which some stages require, would probably sink in numbers from the diarrhœa and sleeplessness to which they are liable, unless they were occasionally relieved by others. A workman who has become seasoned acquires a cachectic coloration of the face, which is removable by the use of iron, and a peculiar cast of countenance, or *facies*.

This alteration in the colour of the face, the author attributes to the absorption of the poison into the blood, although the nicotine has not yet been detected in this fluid. Corroborative of this view is the excitement of the disorder of the digestive organs, and the more rapid dissipation of this by purgatives. So, too, in a female operative, the *liquor amnii* acquired a distinct odour of fermenting tobacco; and M. Horteaux has observed that, on abstracting blood from these persons, the coagulum

is very slight. They do not bear bloodletting well; and their ailments consist rather of passive congestions than of inflammations, as is shown by a menorrhagic disposition in the females. Notwithstanding the great sweating which occurs during some processes of the manufacture, the tobacco produces a powerfully diuretic effect.

But the evils of tobacco are not unalloyed. The workmen declare that they are longer lived than other operatives,—an assertion, however, destitute as yet of positive proof. It is certain that there are several old workmen, but these are generally asthmatic or short-winded. The medical officers of the factories, however, confirm another claim of tobacco, viz. its preservation from, or curative power over, rheumatism, sciatica, and various cutaneous affections. A statement has been put forward, too, upon the strength of considerable medical testimony, of its power over phthisis; but M. Melier, while allowing some weight to the evidence, maintains that, at present, it is too loose to admit of deductions being safely drawn from it.

Besides the papers we have noticed, these volumes contain the following, which do not seem to call for equal attention: *Intestinal Sutures*, by M. Moreau-Boutard; *Contagion of the Plague*, by M. Keraudren; *Excision of a Pilous Ovarium*, by M. H. Larrey; *On Imitation*, by M. Solly; *The History of the Philosophy of Surgery*, by M. Malgaigne; *A Report on the Salt Marshes of France*, by M. Melior; and *A Case of Scapulo-Humeral Hydrarthorosis*, by M. Jules Roux.

ART. VI.

Prison Discipline, and the Advantages of the Separate System of Imprisonment. By the Rev. J. FIELD, M.A., Chaplain to the County Gaol at Reading. Second Edition.—*London*, 1848. 2 vols. 8vo, pp. 900.

IT might at first glance be doubted how far a work like that before us could afford fit subject-matter for a medical review. More mature reflection, however, will, we believe, make it evident to all, that, interwoven as the duties of the medical practitioner are with the great cause of philanthropy in all its phases, still one of the more immediate and important connexions of our science with society is to be recognised in the active discharge of our duties, when alleviating the miseries of the unhappy inmates of gaols.

In what, indeed, does the great misery of imprisonment consist? Poor or imperfect diet — deprivation of accustomed exercise — deficient ventilation—exposure to cold and damp. Such, at least, were, until lately, the chief discomforts of prisons; and, to a greater or less extent, these physical evils are experienced in most of our modern establishments. Doubtless, under the separate system, mental anguish forms some part of the torture to which the criminal subjects himself; and it is only of late we have arrived at the important discovery, that in the assailable points of man's psychical nature we are to find the power, not only to punish, but to save.

Everything which interferes with the prisoner's physical comfort is, of course, under the control of the medical officer; or, more justly speaking, he is held responsible for any evil arising from such causes. Mental

discomforts, too, are early obtruded upon his notice as subjects for his anxious care and consideration.

Nearly everything, therefore, that distinguishes the prisoner from the free man consists in the application of certain conditions to the former, which constitute objects of inquiry for the physician, and which cannot be advantageously intrusted to any one who has not enjoyed the advantage of a medical education.

The volumes before us are evidently from the hand of a fervent and benevolent man, and treat of the separate or cellular system of confinement, not only from the results of his own experience, but with the assistance of various documents printed both in this country and abroad.

Our author is the chaplain of Reading Gaol, which was the first county gaol in England constructed and regulated upon the improved system of separate confinement; the Government prison erected at Pentonville having served as its model.

It may be as well to describe shortly the system of confinement distinguished as "separate;" since much confusion has arisen from the presumption that the separation of prisoners one from another must of necessity subject them to "solitary" confinement, a punishment, the effects of which were tried in America, where the most disastrous consequences were observed. These evils are dilated upon by Mr. Field; and the distinction between separation and isolation is carefully drawn in an early part of his work. The great and important distinction, then, between separate confinement and that total isolation now no longer practised in civilized countries, may be described as consisting in the former allowing free and unreserved communication on the part of every prisoner with the disciplined officers, schoolmasters, and trades' instructors, and also with the governor and other superior officers of the establishment, during their visits to the cells. Isolation, on the contrary, forbids all such intercourse, and even excludes the view of the human face from the sight of the prisoner during his imprisonment.

Again, we have to make a distinction between the "separate" and another system of confinement known as the "silent" system; inasmuch as separation, at a first view, might be considered as of necessity involving silence on the part of the prisoner. The silent system (now carried on in perfection at the Coldbath-fields prison) admits of association of the prisoners in workshops during the daytime; but, notwithstanding this, no word is permitted to pass from one to the other, and any breach of this rule meets with severe and inevitable punishment. The effects of association therefore, provided the silent system is correctly carried out, can influence the mind only so far as the organ of sight is concerned; the great and important element of association, viz. intercourse by speech and exchange of thought, being denied the prisoner. It thus appears clear that the *separate* system, psychologically and correctly speaking, possesses many advantages of association in which the silent system is deficient.

It will be our object here more especially to direct attention to the medical history of the separate system; and to show how far it is proved by experience to demand attention or approbation as a safe mode of punishment, and one which, by comparison with the health and mortality of other prisons, we are warranted in imposing on our fellow-beings.

We have no satisfactory records of the state of health as observed among

the prisoners first subjected to separation in this country. As early as 1781 a prison with separate cells was erected at Petworth; and about the same time a county penitentiary was built at Gloucester. These buildings were not of sufficient size, however, for the increasing number of prisoners; and after a few years the cellular or separate system was discontinued, as requiring too much room for the convenience of the establishments. It may be stated, that previous to 1781 nothing was known of the separate system; and it was many years before it was revived in America, after its disuse in this country. We find, however, that both France and America lay claim to the invention of this method of imprisonment; but with no show of truth, so far as we can discover.

An experiment was made on separation at the Millbank Penitentiary, previous to the opening of the Pentonville prison; but the system was never carefully carried out. The returns of the state of health, however, were not satisfactory; and there was a prevalence of mental disease observed, which was attributed to separation by those deputed to watch the experiment.

In 1832, the British legislature determined on appointing a commission to inquire into the state of the American prisons; and in 1834, a most interesting report was addressed to the Secretary of State by William Crawford, Esq.* Mr. Crawford visited the American prisons with a mind well prepared for the task, by laborious attention to the subject of prison discipline in England; and the consequence of his American inquiries was the erection of the prison at Pentonville.

This institution, which may be considered the parent of the Reading and other gaols since completed on the separate system, was opened for the reception of prisoners in December, 1842. In this establishment the Government proposed to put the separate system fairly to the test. The building was erected with every attention to warmth, ventilation, and drainage. The diet was to be liberal, the prisoners chosen as healthy as possible, and careful returns were to be made from time to time by the officers, and more especially by the superintendent of the medical department. The term of imprisonment was to be eighteen months.

As a part of the subject more especially interesting to the medical reader, we will now proceed to consider the remarks of the author on the subject of diet, a question which unfortunately appears never to have met with that attention from the medical profession, which its great importance demands. It is true that the opinions of those immediately connected with penal and charitable institutions have occasionally been asked; but the inquiry, in its broader and more extended relations, has been intrusted to most incompetent hands; and while we are unwilling to find fault with the errors of the inquirer whose education never fitted him for his task, we cannot but view with a lively feeling of regret the waste of public money and valuable time, which has been consequent on the employment of the incompetent, and on deliberations over their worse than useless reports. We trust the reader will not consider us to be trifling with an important subject, when we state that the worthy members of a great commission lately intrusted with the administration of

* This gentleman died suddenly, while attending a meeting of the Commissioners for Pentonville prison. He was a most valuable member of that board; and it is chiefly to his untiring resolution and able advocacy, that we are indebted for the success of the separate system in this country.

a most important law, had it clearly proved that the best health and smallest mortality occurred when the cheapest diet was exhibited; that the smallest allowance of nourishment was most conducive to health; and all this, too, while the richest diet under their consideration was one on which any member of the honorable board must have speedily emaciated. We have some difficulty in understanding how this could be done honestly. We know well, that incompetent as the gentleman was who made the inquiry—totally ignorant as he was known to be of physiology and other sciences necessary to the investigation, he still possesses some degree of natural capacity; and there are errors in his tables which a butcher or baker would detect at a glance, and of which he himself can only have failed to be aware, through most extraordinary carelessness.

Mr. Field in his work says but little of diet; and it would have been far better had he said nothing. Unprofessional men are too liable to believe the physiological falsehoods which carry wonder in their train, to be competent judges of things medical; and we can trace in the following passages convictions imprinted on the worthy chaplain's mind, which are plainly the offspring of early imbibed poor-law superstitions, and which would be most amusing to the better-informed, were it not for the sad reflection that they are adding force to mischievous opinions, already too forcibly impressed upon the vulgar mind. Hear, then, our chaplain:

"The various dietaries have been the subject of repeated discussion.

"Justice and humanity, although never really opposed, have on this question appeared at variance. To supply the criminal with a portion which the honest labourer can hardly by industry obtain, and equal to that which the innocent pauper is allowed, might seem to encourage offences; on the other hand, to withhold a sufficiency, especially under a system of imprisonment which has a tendency to depress, would be inflicting injury both present and permanent, and in all probability would prevent the efficacy of those means which now prove corrective. On the other hand, it must be observed that remorse and consequent depression is seldom lasting; and when it has given place to other feelings, certainly less food may be sufficient. Amidst difficulties of this kind, it is perhaps most safe to regulate the diet of convicts only with reference to health and as to quantity, irrespective of punishment. If this principle be admitted, it may be a question whether the food itself should not be so coarse as, although perfectly wholesome, to prevent self-indulgence; so that the prisoner, instead of eating to excess, should rather be induced to abstain from more than the calls of hunger require."

Why cannot Mr. Field take a more philanthropic and independent view, than that which would disgrace the ethics of a relieving-officer? Why does he not declare the reason why justice and humanity have on this question appeared at variance?—viz. that the question is a political one, and that humanity tells us, not that we should feed the prisoner worse, but that we should feed the pauper better;—while the honest labourer should feel that his hard toil will be surely crowned with those various blessings which are the products of the field, and the most obvious and immediate gifts of an all-merciful Father. This may not be the more immediately politic, but it is the correct and the practical view. The evil should be looked in the face, even though we cannot remove it; and if the prisoner is placed in punishment by the law, the law is bound to provide for his preservation in physical health, not only in justice to the criminal, but as a sure mode of preventing the production of an unhealthy convict population, which may be regarded as one of the most expensive

burthens to the community at large. Doubtless we may here be met by the declaration, that the increased advantage to the labourer and pauper cannot be brought about. We believe that it can, and that it will; and even if it cannot, nothing will excuse the barbarity of half starving a prisoner, because the state of our country inflicts corresponding misery on the poor. Keep the prisoner in health, while you strive to ameliorate the condition of the free.

Here we are met with the assertion, that we induce crime by holding out a good prison diet as a temptation. Now let us consider the facts connected with this; let us look to the number of voluntary prisoners, and the conditions under which they appear as candidates for gruel and ox-head broth—these engaging specimens of humanity, forming the stock in trade and especial pets of the powerful barbarous. In the first place, these voluntary prisoners have been found quite as frequently, and in as large numbers, in the gaols of Scotland, where the diet has always been low, as in those gaols more favoured in that respect. These voluntaries have never existed in large numbers at all; and, when we regard the whole convict population, may indeed be said scarcely ever to have been met with. Yet to what importance have they arisen, how eagerly are their cases quoted, and how nauseously to the humane and philosophical mind are such instances adduced to defend the old system of wholesale murder, which has disgraced our gaols until within the last very few years! It is surprising that any one who has visited a gaol, can doubt that the irksomeness of confinement far outweighs the *agrémens* of the most choice prison diet, or that anything but approaching starvation (or prominently marked moral insanity) can induce the free man to hug the chain. The little boys who come to prison to get more food, whose cases are quoted, form instances not uncommonly met with among juvenile offenders. They make the best of their fate, and pretend to have gained an object, instead of incurred a punishment. How amused would they be, could they read the book before us. With regard to the statement that the remorse felt by the prisoner on first being subjected to discipline, requires that more food should be exhibited than need be given at subsequent stages of confinement, we can only state that such an idea is discountenanced by every physiological as well as common-sense view of the case; and it is too evident that our kind-hearted chaplain has been cruelly deceived by cunning men, whose remorse has mainly consisted in the development of a remarkably improved appetite, which they have but ill concealed under a show of penitence.

The exhibition of *coarse food* to prisoners, suggested by Mr. Field in order to prevent gluttony, will too surely and obviously strike the minds of our medical readers as the best means of creating dysenteric and other intestinal affections, to need remark here.

Our author speaks of the great evil which has resulted from feeding boys upon the Government Dietary.—He calls it *feeding in excess.* This is a great pity, if true, but not nearly so horrible as the truth itself happens until lately to have been; for we can declare positively that in an institution in which the diet was considered ample, young boys, who have resided there from 2½ to 3½ years, instead of growing, have absolutely lost in weight, and that, too, to the amount of some 4 or 5 lbs. each, in 17 per cent. of their number.

These boys were from 7 to 10 years of age on admission. This state of things was lately remedied by the humane exertions of Sir James Graham, to whom the public are also indebted for a most admirable *economical measure*, in the institution of a *more liberal* scale of diets for the prisons of England generally. The money which will be saved by improving the health of the criminal population, will far exceed the immediate expenditure consequent on improved diet. We cannot leave this part of our subject without remarking upon what has probably been the great source of all these evils, and the probable parent of the prevailing opinion daily gaining strength, that the human body requires ventilation and drainage rather than food.

Medical men have been too much biassed by the opinions of their masters. They have feared to place prison boards in a difficulty, by strong representations of evils, which could only be remedied by measures entailing great expense and infinite trouble on magistrates.

But why should this deter? Why should not the medical attendant state his *purely medical* opinion, and do his best to prevent the urgency of the times from affecting the bearings of a great physiological problem, which, once worked out, as it admits of being in such a case, must stand as an immutable truth, unassailable by the quotations of the corn market, and pregnant with lasting benefits to the human race? The question of expediency and policy is for others; and had the subject been so viewed by our prison officers, the government and magistracy might have had occasion long before this to hold in high esteem and regard a branch of prison service, which now by no means commands the respect it deserves.

The important subject of health as regards comparison between the sanitary condition of prisoners submitted to separate and associated confinement, is not entered upon at any length by the author; and we commend him for quoting the opinions of the medical officers of various establishments, rather than hazarding his own opinions, or drawing even what might appear to him the most obvious conclusions from such facts as have come under his own immediate observation. This careful treatment of the subject, though it is but what we have a right to expect from an educated man and a gentleman, has still not always been met with in connexion with this matter; and we have now before us the reports of an important establishment which shall be nameless, showing the workings of a meddlesome spirit, and an illiterate and illogical mind; which we are sure will not be read by Mr. Field without a lively sense of honest indignation.

There appears every probability, from what we have before us in the way of evidence, that separate imprisonment is far *safer to health* than any other plan of discipline. The facts adduced by the medical officers of the prisons of America and France, as well as those of England, are of a most striking character. From America we find the following evidence from a committee appointed by the State to visit the Eastern Penitentiary.

"A comparison of the bills of mortality of the Eastern Penitentiary with those of several other prisons in the United States, shows conclusively that the unbroken solitude of the Pennsylvania discipline does not injuriously affect the health of the convicts. At the Eastern Penitentiary the deaths are 2·5 per cent., at the Sing Sing prison 4 per cent., at Auburn 2 per cent., and so on; settling the question

beyond the possibility of doubt, that as great a measure of health is preserved in the Pennsylvania prisons as in other prisons elsewhere."

The physician to the Eastern Penitentiary reports as follows:

"The peculiar mode of confinement, so far from being injurious to the health of the convicts, is generally beneficial, and forms a decided improvement in this particular over the modes of incarceration pursued in other prisons."

Dr. Coates, of Philadelphia, in a paper read a short time ago, before the American Philosophical Society, observes that "the average mortality of the white convicts in the Penitentiary was less than that of the white inhabitants of the city and liberties of Philadelphia."

MM. de Beaumont and De Tocqueville, comparing their associated French prisons with separate prisons generally, say: "With us one prisoner dies out of fourteen in the Maisons Centrales. In the penitentiaries of America there dies on an average one out of forty-nine."

Dr. Combe, who wrote against the separate system before he was personally acquainted with it, subsequently wrote as follows: "We visited a number of the male convicts who had been confined for periods varying from seventeen months to eight years; and their appearance did not indicate either bad health or mental depression."

Let us now look to the Reports of the Commissioners for Pentonville Prison, in which establishment the separate system has been carried on for upwards of five years; and we shall find that the mortality observed bears comparison with that of persons chosen as healthy among the general population, viz. among the household cavalry. The mortality among those troops has been from 13 to 15 per 1000; that of the prisoners at Pentonville 15·7 per 1000. It must be observed here that the prisoners are selected as healthy as possible for Pentonville; but coming as they do from an unhealthy population, it was hardly to be expected we should observe so close an approach to the health of selected troops, as that shown by the returns.

The general unhealthiness of the criminal population may in all probability be traced to two causes: 1stly, the constant exposure to general causes of disease while they are free; and, 2dly, the bad diet and exposure in prison. The thief at liberty, especially in London, lives on the fat of the land. His passions are indulged to the fullest of their bent. He feels the control neither of physical nor moral influences. The flesh and the spirit alike are free, and conscience drops the rein. Debauchery in every form exercises its pernicious influence on the frame. The heated and contaminated atmosphere of the gin-shop or the brothel instils its burning or enfeebling poison; and irregularities of diet, in connexion with a weakened power of the assimilating organs, sow the seeds of those scrofulous and tubercular forms of disease, now universally acknowledged as prevailing among our prison population. In such a condition, how heavily must confinement weigh upon the man, both morally and physically! The intellectual occupies, alas! too small a part of his nature to require much consideration; but wherever it can be developed, there must we look for the antagonist power to support and preserve.

Mental depression, that fertile source of disease, must early affect the imprisoned libertine. He who was the most free is now watched closely

as a child; his every act canvassed; his every look interpreted; while his demeanour is to be changed from the pertest daring to the most respectful submission; and all this under the fear of the black-hole or the lash.

So much for the mental condition as productive of disease. Now let us look to physical conditions. Bad diet and the tread-wheel have more to answer for, than those who watch their immediate application need be aware of. It is not immediately, but more remotely, that the evils so inflicted show themselves in the frame. Pulmonary phthisis is not always developed or produced by three or six months' torture; and hard labour under insufficient diet produces its pernicious influence on the organs connected with circulation, rather when the muscles are gaining their ordinary state, than during periods of heavy exertion.

We cannot be surprised, considering the sad combination of mental and physical evils which has until lately been presented by our prisons and houses of correction, that we have a criminal population prone to tubercular scrofula in every form; and it is gratifying to observe the Pentonville prison maintain most excellent health, under disadvantages which must have produced great mischief in any less favorably circumstanced institution for the detention of convicts during a period of eighteen months. The examination which is carefully made at Pentonville by Dr. Rees, though it may have the effect of enabling him to reject such cases as may show either general or auscultatory signs of phthisis, yet must of necessity fail to exclude from the prison many cases of incipient disease; and the more prone to phthisis the class under examination may be, the more frequently must such cases of incipient disease gain admission.

We could have had no right, therefore, to expect such good health at Pentonville, as among troops made up of men who are selected from a healthy class of society. The comparison quoted above speaks volumes both for the separate system and the arrangements of Pentonville prison.

The precautions which have been taken at Pentonville in connexion with the subject of phthisis, to which we are about to allude, will, we believe, do much to reduce the number of cases of consumption among prisoners. An exciting cause has probably been removed, and incipient cases will now, it is to be hoped, go through imprisonment without showing symptoms of the disease.

Our medical readers would do well to consult an able article which appeared some months ago in the 'Quarterly Review,' entitled "Pentonville Prisoners," from which Mr. Field makes the following extract:

"Striking has been the diminution of consumptive cases, from attention to suspicions as to their origin. From the opening of the prison to the termination of 1844, the annual mortality per 1000 from phthisis had amounted to 11·47.

"The physician, Dr. Owen Rees, suspected that the dusty trades carried on in the cells might have added to the chances of death by this disease. In 1845, measures were taken to guard against the supposed cause; in 1846, only four cases per 1000 of consumption occurred; and in 1847 (up to the 20th of October), there has not been a single death from this terrible scourge."

Since Dr. Rees's observations, the surgeon of Perth County Prison has drawn attention to this same cause for the production of pulmonary disease; and we have every reason to believe that separate imprisonment will now be even less noxious than it has been to health, inasmuch as the greater part of the deaths have been from phthisis, and the custom of

working in cells probably renders dusty trades more dangerous in separate than in associated prisons. As regards the physical effects of the separate system observed at Reading, we will now quote the work before us:

"But beyond all evidence hitherto adduced as to the general healthfulness of separate imprisonment, that afforded by the records of the medical officers of Reading Gaol, as showing a contrast between former systems of punishment and that recently adopted, is most satisfactory.

"The following Table represents all the cases of criminals under medical treatment during three successive years of the associated and separate system respectively:

	Prisoners Associated.				Prisoners in Sep. Con.			
	1840.	1841.	1842.	Average 3 years.	1845.	1846.	1847.	Average 3 years.
Number of prisoners	720	633	665	672	683	664	877	741
Cases of slight indisposition	372	337	345	351	128	159	101	129
Infirmary cases	85	86	70	80	8	8	13	10
Total	457	423	415	431	136	167	114	139
Cases of death	12	4	5	7	1	1	1	1

This is certainly most satisfactory. It may be remarked, however, that the years 1840-1-2 quoted for the associated system, were not nearly such healthy years as 1845-6-7 quoted for the separate system. This fact, however, does not disturb the conclusion; the advantage on the side of separate confinement being far too great to be materially affected by such differences.

On the whole, it now appears certain that separate confinement, so far as physical conditions are concerned, must be looked upon as by far the safest mode of imprisonment yet devised for the punishment of criminals.

Let us now turn to the evidence before us respecting the mental effects of separation; and we cannot fail to be struck with the extraordinary contradiction, which close attention to the subject has enabled prison officers to give to the very generally prevailing opinion, that separation is productive of mental disease. The following is the report of a committee appointed in 1837 to inquire into the discipline of the Eastern Penitentiary of Philadelphia.

"An objection sometimes urged against the Pennsylvanian system of discipline, is the supposed tendency of uninterrupted solitary seclusion to 'dethrone reason and make wreck of the immortal mind.' In this case, too, the Committee had recourse to indisputable facts, and the veracity of recent evidence. A comparison of the registers of several penitentiaries of the United States will demonstrate the position that the Pennsylvanian prison exhibits as few (if not fewer) cases of mental derangement as any similar institution; indeed, no instance of insanity has yet occurred in the Eastern Penitentiary, which has not been traced to causes wholly independent of or either anterior or posterior to confinement."

In 1844, the Inspectors of the same prison, state—

"The hazard of stultifying the mind has been regarded as a possible concomitant

of separate confinement with labour; the inspectors desire to record their conviction in regard thereto, that no case has occurred within their knowledge where such effects have ever been produced."

Several other extracts might be made from the work before us, reporting evidence highly favorable to the separate system. Passages are also extracted by the author from the reports of the officers of American prisons on the *silent* system, viz. the Auburn and Sing Sing Prisons; which contain evidence that insanity is not unfrequently observed in those institutions. To come nearer home, we find Mr. Perry, an inspector of prisons, reporting most favorably of separate confinement. He states—

"The places of confinement in the Southern and Western districts are eighty in number; of which seven are conducted on the separate system. In the year from 29th September, 1844, to 26th September, 1845, the daily average of prisoners in the whole eighty places was 4361; in the seven on the separate system, it was 644. Thirty-seven prisoners were affected with insanity, in nine of whom the symptoms first showed themselves during the period of their imprisonment; *but of these nine not one occurred in the seven prisons on the separate system.*

Dr. Owen Rees, in a Report to the Commissioners of the Pentonville Prison, makes the following remark, which bears strongly on this important subject; inasmuch as it refers to the *general mental condition* of the prisoners under his observation:

"There is a general improvement in manner and address, indicative of intellectual advancement, very strikingly shown among the prisoners who have been longest in confinement; and an increased alacrity and desire to excel in work is, in most cases, the early result of this system of confinement."

The recorded experience of Pentonville, as regards the subject of Mind, is ably commented upon in the 'Quarterly Review,' in the article above mentioned. It is there shown that the amount of insanity observed among the Pentonville prisoners has been 1·48 per 1000; while it is 1·00 among the Dragoons in England, 1·43 among troops in the Ionian Islands, 1·33 in Canada, and 1·41 at Gibraltar; so that it may be fairly said that, "the prisoner under separate confinement suffers about as much as the soldier on the choicest spots of the Mediterranean or in the bracing climate of Canada."

The above return evidently refers exclusively to cases of severe mental disease, requiring removal to asylums. The Commissioners of Pentonville describe minor cases of mental affection as having occurred in their prison, and make the following remark:

"In many of the cases which occurred at Pentonville, the symptoms were no more than are frequently met with in private life, and were such as would probably have been overlooked, without that strict scrutiny to which the Pentonville prisoners have been subjected."

Such evidence as we have now quoted, is not only satisfactory as to the safety and advantages of the separate system, but it is most gratifying as the explanation of a fact for which we were at a loss to account, viz. the progress of the separate system in England, in spite of the efforts of most clamorous enemies. The friends of the system may be said to have remained silent; and even now that the good work is accomplished, they have merely adduced their facts, with only such conclusions appended as even the most fastidious will be puzzled to cavil at. This is a most en-

couraging instance, showing how the truth may possibly be obstructed, but never crushed; and that, however falsehood and error may obscure it, they act only as fertilizers to a seed which must inevitably produce a goodly and firmly-rooted tree.

We feel that, in merely noticing as we have done those parts of the work referring to the more medical bearings of this subject, we have done but scanty justice to the volumes of Mr. Field. It may be allowable, however, just shortly to state, as it cannot fail to be interesting to all, that the moral advancement of the separated prisoners under Mr. Field's care has been most striking, and that *the recommitments have been comparatively few.*

Again, the Reports received by the Commissioners of Pentonville Prison from those who had charge of the convicts subsequently transported, have placed it beyond a doubt, that the separate discipline has produced a new class of men for our naval colonies, and a class much desired by the free colonial residents. Mr. Field gives also a most interesting account of the prisons of France and Belgium, and other countries less favoured by the impress of humanity. We cannot, however, forget the sickening account of the Bastile as among the curiosities of prison literature, and as rivalling the horrors of Russian and Italian brutality.

It is worthy of remark, that the severity of this punishment by separation was much dreaded by the criminal population before the Pentonville Prison was opened; and the evidence which has since been afforded to that class by the experience of their fellows, who have been subjected to separation, has by no means lessened the terror which was first inspired by a theoretical view of the case. This has an important bearing on the question of diet; inasmuch as prisons conducted on this principle may be very safely endowed with sufficient and wholesome diet, without any fear of candidates presenting themselves for confinement.

We mention this point as important, because it bears on the prejudices of those who are powerful, and not for any intrinsic importance which we ourselves are inclined to attribute to it. The fact will assume a value, according to the degree of anxiety the reader may feel, lest our population should be tempted into crime by the superiority of our prison fare; a fear which, we are happy to find, may now be alleviated, even in the minds of the most sensitive, by the application of the separate system. Mr. Stephens, Superintendent of Birmingham Police, deposes as follows on this subject: "Q. Have you much confidence in the effect of punishment in deterring people of the criminal class from committing offences?—A. By the separate system I think there is no question of it. Q. They dread it very much?—A. They do."

Captain Groves, Governor of Millbank Prison, says: "I think they look upon the confinement in Pentonville as a very severe confinement."

Again, the Ordinary of Newgate deposes: "The question is put to every prisoner who is committed to Newgate, whether he chooses to be by himself or not, and we find that only one in one hundred chooses to be by himself."

We could not refrain from adverting to the question of diet in connexion with separation; because it appears to us that the relation which that system of discipline bears to the general question has not been so fully considered as it deserves.

In conclusion, we must congratulate our author on having laboured industriously and ably in a noble cause. We do not agree with him in everything, and more especially on the question of diet. In this respect, however, his opinions are countenanced, and we regret to say it, by many members of the medical profession; who, we believe, have fallen into error by having taken a low view, not only of the subject, but also of their own position as the high priests of Nature.

It can scarcely be doubted that the separate system has already done much; and, under the guidance of such persons as Mr. Field, we may venture to entertain a hope that it is to become one of the most powerful instruments, which it has pleased the Almighty to place in the hands of men, for the reformation of the fallen.

ART. VII.

1. *Lectures on Diseases of the Eye.* By JOHN MORGAN, F.L.S., late Surgeon of Guy's Hospital, and Lecturer on Surgery at that Institution. Second Edition, carefully revised, and enlarged with Notes, by JOHN F. FRANCE, Surgeon of the Eye Infirmary, and Lecturer on Ophthalmic Surgery, at Guy's Hospital. Eighteen Plates.—*London,* 1848. 1 vol. 8vo, pp. 222.
2. *On the Cure of Cataract, with a Practical Summary of the best Modes of Operating, (Continental and British.)* By HUGH NEILL, Surgeon to the Liverpool Eye and Ear Infirmary.—*London,* 1848. 8vo, pp. 224.
3. *Reports of the Liverpool Eye and Ear Infirmary, for the Years* 1843-7.

THE late Mr. Morgan's 'Lectures on Diseases of the Eye' having long been out of print, a new edition has been undertaken by Mr. France, Mr. Morgan's friend and former pupil, who has prefixed a short memoir of the author, and added a variety of notes to the text. The introduction to the Lectures had undergone the revision of the author, previously to his confiding the task of editorship to Mr. France; and various emendations had been made by him throughout the work. Although Mr. Morgan's Lectures, in general, only skim over the subject, and even that very imperfectly,—as witness the three pages on Artificial Pupil, and little more than three on Entropeon and Ectropeon,—and although many of his notions are antiquated and exploded, such as his belief in the occurrence of primary variolous pustules on the cornea, and in the existence of a staphyloma in which the cornea alone is opaque and protuberant—yet, on the whole, to his friends and pupils, this republication will afford a pleasing memorial of his attention to their interests, and his anxious desire for the extension of their studies in one of the most interesting and important departments of pathology.

We do not mean to review Mr. Morgan's Lectures in detail. They have long since passed the ordeal of criticism. We shall merely make a few remarks on Mr. France's notes, which on the whole are judicious.

In a note to the introduction (p. 10), Mr. France cautions the student against mistaking analogy for identity; a mistake which Mr. Morgan's anxiety to enforce the analogy of diseases of the eye with diseases of corresponding tissues in other parts of the body, he thinks, might beget.

The following part of this note contains some statements which would require modification:

"Owing to the anatomical dispositions of the mucous membrane of the eye, certain specific phenomena attend its affections; though these affections be essentially common to other mucous membranes: instance the effects of gonorrhœal inflammation. The sub-conjunctival cellular tissue fills abundantly with serous and firm fibrinous exudation; and slough of the cornea, resulting from its nutritious supply being thereby mechanically impeded, results; the eye being irretrievably spoiled: perfectly analogous tumefaction of the prepuce occasions phymosis, which may proceed to almost any extent, with temporary inconvenience only as the consequence. So, again, from the continual motion of surface over surface, the conjunctiva is sometimes under inflammation irritated to the production of a remarkable granular state, such as we see in no other portion, however villous, of the mucous tracts; simply because no other portion is under inflammation subjected to the same aggravating influence." (p. 11.)

True it is that phymosis, in general, is subdued with only "temporary inconvenience;" but this is not always the case, the tumefaction of the prepuce is sometimes so rapid and extensive as to cause gangrene, and we have known this speedily to terminate in the death of the patient. As to granular conjunctiva, Mr. France shows, in a subsequent note (p. 94), that he is quite aware that this sequela of purulent ophthalmia is by no means the result of the continual motion of surface over surface, and that it cannot be said with truth that a similar state does not occur elsewhere, "simply because no other portion is under inflammation subjected to the same aggravating influence." The granular state of the conjunctiva is rightly attributed by Mr. France, at page 94, to a peculiarity of structure possessed by this portion of the mucous system, a view of the matter quite the opposite of what is stated in the above extract. Granular conjunctiva is, in fact, a hypertrophy of the conjunctival papillæ; and unless other portions of the mucous system possessed a papillary structure, they could not, by any irritation to which they might be subjected, assume the granular state.

Under the head of catarrhal ophthalmia, Mr. France states, that this disease is "particularly apt to occur in a mild form in suckling women, especially if the subjects of leucorrhœa, or if lactation have been protracted beyond the natural term, or if from any other cause the constitution be unable to meet vigorously the call made upon it." (p. 25.) All this we perfectly agree with, and we quote the passage merely to remark, that it would have been well to have pointed out the fact, that women who have been long giving suck, are liable to a still more serious ophthalmia than the simple catarrhal,—one which implicates the internal structures of the eye, and among these the retina, as well as the conjunctiva and sclerotica, and which is by no means very amenable to treatment. This is the disease described by Mr. Middlemore under the indiscriminating name of "irritable ophthalmia."*

Mr. France demurs (p. 33) to the propriety of conceding, as Mr Morgan has done, a separate and distinct existence to aphthous inflammation of the conjunctiva; regarding aphthæ as an accidental complication only of scrofulous ophthalmia. We incline to believe, however, that the discrimination of aphthous conjunctivitis, formerly called pustular ophthalmia,

* Treatise on the Diseases of the Eye, vol. i, p. 297. London 1835.

is necessary, both in a nosological and in a therapeutical point of view. The disease is, as Mr. Morgan pointed out, closely analogous to aphthous inflammation of other divisions of mucous membrane, as of that of the fauces; the eruption is different from the phlyctenulæ of scrofulous ophthalmia; the distress attending it is much less than that which accompanies the latter disease; and the treatment is simpler and more successful. Instead of simplifying the subject, Mr. France's views would only lead us a step back towards the old confusion, which prevailed when the inflammation of every texture of the eye, and all the various kinds of inflammation of this organ, were huddled together under the name of ophthalmia, or lippitudo.

Mr. France's note on variolous ophthalmia (p. 42) is merely a deprecation of depletion in that disease. He enters no protest against Mr. Morgan's averment, that smallpox pustules form on the conjunctiva of the eyelids, of the eyeball, and even of the cornea; a doctrine which the observations of Dr. Gregory* and Mr. Marson† have altogether set aside. The ophthalmia which so often destroys sight after smallpox, is in fact not so much a variolous as a post-variolous disease; and the affections of the cornea which prove so obstinate and so dangerous, are not the results of pustules, but consist, in some cases, in a thickening and opacity of the corneal conjunctiva, and, in others, in abscess within the cornea. All this, we believe, to be generally known to the profession, and regret therefore to see the contrary statement, as made by Mr. Morgan, pass uncorrected by his editor.

Adopting the view originally promulgated, we believe, by Mr. Travers,‡ that the destruction of the cornea in purulent ophthalmia is owing to impeded nutrition, caused by the pressure of the greatly swollen conjunctiva and eyelids, Mr. France recommends strongly (p. 70) the radiated scarifications of the eye adopted by Mr. Tyrrell. Scarification is undoubtedly useful, but the direction of the incisions is, we think, of little consequence. Scarpa's *excision* of the conjunctiva, round and round the cornea, should have saved Mr. Tyrrell from the fear of killing the cornea by *incisions* concentric to that texture. In bad cases of purulent ophthalmia, when the eyelids are much swollen, and the conjunctiva chemosed, we can scarcely bring even the cornea into view, much less expose the conjunctiva bulbi so as to be able to incise it in radii from the cornea towards the eyelids. Mr. Tyrrell, in fact, admits almost as much in his reply§ to Mr. Wharton Jones, when he says—

"I have not in any case been able to make the number of incisions that I wished, and when I have been able to make any in the directions between the superior rectus, and the external and internal recti, they have been very limited. When the ocular chemosis is complete, the tumefaction of the palpebræ is always so great, that the surface of the globe can be exposed but very imperfectly."

The fact is, that the cases in which it is possible to incise the conjunctiva in radii from the cornea towards the eyelids, are not bad cases, and would in all probability get better without this sort of operation.

The only remark which occurs to us in regard to Mr. France's note on

* London Medical Gazette, vol. v, p. 222. London, 1830.
† Ib. vol. xxiv, p. 204. London 1839.
‡ Synopsis of the Diseases of the Eye, p. 91. London, 1820.
§ London Medical Gazette, vol. xxiii, p. 706. London, 1839.

scrofulous ophthalmia, p. 88, respects the use of anæsthetic agents in this very common and troublesome disease. He speaks of them only as facilitating the inspection of the eye; whereas a much more valuable effect, which has been found to follow their employment, is the sudden, complete, and permanent removal of the intolerance of light, which forms so distressing an attendant on this, as well as on some other ophthalmiæ.*

In the following parts of the note on abscess of the cornea, p. 116, are several statements to which we cannot assent.

"Two circumstances with respect to this subject, not alluded to above, are worthy of notice. The first is, that whenever a collection of matter forms between the layers of the cornea, there is always produced an ulcer upon the corresponding portion of the surface; the second, that notwithstanding this overture from without, the abscess, in apparent opposition to analogy, habitually discharges itself internally. . . .

"Operative interference should always be refrained from, unless the case imperatively demands it; but progressive augmentation of the morbid effusion, unchecked by treatment, so as to reach the level of the pupil, with continual maintenance of inflammation of the part, and of severe hemicranial and local pain, unrelieved by cupping, anodynes, and other measures enjoined in the text, indicates the necessity of puncturing the cornea. When this is urgently called for, the operation should be performed with the point of a cataract knife; and a low situation in the cornea should be selected, to facilitate the discharge of the morbid effusion, to diminish the danger of wounding the iris (which is, of course, lessened by the introduction of the instrument nearly in the same plane with, instead of at right angles to, that membrane), and to prevent a subsequent cicatrix in the axis of vision. After evacuation of the anterior chamber in this way, belladonna must be applied for the same purpose as after perforation from ulcer."

Various objections occur to us respecting this note. "Overture" is not used in the English language to signify an opening in the body, whether natural or morbid. Mr. France confounds onyx or abscess in the cornea with hypopium or abscess of the anterior chamber. But the points most material to be noted are—1st. That a collection of matter frequently forms between the layers of the cornea, without any ulcer being produced upon the corresponding portion of the surface. We every day see the pus of an onyx absorbed, without the cornea becoming ulcerated. 2dly. That when the lamellæ of the cornea, exterior to an onyx, ulcerate, as they often do, the pus is discharged externally by the ulcerated aperture. The discharge of an onyx internally, into the anterior chamber, if it ever happens, must be rare indeed; and Mr. France's assertion to the contrary, is explicable only on the supposition of his mistaking the coexisting diseases of onyx and hypopium (no unfrequent occurrence), for the discharge of an onyx into the anterior chamber. 3dly. Puncturing the cornea, for the purpose of discharging the pus of an onyx or a hypopium, is bad practice. The pus in onyx infiltrates the substance of the cornea, and does not escape, as if from the cavity of an abscess, when a puncture is made; and even the pus of hypopium is so thick and glutinous, that it does not flow from the anterior chamber, when this cavity is opened, but remains adherent to the surfaces of the cornea and iris which have secreted it, exactly as a piece of purolymph sticks to inflamed peritoneum. Puncturing in the manner recommended by Mr. France uniformly does mischief, exasperates the inflamed state of the parts, and leads to partial

* London Medical Gazette, vol. xxxix, p. 1077. London, 1847.

or total staphyloma of the cornea and iris. Puncturing the opposite edge of the cornea, where that texture is healthy, so as to allow the aqueous humour to escape, and to take off tension from the eye, is often useful in the cases in question. If the surgeon is bent on evacuating the pus of an hypopium, let him puncture the opposite edge of the cornea to the extent of two lines in length, and draw out the tenacious matter with a hook. In any other way, he will be foiled. 4thly. It is not after evacuation of the anterior chamber merely, but from the very first, and through the whole progress of a case of onyx or hypopium, that the pupil should be kept as much as possible under the influence of belladonna.

Mr. France in his note on Fungus hæmatodes of the eye, very properly directs attention to the fact, that there are various cases of non-malignant depositions, deep in the eye, which it is difficult to distinguish from the malignant. He mentions, besides, on the authority of a paper, in the twenty-ninth volume of the 'Medico-Chirurgical Transactions,' by Dr. Cumming, that a cat's-eye-like reflection is perceptible from the bottom of the healthy eye, when examined in a particular manner.

"The subject of experiment should be seated in a darkened room, at the distance of ten or twelve feet from a lamp, isolated rays from which are allowed to stream horizontally upon his face; the observer, placing his own head close to the lamp, but shielded from its light, will, when a neighbouring object is looked at by the other person, perceive his pupils gleaming like those of a feline animal. The essential conditions for the success of the experiment are, the confinement of illumination to the subject of it, the incidence of direct rays from the lamp upon the retina and choroidal pigment, and such position of the observer as may permit all rays reflected again therefrom to reach his own eye unintercepted by the irides of of the person examined." (p. 197.)

On the subject of this experiment we shall not speak very positively. We know that Sir David Brewster has mentioned a bright red reflection, as being observed, in one instance, from the bottom of the eye. Reasoning *à priori*, we should certainly conclude, that in the natural condition of the human choroid, covered as it is with pigment of a pretty dark tobacco colour, there can be little or no reflection of the light which has traversed the humours of the eye. In the attempts we have hitherto made to repeat Dr. Cumming's experiment, the light which we saw reflected from the eye, seemed to us to come entirely from the cornea and the surfaces of the crystalline, producing something of a blaze, when viewed at the distance prescribed, and to which the aid of imagination might assign a deeper seat, and a resemblance to the reflection from the lucid tapetum of some of the lower animals.

Mr. France assigns, page 184, the suggestion of the catoptrical test to Professor Sanson, whereas it belongs to Purkinje, who published it in his work on the 'Physiological Examination of the Organ of Vision,' in 1823.

Glaucoma, Mr. France states, page 185, to leave the crystalline pellucid, except in its most advanced degree, but to impair the transparency of the vitreous humour; whereas, glaucoma, from the first, affects the crystalline, and generally leaves the vitreous humour perfectly pellucid.

We have noted a number of other passages in Mr. France's notes deserving of remark, but we refrain from mentioning them, lest we should tire our readers with too many minute animadversions. Of Mr. France's labours we have already pronounced a favorable general opinion. As a

practical oculist, they show him to be carefully observant. As a writer, we would earnestly press upon him the cultivation of a purer English style, and the employment of common words and phrases in their ordinary signification. Such expressions as "the tolerance of general depletion by the public constitution," are bad, because they are obscure, or actually destitute of meaning.

The second work in the above list does not consist, as the unwary reader might conclude from its title-page, of a treatise on the cure of cataract, by Mr. Hugh Neill; but is made up chiefly of three long extracts from the following French works, viz. Stœber's '*Manuel Pratique d'Ophthalmologie;*' Magne's '*Hygiène de la Vue;*' and Desmarres' '*Traité Théorique et Pratique des Maladies des Yeux;*' to which a fourth extract is appended from Waldie's pamphlet on Chloroform. The whole of these four extracts are prefaced, and interlarded with notes, by Mr. Neill. The translations from the French are a sort of mongrel production, being, we are told, "a digest of the *thoughts* instead of a literal and servile 'translation' of the mere *words;* together with an analytical REVIEW." (Pref., p. ii.) Mr. Neill's ignorance of the French language, of which we shall offer some amusing proofs, sufficiently excuses him from attempting anything like an accurate translation of the three Frenchmen, whom he has chosen to mangle. The "analytical REVIEW" has unfortunately escaped our observation in the perusal of Mr. Neill's pages.

Stœber's *Manuel,* which is the first work, a portion of which (namely, that on cataract) Mr. Neill has taken the liberty of "digesting" for our benefit, is a tolerable treatise on eye-diseases, published at Strasburg in 1834. The author appears, from an expression in his preface, to be a young man, of modest pretensions, and not much experienced in the treatment of eye-diseases. He does not announce himself as connected with any hospital for the treatment of those diseases, but merely as one "livré depuis quelques années à l'enseignement tant théorique que pratique des maladies des yeux; souvent consulté sur le mérite des différens traités d'ophthalmologie." Full of affected admiration, Mr. Neill converts Dr. Stœber into "a distinguished continental author," and ends with dubbing him "the old and worthy pioneer—Victor Stœber." Fourteen years, no doubt, have passed over Dr. Stœber's head since the publication of his *Manuel,* but he may not thank Mr. Neill, perhaps, for reminding him of the circumstance; and as for his being a pioneer, we can discover no meaning in this application of the term, unless it is a hint, that in the rear there must also be an awkward squad, for a place in which, Mr. Neill's attempts to translate from the French must afford him an ample claim, notwithstanding the clangour and applause of his Liverpool troop of drummers and trumpeters—of whom more anon.

Stœber's subsection on cataract contains nothing at all remarkable; it is meagre and commonplace—so much so, that no one acquainted with any even of the commonest English works on eye-diseases, would ever think of referring to it a second time. Mr. Neill's translation of it, as well as of the extracts from Magne and Desmarres, is totally useless; as we never know when the original speaks, when Neill. In illustration of this remark, we shall quote part of a paragraph from Stœber, with the translation, and a note by Neill:

"L'opération est contre-indiquée, lorsque la cataracte n'est pas mûre. On fait bien encore de ne pas opérer aussi long-temps que le malade voit bien d'un œil: ce précepte cependant n'est pas admis par tous les médecins; il y en a qui opèrent l'œil cataracté, lors même l'autre remplit parfaitement ses fonctions; ils agissent ainsi parce que, disent-ils, on voit mieux avec deux yeux qu'avec un seul, et que la présence d'une cataracte dans un œil occasionne son développement dans l'autre. Mais la première de ces assertions n'est pas généralement vraie, car on voit très-souvent des individus qui, après avoir perdu un œil, n'ont pas la vue plus faible qu'auparavant. Quant à la sympathie qui lie les deux yeux, elle est incontestable, et le plus souvent la cataracte d'un œil est suivie de cette de l'autre; mais l'opération de l'œil cataracté, n'empêche pas ce développement consécutif de la maladie dans l'autre œil; et d'ailleurs il arrive encore assez fréquemment que des individus portent pendant de longues années, et même jusqu'à leur mort, une cataracte à un œil sans que l'autre s'en trouve affecté." (p. 322.)

"So long as Cataract is immature, the operation is contraindicated. Some contend that it is imprudent to operate so long as the patient can see well with one eye—a precept repudiated by others. There are those who operate upon the affected eye when the other perfectly fulfils its functions. They do so, because quaintly quoth they, 'One sees better, you know, with two eyes than with only one;' and because the presence of Cataract in one eye is speedily followed by Cataract in the other. It is a delusion, however, to suppose that operating upon the eye which already has Cataract, stops the consecutive development of the disease in the other.[a] Besides it happens, over and over again, that individuals carry a Cataract in one eye for many years, and even to the day of their death, without the fellow optic becoming affected. [b]" (p. 24.)

Without any interruption, Stœber goes on, as follows:—

"L'opération n'est pas seulement inutile dans ces cas, elle peut aussi avoir des inconvéniens plus ou moins graves; elle occasionne quelquefois l'inflammation et la désorganisation de l'œil opéré, et par conséquent une difformité qui n'existait pas auparavant. L'inflammation ne se borne même pas toujours à l'œil opéré; l'œil sain y participe quelquefois, et peut également être désorganisé, de sorte que le malade qui voyait bien avant l'opération, se trouve aveugle après." (p. 323.)

The looseness of Mr. Neill's translation is exemplified by his rendering "On fait bien encore de ne pas opérer," by "Some contend that it is imprudent to operate." His note [b] will show the liberty he takes with his author, when he "digests" him.

[b] "This is one of the really forcible reasons why it is that an operation for Cataract in certain cases—as in rash and incompetent hands—is not only useless, but pernicious. Beyond doubt, as Stœber elsewhere ably observes, it (an operation) may involve unpleasantness more or less grievous. An injudicious operation sometimes sets up inflammation, and destruction of the eye so tampered with; thereby bringing surgery into discredit, and producing a deformity which did not previously exist. Every surgeon, competent by observation to bear faithful witness, will avouch the declaration that inflammatory action, after an incompetent or maladroit operation, is not always confined to the eye operated on: the sounder organ may share the mischief and be equally destroyed. A patient who *could see* 'something,' before his eye has been poked by experimentalists, may find himself quite blind afterwards.—H. N."

By the initials "H. N.," Mr. Neill means to tell us, we presume, that

the note is his; but the reader who will compare it with the text of Stœber, will see to whom the substance of the note really belongs. The trash about "every surgeon, competent by observation," and the eye "poked by experimentalists," does not exist in Stœber's modest text; these are flourishes of Mr. Neill, who would artfully lead us to believe, that he has been at the pains of bringing part of this note from some distant portion of Stœber's book, although the passage is not "elsewhere," but part of the very paragraph to which this note is appended.

Fifty other places might be quoted, were it necessary, to show that Mr. Neill's translations are nowhere to be depended on, as conveying the meaning of the originals, and that we can never know, but by referring to Stœber, Magne, or Desmarres' works in French, what is theirs, and what Mr. Neill's.

The note [a], attached to the passage above quoted, is entirely Mr. Neill's, and is an example of the nonsense with which most of his original remarks abound:—

"[a] It is no delusion to say that the operation on one will sometimes save the other, for I have sometimes seen it do so. No case could better exemplify this than the case of Captain PATTERSON, of the Royal Marines. Cataract had formed in one eye, and the other was much affected. I was assisted in this operation by my friend Dr. THORNBURNE. Capt. PATTERSON is alive and well now, and the eye which was not operated on has quite regained its tone. The gallant officer says, the other is a regular telescope, which he keeps for 'long range.'—H. N." (p. 25.)

Capt. Patterson has cataract in one eye, probably the consequence of a blow or concussion. Mr. Neill removes it by operation, and vision is restored to it. Was the other eye cataractous or not? We believe not. Even Mr. Neill does not say it was cataractous. All that he says is, that it "was much affected," and that it "quite regained its tone." There is the very strongest reason to doubt, not only from Neill's cautious manner of expressing himself, but from the nature of the case, and the universal experience of surgeons in all ages, that a cataract was cured in the second eye, either spontaneously, or in consequence of the first eye being cured by operation. As to the operated eye being a telescope, this is only the repetition of a piece of fustian, which is to be found in a previous note:—

"Dr. Carson, F.R.S., upon whom I operated, (when upwards of 70,) had his eyes made equal to a telescope." (p. 20.)

The transcendent ability of Mr. Neill to indite nonsense and bombast, might be illustrated by quotations innumerable; but we must content ourselves with the following:—

"The vicissitudes of the season in a climate so remarkable for 'change' as ours is, form a standing topic of remark, as much to this hour as when the imperial conqueror, and equally remarkable scholar and commentator, JULIUS CÆSAR, took possession of France and partially invaded Great Britain. So graphically did he describe the climate of adjacent Gaul and British counties which he actually observed, that they remain on record as great facts for the compelled echo of present times. Climate has a close relation to temperament, and wonderfully modifies the effects of remedial measures, as well as the course of disease. But the whole face of operative surgery throughout the inhabited globe, may now be said to be revolutionized into similarity, by the discovery (proclaimed in 1847) of the use of

Æther and '*Chloroform*,' when properly inhaled in the form of vapour, by patients immediately before they are operated upon. Truly, the remarkable saying of one of the greatest of the poets of old Rome, never was more descriptive of the *transition-stage* in respect of the MORALS and PUBLIC OPINION of *his* age, than his phrase is happily expressive of the *transition-era* in the SURGERY and MEDICINE of the present day. Stœber, in the text, has alluded to the administration of ÆTHER; but it is by the gullet, or *old Roman road* of SWALLOWING, and thereby brought into action through the medium of a labouring stomach reacting in a tedious, uncertain, and most roundabout way, on the Brain, Respiratory Nerves, and Spinal System. As regards the new dispensation of Chloroform or Æther-vapour, administered by the lungs, every surgeon on earth has reason to exclaim with the telegraphic poet,

'Tempora mutantur, nos et mutamur in illis.'

...........In the concluding part of this treatise, I shall have more to say, on Æther, especially as I claim to myself some of the general credit as a pioneer to the discovery;—a discovery which, though not begun, was brought to a state short only of perfection in America, by a dentist of that country.—H. N." (p. 44.)

That in so sublime a burst of the ridiculous, there should be bits of obscurity, is not to be wondered at; such as, that "adjacent Gaul and British counties" "remain on record as great facts for the compelled echo of present times." Why the author of the line "Tempora mutantur," &c., is characterised by Mr. Neill as the telegraphic poet, we cannot conjecture; but as Mr. Neill speaks quite familiarly of him as "one of the greatest of the poets of old Rome," we should have been glad if he had at once told us his name. The fact is, that the line, though many centuries old, is not to be found in any of the Latin classics, but is, we believe, a mediæval monostich. As for Mr. Neill's claim about ether, we shall take it up before we have done.

We have already referred to Mr. Neill's ignorance of the French language, and shall now quote a few passages, in confirmation of this very serious objection to him as a translator.

In the following passages Mr. Neill makes Desmarres, in English, say just the opposite of what he says in French.

"On a reconnu depuis longtemps qu'il est inutile de recourir à un traitement préparatoire avant l'opération de la cataracte, lorsque cette maladie est simple," &c. (p. 555.)

"Les chirurgiens qui veulent laisser un intervalle entre ces deux opérations, ne manquent pas non plus d'arguments solides." (p. 554.)

"It has been acknowledged for some time, that recourse to 'preparatory treatment' is useless, in operating for Cataract, where it is neither simple," &c. (p. 117.)

"Those who wish an interval between the operations, equally fail in solid argument." (p. 115.)

Mr. Neill gives the following, as part of Stœber's directions, for preventing the rolling of the eye, during operations for cataract:—

"A less dangerous method of controlling undue movements of the eye, is to keep up slight but sufficient pressure *on the eye* by means of pledgets of lint, or of a compress of charpie by a bandage." (p. 31.)

We had never heard of this method before; and though we could not but grant that it might be very effectual, we had some doubts about the degree of interference with the manipulations of the surgeon, which pressing on the eye with pledgets might cause, or fixing by a bandage a compress of charpie into contact with the eye upon which the operation

of extraction, perhaps, was about to be performed. On turning to Stœber, we found the originality to be entirely Mr. Neill's, and to be occasioned by his inability to translate the word "autre." Stœber's remark is—

"Un moyen moins dangereux d'empêcher les mouvemens trop vifs de l'œil, consiste à exercer une légère compression sur l'autre œil, au moyen de quelques plumasseaux de charpie qu'on fixe par une bande." (p. 327.)

Among the characters of cataract, Desmarres gives the following:—

"Vision abolie complétement ou s'améliorant à un jour modéré." (p. 524.)

Mr. Neill's translation—

"*Vision* completely destroyed, or improving in a moderate time." (p. 107.)

Desmarres directs—

"On examinera avec soin, surtout s'il s'agit d'une opération par extraction, si un ou plusieurs cils ne tendent pas à se dévier." (p. 539.)

Mr. Neill translates this—

"Carefully examine, especially if extraction is intended, whether one or more of the eyelashes do not diverge." (p. 108.)

The following refer to the operation of extraction:—

"Affaissement du lambeau."	"Sinking of the wound."
"Soulèvement du lambeau."	"Swelling of the wound."
"L'humeur aqueuse rende au lambeau sa souplesse et sa longueur premières." (p. 640.)	"The aqueous humour may impart to the wound its original suppleness and length." (p. 146.)

"Lambeau" signifies "flap," not "wound." How the wound of the cornea could sink, or how it could be restored to its original suppleness, must *puzzle* the unfortunate readers of Mr. Neill's translation.

Desmarres having occasion to mention the name of Marc-Antoine Petit, (p. 553), Mr. Neill translates it into Mark Antony "the little." (p. 114.)

Desmarres quotes from Guillemeau, a sentence to the effect, that when the water, in which the ancients believed the disease to consist, came to thicken and *ripen*, being more firm, it was called cataract.

"Quand elle vient à s'espaissir et meurir estant plus ferme, est dicte cataracte." (p. 550.)	"When it begins to thicken and to die, being more firm, it is called *Cataract*." (p. 113.)

What idea Mr. Neill attached to a water *dying*, we cannot say.

Desmarres advises us to prescribe a purgative, the night before the operation for cataract, to empty the bowels. Mr. Neill thinks this a fit opportunity to favour us with a specimen of his wit and learning, at one blaze.

"Au reste, dans tous les cas, quelques jours de régime ne sauraient nuire, et l'on fera bien de prescrire la veille un purgatif, pour débarrasser les intestins." (p. 555.)	"In other respects, some days' 'regimen' will not hurt; and it will be well, according to Desmarres, to prescribe to 'old women' [of both sexes,] a purgative, to unload the intestines." (p. 117.)

This phrase "la veille" is a puzzler for poor Mr. Neill. Having by a violent metonymy converted "the evening before the operation" into a set of antiquated hermaphrodites, he finds, next time "la veille" makes its appearance, that this extremely clever prosopopeia will not answer, or, at any rate, will not bear repetition. So he winks hard, and takes no farther notice of his former acquaintances—the "old women [of both sexes.]"

Desmarres, that the patient may not be disturbed, by being obliged to rise from bed, after the operation, prescribes a purge the night before; but Mr. Neill, making nothing of "la veille," orders the dose, the evening of the operation.

"Lui prescrire un purgatif la veille du jour fixé pour opérer." (p. 640.)	"Prescribe a purgative the evening of the day fixed for the operation." (p. 146.)

Thus, by his ignorance of one of the most common French words, he converts the sensible direction of Desmarres into an advice which might be followed by the most disastrous effects.

The display of affectation and egotism, in those parts of this pamphlet which belong to Mr. Neill, is of the most nauseating description. We shall put down a few illustrations:—

"I have the vanity to consider myself a successful operator." (p. 21.) "I have one of the prettiest little Eye Hospitals in the world. I have now seventeen beds, and room for thirty............My bedsteads are iron, and I have hair mattresses." (p. 28.) "Very often I have saved my patient's eye by such a step, when inflammation has been going on, and no announcement of it made by pain—but having seen it, of course I checked its progress.—H. N." (p. 56.) "The 'sky-ey influences,' as Shakspere happily expresses a great fact, claim, in our day, especial consideration; and by the use of that apt phrase, I would be understood to mean METEOROLOGY in the broadest phase of its legitimate signification, namely, fluctuating states of the atmosphere in their physical, moral, aud electrical modes of diversified and subtle influence, on health and disease." (p. 67.)

Such phrases as "shying the instrument," "Contrary appearances forsooth indicate hardness!" "our honorable brother-pill, Dr. GONDRET," "saddled with the blame," "I cannot allow myself to buckle to, to an analytic review," "the dodging days among women," "to avoid these breakers a-head," "fumbling with the needle," "the operation is floored," "worse off, than an ordinary tailor proverbially is when without elbow room," "tapping the admiral," "crystalline chopped up, as if made mincemeat of," "shut up shop," show the vulgar taste of Mr. Neill.

We have no right, perhaps, to object to such phrases, when introduced into the notes; but when inserted into the translations, the originals being entirely free of any analogous expressions, they are impertinences committed against the very authors, whom the translator affects to laud and admire.

Is it to be wondered at, that a person who would stoop to write in the style of the above extracts, should also descend still lower—to quackery and hypocrisy?

The following is a sample of quackery:—

"Battley's Solution of Belladonna is a good preparation whereby to dilate the pupil. But the best for the purpose is a solution of the *Muriate of Atropina.* I am so particular in such matters of detail that I employ a formula of my own, prepared by a faithful chemist under special directions. It is colourless, free from smell, and a few drops of it effectually dilate the pupil.—H. N." (p. 27.)

Desmarres, speaking of the medical cure of cataract, makes the following statement:—

"Les *lunettes* comptent aussi leurs succès; elles s'adressent, de même que les *révulsifs*, non seulement à la cataracte, qu'elles guérissent *toujours*, mais encore aux taches de la cornée, aux pannus, à l'amaurose, etc., etc. Hâtons-nous de dire

que ceux qui préconisent les lunettes, en vendent le plus qu'ils peuvent (²), et qu'on peut sous ce rapport les assimiler aux marchands de pommades révulsives."

Mr. Neill's "digest" of the above is as follows:—

"Electricity," "revulsives;" hocussing "spectacles;" [a man rejoicing in the dashing name of Schlesinger had actually the audacity to "assure" a radical cure, by the use of spectacles alone, of the majority of "alterations of sight."](a)

Desmarres' note (²) is the following:—

Schlesinger, *Guérison radicale par le seul moyen des verres de lunettes de la plupart des altérations de la vue.*

He mentions neither the place where the book was printed, nor the date. The probability is, that, as is frequently the way with quacks, Schlesinger omitted these circumstances.

Mr. Neill's note (a) is the title, as given by Desmarres, with this addition:—

"I decline to advertise the name of the printer, the bookseller, or the place. —H. N."

Now it is extremely improbable that Mr. Neill ever saw Schlesinger's pamphlet; his whole knowledge of it, in that case, must have been drawn from Desmarres; as Desmarres mentions not where, nor when, nor by whom, the book was printed, neither could Mr. Neill; and his pretending to conceal, therefore, what he did not know—his assumed unwillingness "to advertise" a quack—is a piece of contemptible hypocrisy.

A considerable portion of the extract which Mr. Neill has taken from Magne's *Hygiène de la Vue*, is devoted to the catoptrical examination of the eye. Every tyro knows that it is useful, in suspected cases of cataract, to dilate the patient's pupil, and placing him in a room otherwise dark, to pass a lighted candle before his eye; that, if there is no cataract, three images of the candle appear, one behind the other; that of these images, the anterior and posterior are erect, the middle one inverted; that the anterior erect image is formed by reflection from the cornea, the posterior by reflection from the anterior capsule, and the middle or inverted image by reflection from the concave surface of the posterior capsule.

We must confess we were a little staggered, as to Mr. Neill's optical knowledge, near the beginning of his book, by his translating Stœber's phrase "lunettes à verres biconvexes," by "glasses of biconvex power;" but the following comment on the three images exhibits a degree of ignorance, as well as of self-conceit, deplorable indeed:—

"Before passing to other topics, I can here only remark why I prefer the word *images* or lights to '*reflections*.' It is because Magne too comprehensively so includes the *three*. Now, I question if all three are caused by *reflection*. If I am right, images would be strictly a less exceptionable word, because I am compelled to think only two of the lights are reflections, and the inverted image—the odd looking middle one—is owing—not to reflection, but *refraction*. As tested again and again by me on the model, and on healthy eyes, I repeat that the first shadow is upright, and on the cornea; the next is a dim shadow or image. Call it what you will, it takes a *middle* place, and is *inverted*. The third, and most backward, resembles a small clear flame, and is *erect*. So much for phenomena, which are matter of fact under actual observation. Their true theory, or explanation, may be left to whatever difference of opinion 'learned men' proverbially entertain on moot points. Part at least of such 'appearances' is explicable on similar

optical laws to the *mirage*, as described by scientific travellers from Egypt and the Highlands of Scotland; or to the *rationale* of that phantom at sea known as the "Flying Dutchman." (p. 170.)

We know not whether pupils are admitted to the Liverpool Eye and Ear Infirmary, over which our author presides; but if they be, what sort of instruction are they likely to reap from a man, so totally destitute of optical knowledge, as to suppose the inverted image seen in the eye, to be the result either of usual or unusual refraction!

As we have not the original of Magne, we must take it for granted that the following translation by Mr. Neill is correct. The author is speaking of the three operations for cataract,—division, depression, and extraction:—

"All three have a relative value. This is a truth which should be understood, instead of some saying, 'I will trust to Mr. SUCH-A-ONE because *he* employs such a method.' On this catch the charlatan does not fail to take advantage of PUBLIC CREDULITY, by vaunting the superiority of '*his* method' at the expense of others."

To the truth of these remarks we readily subscribed. When we came, however, to Desmarres' account of extraction (which he calls *kératotomie*, changed by Mr. Neill into *keratomy*), we began to suspect that our translator had forgotten the impropriety of vaunting the superiority of one method at the expense of others, for our eye could not overlook the following announcement of a peculiar mode of extraction, belonging to Mr. Neill:—

" I. Lower Keratomy.
" II. Oblique Keratomy.
" III. Upper, or superior Keratomy.
" IV. Other processes; *ex. gratia*, of ALEXANDER, of GUTHRIE, and of NEILL." (p. 134.)

Desmarres goes on to compare the advantages and the inconveniences of the lower, semi-lateral, and upper section of the cornea, and ends by giving the preference to the last. Then comes another note of preparation for, as we imagined, Mr. Neill's mode of extraction, in the following shape:—

"The processes of Alexander, of Guthrie, and of Neill.

"But there are other processes, for instance, of Guthrie and Alexander, and my own, described farther on." (p. 138.)

Farther on, and farther on still, we went, but nothing could we find of Mr. Neill's process of extraction. At length, at page 184, our eye caught the words, "The Author's process." Now, thought we to ourselves, we have it. But, lo! and behold, Mr. Neill's process turns out to be neither more nor less than reclination. Here is his account of it:—

"The operator, while he fixes—say the patient's *left* eye with his (the operator's) left hand—the process is done thus: and I am supposing that the speculum is not used. The second finger raises the upper eyelid and fixes it against the eyebrow. The first finger depresses the lower lid. The eye is thus firmly enough fixed: its outer portion is free to be punctured, and there is no impediment to a thorough inspection of the pupil. A *flat* straight needle, with two cutting edges running to a point, is carried, at the eighth of an inch from the cornea, towards the very centre of the eye through the sclerotic. The needle must not be poked into the lens. As soon as it is judged to be behind the *iris*, it should be gently forced to the

anterior of the lens, and posteriorly to the iris, *forwards* toward the pupil. Do all gently. Let there be no abrupt or sudden motion. The point of the needle seen in the pupil is now to be depressed, and *pressed down* the face of the lens to its lower edge, and carried back *below the lens* right into the vitreous humour. The needle is then moved horizontally backwards and forwards, so as to cut a free course in the vitreous body for the passage of the lens. All this is a *continuous*, momentary, and essential movement. Attempted otherwise, a laceration of the vitreous capsule is not easily accomplished; and should the membrane be tough, and not lacerated, the lens may be turned topsy-turvy, and tilted into the anterior chamber.

"The needle having made its way through the capsule of the vitreous humour, now comes back to the anterior of the lens. Commence pressure with it, to cause displacement, by resting heavily on the anterior portion of the lens. The lens now moves downwards. The needle passes down also, and the lens begins technically to *recline:* that is to say, it turns backwards.

"The needle ought to get on the (now) top of the lens; and away it goes, launched as it were into the vitreous humour. Its position is supine, offering its whole flat surface opposed to the vitreous humour. It cannot rise up: its flatness is also opposed to the retinal portion of the vitreous humour.

"It only remains for the parts to become adapted to their new position, and the case will be as brilliant as the most enthusiastic eye-surgeon could desire.

"Such is my favorite operation for the cure of Cataract by *RECLINATION*—not as hitherto described, confusedly, or with needless elaboration in books. In homely language, I will pit hundreds of cases of *Reclination* which I have successfully performed, against the very best cases of *Extraction;* and the eyes of my patients shall show as little injury surgically produced, *aye and less*, than the optics of those who have had the rare good luck to have been 'touched off' by the most dexterous 'Extractor.'"

Mr. Neill is more particular in cutting "a free course in the vitreous humour for the passage of the lens," than most other operators. Either his operation, however, or his account of it, is extremely imperfect, in so far as no notice is taken of the fate of the anterior and posterior hemispheres of the capsule. Their being left entire, or their being divided, seems to be entirely a matter of chance.

To return to the operation of extraction, Desmarres, as translated by Mr. Neill, gives us very little information about the processes of Alexander and Guthrie. It would be difficult, indeed, to describe Mr. Alexander's method of operating—that being a thing which, we understand, he keeps under lock and key. Dissatisfied with Desmarres' account of Mr. Guthrie's operation, Mr. Neill writes to this gentleman for some further information on the subject, and receives the following answer:—

"4, Berkeley Street, March 17, 1848.

"MY DEAR NEILL,—I have not seen Desmarres' book.

"I stand behind for the right eye, and in front for the left. I use Beer's knife for both. I will send you CHARLES's book, which describes accurately what I do.

"The incision should be made at once, if possible, and *not too near the edge* of the cornea—a good eighth of an inch. I always cut out, unless the eye is spasmodic, when I do sometimes leave a little bit uncut at top: but the cleaner the cut, the better will be the result. When the iris falls on the knife, *rubbing* and *pressing* will disentangle it.

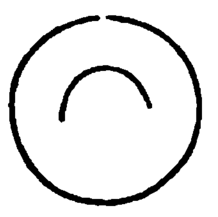
"Yours ever,
"G. J. GUTHRIE."

"H. Neill, Esq., &c.,
Liverpool."

(p. 181.)

Mr. Guthrie is, no doubt, a bit of a wag in his way, and probably thinks "Dear Neill" fair game. Of course, whatever "Dear Neill" might

attempt in the extraction-line, Mr. Guthrie never imagined that his letter should be published. "A good eighth of an inch!" Let us see what sort of section that would give us! Let the circle, in the annexed figure, represent the circumference of the cornea, which measures $\frac{9}{20}$ inch in diameter; then the semicircle is the section recommended by Mr. Guthrie to "Dear Neill," not "a good eighth," but exactly an eighth of an inch from the edge of the cornea. At page 186, Mr. Neill informs us—"In fairness I would have it borne in mind that *I do* EXTRACT." We do not doubt it, and shall be glad to learn, in his next publication, how he gets on, when he follows Dear Mr. Guthrie's directions, and makes the section "a good eighth of an inch" from the edge of the cornea.



Not satisfied with the ample fame which such a discovery must afford, as that the inverted image within the eye is nothing else than a small "Flying Dutchman," formed by refraction, Mr. Neill next gives us a touch of his quality as an etymologist:

"Though the Greek verb Καταράσσω means to rush, destroy, or abolish, &c., yet in nature there is little in common between the *disease of the eye*, figuratively designated 'Cataract,' and the *Cataracts* which give majesty and interest to the scenery or landscape of WATERFALLS, whether on the scale of NIAGARA, or of the *Cataract of the Ganges*, or the *Falls of Clyde*. There is no rushing or precipitate fall in the Cataract of surgery! To my mind, the only accompaniment of the Cataract in nature, and that which the *disease* so designated by Galen and the Arabians is also attended by, is the *misty vision*, and confusion of light corresponding to the *spray* which rises upon the headlong descent of volumes of fluid, varying in force from tiny waterfalls to sublime Cataracts, such as those I have incidentally named. Many of my friends have expressed their surprise how such a '*sounding* term' came from the Greeks—who as every scholar is aware were singularly happy in the characteristic appropriateness of their epithets. I could say much more on this theme: but I forbear." (p. 163.)

Where Mr. Neill picked up this notable piece of blarney, we cannot say. There can be no doubt, however, that the word καταῤῥάκτης was never used by the Greeks to signify a disease; and that the application of the Latin word *cataracta* to a disease of the eye, arose in the following way: Galen's name for the disease in question was ὑπόχυμα, or ὑπόχυσις ὑγροῦ, that is to say "a suffusion or flowing down of a humour;" the Arabians translated this by the words نزول الماء *Nuzúlu-l-má*; the Latino-barbarous translators rendered this literally by "aquæ descensus," or simply "aqua," from which, as a sort of punning substitute, or synonyme, arose "cataracta."*

* Du Cange refers us to the 'Acta Sanctorum' for the use of the word *cataracta*, as signifying a disease of the eye. This must have been early in the 14th century. It is evident, however, from the following memoranda, that the use of *cataracta* as a synonyme for *aquæ descensus* was but very partial even in the 16th century: In the Latin translation of Avicenna, printed at Venice in 1544, we have a chapter "De Aqua;" in that of Albucasis, printed at Basil, in 1541, we have a chapter "De Cura Aquæ descendentis in Oculum;" in the Practica Johannis Arculani, Venice, 1504, we have a chapter "De Aqua descendente in Oculo;" but in none of these do we find the word "cataracta." The Liber Constantini de Oculis, contained in the Omnia Opera Ysaac, printed in 1515, has a chapter "De Cataracta;" and in the Practica in Chirurgia of Joannes de Vigo, printed at Venice in 1504, (the first edition of that work, and very rare), we have a chapter "De Cura Cataractarum." We understand that one of the editions of Albucasis has a chapter "De Cura Aquæ, quæ descendit in oculo, vel Cataractæ;" but this edition we have not seen. The Juntine edition of Galen, 1556, has "cataracta;" the Frobenian edition, 1562, has "suffusio," the word used by Celsus as a translation of ὑπόχυσις. (vi, 6, § 35.)

Mr. Neill next tries his hand as a statist. He favours us with a table of diseases treated in the Liverpool Eye Infirmary, in 1847, which elicits the following remark:—

"On comparing the printed reports of other eye-institutions in various parts of Great Britain and Ireland, with those of the Liverpool Eye Infirmary, I cannot wonder at the surprise frequently expressed by visitors and benefactors, that the number and importance of the cases annually under treatment here, should surpass those of most other large towns." (p. 167.)

In the next page, we find as follows:—

"If asked by those interested in authenticated returns of particular forms of disease cured by operation:—if asked by statistical inquirers, how I make out my seven hundred cases of Cataract operation, in Liverpool:—I readily do so, by citing the number for each year—from 1834, down to and including 1847. Here is what a grand jury would return as 'a true bill.'"

Then follows a table of years and numbers, making the total of cataract operations 714, and signed "Hugh Neill."

It is very remarkable, on examining the number given in this table, as that of *cataract operations* in any one year, that it is exactly the same as that given in the Report of the Liverpool Eye and Ear Infirmary, of *cases of cataract*, for the same year. Thus, in the table of diseases in 1847, in the Report, the number of cases of cataract is 81, and this number 81, is the number of operations for cataract in Mr. Neill's table; and the same for the years 1846-5-4-3. Now, it is well known, that two circumstances concur to render such a striking coincidence altogether incredible. The one is, that at an Eye Infirmary many cataract patients apply, who are never operated on; and the other, that cataract generally affects both eyes, rendering more than one operation necessary to make a complete cure. Yet, if Mr. Neill's table is correct, the number of cataract cases occurring annually at the Liverpool Institution has for many years past been exactly the same with the number of cataract operations performed. Such loose statements are totally useless in a statistical point of view. To be of any value, the number of patients should be put down, with their age and sex; the number operated on, the number of eyes operated on, the number of operations performed on the same eye, the nature of the operations performed, and the result.

It appears that "visitors and benefactors" express surprise at the very great number of cases treated at the Liverpool Eye Infirmary. Mr. Neill says, he "cannot wonder" at that surprise; neither can we, when we analyse the tricks used to excite it—the monstrous exaggerations which are palmed on the public. Thus, in the Report for 1846, we find a mover of a resolution saying —

"I never attended the Annual Meetings of this useful Institution without witnessing mingled and varied feelings,—feelings, in the first place, of sorrow and sympathy for the large amount of suffering and distress which are here made known, and of which, but for their being thus brought before us, I apprehend few of us would suppose to exist to the extent they do—19,200 cases prescribed for and attended to in one year." (p. 11.)

There can be little doubt that this number, 19,200, was put into the hands of the gentleman in question—that he was primed to fire off this gasconade at the meeting—and to have his speech reported in the news-

papers, where thousands would read it without ever doubting for a moment, that Mr. Neill had prescribed for and attended to 19,200 cases of eye and ear disease in the year 1846! Not one out of a thousand would suspect the truth, that this was not the *number of cases*, but the *number of times* that patients had been prescribed for!!

Mr. Neill calls his table in page 109 "a true bill." We agree with him for once. It is a true "Bill;" and to make it still a truer bill, it requires only to be printed on an octavo side, surmounted by an appropriate device, signed "Hugh Neill," and distributed in the usual manner. But with what device shall it be adorned? The Royal Arms are stale. The Corporation Crest of Liverpool Mr. Neill affixes to his work "On the Cure of Cataract." We have before us a former "Bill" of Mr. Neill's, with a huge eye at top. Suppose a nose is added to the eye, and from the point of the nose a thumb and four outstretched fingers are made to radiate, we should think, to use the words of Mr. Neill, that the device was "singularly happy in characteristic appropriateness."

The concluding part of Mr. Neill's publication is dedicated to the use of chloroform and ether. He says, at page 197—

"I conclude, as promised, with a compendious article illustrative of the inhalation and use of 'Chloroform' in the operative department of leading cases of eye-surgery."

Now, as very little has been published regarding the use of anæsthetic agents in eye-surgery, we reckon it particularly kind in Mr. Neill to favour us with his experience on this point. The following is the whole that he says on the subject:—

"The public institution in Liverpool in which operations were first tried on patients in a state of *chloroformosis*, was the LIVERPOOL *Eye and Ear Infirmary*. I had great satisfaction in having Mr. Waldie to preside, while I operated in presence of a party of anxious and delighted spectators. *Chloroform* is now of established merit." (p. 208.)

We have not less than twenty-seven pages on Chloroform and Ether, but not one syllable more than the above, on the use of the inhalation of these substances in cases of eye-operation.

Mr. Neill puts in a claim, as having given the hint, which led the American dentist, Mr. Wells, Drs. Morton and Jackson, and others in Boston, to the employment of the vapour of ether. He says—

"As I have put in some claim as an humble pioneer towards its complete discovery—a discovery which unquestionably took place in America—I will state on what grounds I rest my title to be considered as having aided that consummation. Eight years ago, I published a work on the ear; and in 1841 the same work was published in New York, Philadelphia, and *Boston*. I gave a regular diagram of an apparatus for the administration of *Æther* by the *eustachian tubes* (air-passages from the throat) for the cure of some forms of deafness, and for the relief of *painful affections* of the inner ear by the induction of Æther-vapour. Well, from Boston, in 1846, we have a reflex importation, (matured certainly to a climax, entitling the party to the just credit of a complete and triumphant discovery)—to the effect that Æther-vapour will not only soothe the pain, but also produce complete insensibility to pain." (p. 197.)

The next paragraph is margined "American edition of Neill's views, eight years since;" but the claim is so utterly absurd and ridiculous, that we shall spare our readers any further extracts on the subject.

Itard, in 1821, proposed to introduce the vapour of ether into the tympanum by means of a catheter, and to generate the vapour by an apparatus in which acetous ether is dropped on a saucer of red-hot iron. Kramer, in 1837, or earlier, pointed out that this plan was bad, as the ether is decomposed by the red-hot iron, so that the apparatus does not introduce into the tympanum etherous vapour, but an acrid, irritating gas. He therefore used a large glass flask, into which he introduced the ether, allowing it to be there vaporized by the ordinary temperature of the room, and then guiding it through a tube into the tympanum. All this is described at large in Kramer's work, entitled 'The Nature and Treatment of the Diseases of the Ear,' translated by Bennett, p. 271. London, 1837. As we have never seen Mr. Neill's pamphlet on deafness, a thousand copies of which he shipped in the way of business to America in 1841, we cannot say whether his plan of sending the vapour of ether into the tympanum differed or did not differ from that of Kramer. The insinuations that Wells, Morton, and Jackson, having fallen in with the said pamphlet, took therefrom the idea of using the inhalation of the vapour of ether, without acknowledging the source whence they had derived it, are in the highest degree ludicrous, as well as base. Kramer and Itard might complain in the same tone, with more justice than Neill; as their application of the vapour of ether to the tympanum was earlier than his. If the Americans took the hint from any one on this side of the Atlantic, it was from Sir Humphry Davy, who suggested the inhalation of nitrous oxide gas, *as a means of destroying physical pain during surgical operations*;* and this was the gas Wells first employed.

In the mean time, we have no doubt the drummer and trumpeter troop are loud in proclaiming Mr. Neill as the discoverer of the use of ether and chloroform in preventing pain in surgical operations—or at least as having given the hint to the Americans; and probably at their next annual meeting, some one of them, perhaps the announcer of the 19,200 cases, will move the erection of a brazen statue to the indisputable author of the "great fact."

A long rigmarole follows of extracts from Waldie, Simpson, and Dr. Maginn, with references to Dr. Chalmers, Mr. Curling, Dr. Murphy, Homer, Shakspeare, Milton, &c., with which we need not trouble our readers.

It is, indeed, a painful task to wade through such a hash of ignorance, egotism, affectation, and impudence, as this whole work of Mr. Neill's presents; the object of which is evidently not to communicate information —not even to give a fair abridgment of the authors he has disfigured— but to play the fool, and to advertise and puff himself off. If, after the perusal of this trashy production, the reader should ask himself, what benefit he has derived, or if he has derived anything but disgust, from the portions of it furnished by Mr. Hugh Neill, to such interrogation, echo, we fear, would give only the melancholy response of Heu! Nil!

* Davy's Researches, Chemical and Philosophical, p. 556. London, 1800.

ART. VIII.

1. *On Scurvy.* By ROBERT CHRISTISON, M.D., V.P.R.S.E., President of the Royal College of Physicians of Edinburgh. (Edinburgh Monthly Journal of Medical Science, June and July, 1847.)
2. *Contributions to the Pathology and Treatment of the Scorbutus at present prevalent in various parts of Scotland.* By CHARLES RITCHIE, M.D., one of the Physicians to the Royal Infirmary of Glasgow. (Edinburgh Monthly Journal of Medical Science, July and August, 1847.)
3. *On Scurvy in Cumberland.* By HENRY LONSDALE, M.D., Physician to the Cumberland Infirmary. (Edinburgh Monthly Journal of Medical Science, August, 1847.)
4. *Mémoire sur le Scorbut observé à la Salpêtrière en* 1847, *et sur la Composition du Sang dans cette Maladie.* Par le Dr. A. FAUVEL, Médecin du Bureau central des Hôpitaux, &c. (Archives Générales de Médecine, vol. xiv, p. 261.)
5. *Report on Scorbutus as it appeared on board the United States Squadron, blockading the Ports in the Gulf of Mexico in the Summer of* 1846. By J. M. FOLTZ, M.D., Surgeon U.S. S. Raritan. (American Journal of the Medical Sciences, January, 1848, p. 38.)
6. *On the Recent Occurrence of Scurvy in Exeter.* By THOMAS SHAPTER, M.D. (Medical Gazette, vol. xxxix, 1847, p. 945.)
7. *On Scurvy.* By OLIVER CURRAN, M.D., Professor of Medicine, Apothecaries' Hall, Dublin. (Dublin Medical Journal, New Series, 1847, vol. iv, p. 83.)
8. *On the Nature, Cause, and Prevention of Scurvy.* By ALFRED B. GARROD, M.D., Assistant-Physician to University College Hospital. (Edinburgh Monthly Journal of Medical Science, Jan. 1848, p. 457.)

FODÉRÉ remarks, in his admirable article on Scorbutus, in the 'Dictionnaire des Sciences Médicales,' that although with the progress of Hygiène, scurvy is disappearing, yet that its causes too often return, and bring with them, as heretofore, their fatal properties of reproducing this disease in a greater or less intense degree. The truth of this remark has been abundantly confirmed by the experience of the last two years. The extraordinary events which, commencing with the destruction of an almost universal article of food, have now reached their climax in the social distress and the consequent political earthquakes which prevail over nearly the whole of Europe, may well tax the powers of some future Polybius or Alison to develope their mutual relation and dependence, and to trace out the important results which must inevitably flow from them. Among the minor of these results, we must place the reappearance among the population of these kingdoms, and, in a less degree, among those of France and Germany, of a disease which was previously almost traditional; a disease whose existence was to most of our readers a matter of testimony, not of actual observation; and the history of the ravages and devastations of which, in former times, was glanced over by the physician with hardly an expectation that they could ever again become subjects of familiar comment.

The discussion on this renewed outbreak of scurvy has for us a peculiar interest. We have witnessed this disease, so much debated by the great writers of the 17th and 18th centuries, suddenly submitted to the scrutiny of the more finished science of the present day. For the first time since the study of organic chemistry has been sufficiently matured to allow us to investigate the penetralia of the system with some prospect of success, cases of scurvy have become sufficiently numerous to furnish a proper extent of observation. It is not only a matter of curiosity, it is a subject of the greatest importance, to determine of what the powers of the chemist are really capable, and how far our researches have been able to unravel the phenomena which proved so puzzling to our forefathers. For this investigation into the success or failure of modern chemistry, and this comparison between our speculations, and the hypotheses of Wierus, of Hoffmann, or of Willis, may serve as some sort of index of the benefit we shall derive from chemistry in other diseases, and as a test of what the present amount of science will in our time be able to effect. And we believe we are justified in affirming, that while in the description of obvious and external phenomena, and in the determination of remedial and therapeutic means, little or nothing has been added to the elaborate descriptions of the writers of the last century,—in the analysis of the more recondite and deep-seated processes of which these phenomena are signs, we have made a real and tangible step towards the discovery of the true pathology of scurvy.

We propose in the following sketch to select here and there some prominent points, the discussion on which will include the most interesting portions of the papers before us. We shall not linger long on the bare detail of the easily appreciable and coarser characters; indeed, on this head little can ever be added to the admirable delineations of Rouppe, Lind, or Trotter. The picture which these writers have drawn in terms so strong and vivid, and the description they have so emphatically penned, necessarily could not be equalled by those who have witnessed cases comparatively few in number and mild in character. It is, however, but justice to the present writers to say, that although the diminished severity of their cases has not permitted them to depict, in colours of such variety and intensity, the different characters of this disease, still they have not less truly, and with not less skillful hands, traced its prominent and diagnostic marks.

The first point which has lately been most generally and most warmly discussed, is the nature of the cause or causes which render scurvy prevalent; and this point, on account of our greatly advanced knowledge of the laws of diet, might reasonably have been expected to receive a final settlement. Whether this expectation has been realized, or not, will presently appear.

The early writers on scurvy, particularly Wierus and Eekthius, appear to have placed the cause almost entirely in errors of diet. Subsequently, after a long discussion, in which the prevalent theories of the day often curiously blended themselves, and in which, cold,* humidity, atmospheric impurity, the use of salt meat, the effect of the sea air, and many other presumed causes, were warmly advocated, the general opinion seemed to settle itself into the belief, that scurvy might proceed from numerous

* Cullen's definition begins, "In regione frigida."

causes, which, either separately, or in a much more intense degree, conjointly, were adequate to its production. And this appears to be at the present time the creed of the French school, if we may judge from an account of a debate which took place on the occasion of a note on an attack of scurvy at Givet being presented to the Académie de Médecine, by M. Scoutetten. In a somewhat confused discussion on the causes, which followed the reading of this note, we find the influence of all the causes above given, and of many others, as of the causes of "fièvre typhoide," warmly insisted on;* as if, writes the reporter of the Séance "toutes ces causes ne se réunissaient pas presque toujours pour déterminer la maladie." Rochoux and Fodéré, also, in their respective articles in the 'Dictionnaire de Médecine,' and 'Dictionnaire des Sciences Médicales,' adopt similar opinions.

In spite of this general accordance, there have not been wanting writers who advocated an opinion very different, and who have adopted more or less fully the opinion of the early observers, that scurvy was caused solely by errors in diet. This was the opinion of Bachstrom, who published in 1734, an excellent essay on scurvy, and who concluded that abstinence from fresh vegetables is absolutely and solely the cause of the disease. Subsequently Rouppe† adopted the same view, and illustrated it by a variety of examples, which have been quoted by all succeeding writers. At a later date, Trotter,‡ in an exceedingly able Treatise, declared that this malady was particularly owing to the scarcity of recent vegetable matter, and that where this abounded scurvy was unknown. Among recent writers, Dr. Kerr§ has insisted upon the privation of fresh vegetables being the "most constant peculiarity of diet." And still more lately, Dr. Budd, in his admirable article on scurvy, in the 'Library of Medicine,' adopts more unreservedly the same opinion:

"Scurvy," he says, "may occur in all climates, either on land or at sea; in persons who subsist on salt meat, or fresh, and in situations in which the utmost attention is paid to cleanliness and ventilation. There is one condition, however, which is necessary for the production of scurvy, namely, prolonged abstinence from succulent vegetables or fruits, or their preserved juices as an article of food. When this condition is fulfilled, we find scurvy arising in persons whose situations are the most various in every other respect in which we can compare them, while not a single instance can be cited of its occurring in a person well supplied with these vegetables or fruit."‖

Let us now shortly examine into the support which the recent prevalence of scurvy gives to any or all of these presumed causes.

The cases from which Dr. Christison has drawn the materials for his interesting and able paper, occurred either in the prisoners in the Perth prison, which he inspected in company with Dr. Maclagan, or in patients admitted into the Royal Infirmary of Edinburgh, either from amongst the citizens, or from the railway labourers employed in the neighbourhood. Dr. Christison refers scurvy to dietetic causes, but he does not adopt the

* Archives Génerales for August, 1847, pp. 505-6.
† De Morbis Navigantium, Trans., p. 130 et seq.
‡ Observations on the Scurvy; being an Attempt to investigate that Principle in recent Vegetable Matter which alone has been found effectual in the Treatment of this singular Disease. Second Edition. By Thomas Trotter, M.D. London, 1792.
§ Cyclopædia of Practical Medicine, vol. iii, p. 683.
‖ Library of Medicine, vol. v, pp. 66-7.

opinion that the want of fresh vegetables forms the main feature of these; on the contrary, he believes that deficiency in the quantity of azotized aliment, and consequent insufficient nourishment of the body, are the real producers of scurvy. It appears, indeed, that both at Perth and Edinburgh, potatoes and other fresh vegetables had for some time ceased to be used by the persons who afterwards became affected with scurvy; but Dr. Christison is not inclined to attribute so much importance to this, as to the absence of milk, which had also ceased to be an article of diet. The deficiency in the azotized principles, consequent on the limited supply of milk, has been shown by him in tables drawn up evidently with great care and precision. To the consideration of this opinion we shall return in an afterpart of this article; suffice it to say now, that of two possible existent errors in diet which may have caused scurvy, viz. deficiency in fresh vegetables, and deficiency in azotized food, and particularly of milk, Dr. Christison chooses the latter, on grounds which we shall hereafter investigate.

Dr. Ritchie's observations were made on numerous cases admitted into the Glasgow Infirmary; the causes of the disease are said by him also to be dietetic errors, but somewhat different from those assigned by Dr. Christison:

> "The general fact, in regard to the food of all was, *that it failed in variety and in the quantity of its animal constituents*, and that in all but a fraction of the cases, in which they were very deficient, *the patients had been exposed for months to a total deprivation of fresh succulent vegetables.*" (Op. cit., p. 41.)

Afterwards he writes:

> "The errors in diet which have been stated, and, in particular, the want of proper vegetable food, were the true exciting causes; cold and other debilitating agents operated often as the predisposing causes of the disease."

Dr. Lonsdale's cases were witnessed in the north of England; and his remarks on the causes of scurvy are the more important, as he seems to have sought for evidence for or against the opinions of Dr. Christison. Scurvy occurred in two different classes of cases: in one, among the factory people, weavers, &c., the food was deficient in quantity and in variety; in the other, among the railway excavators, the food was abundant, consisting, in large quantities, of beef, mutton, salt bacon, suet puddings, bread and butter, oatmeal, treacle, tea, coffee, and occasional potations of beer. These men, strong, muscular, and iron-framed, often consumed, Dr. Lonsdale says, above a pound of beef at a meal. Milk, these men had not used for several years; but among both these classes of cases, among the ill-fed and the well-fed, there was one point in which they both agreed,—potatoes and all kinds of fresh succulent vegetables were altogether wanting. With regard more especially to milk, Dr. Lonsdale witnessed, with Mr. Carruthers, many cases of scurvy in cottars who were using 10 to 32 ozs. daily; and in this district, while scurvy so extensively prevailed, the ordinary supply of milk was not deficient; the potato crops, however, had entirely failed.

Dr. Graham, of Brampton, also, in noticing the absolute want of potatoes in his neighbourhood, mentions that many of his cases had taken more milk than usual in the season previous to the prevalence of scurvy. In Workington, a sea-port on the west coast of Cumberland, scurvy did

not appear; and the reason assigned by Dr. Dickinson is, that "vegetable food was more abundant there than in many situations, particularly turnips, of which large quantities were used." (Op. cit., p. 104.) Dr. Lonsdale, also, notes that in 1842-3, when milk was very scarce, on account of murrain among the cattle, there was no scurvy; and he justly observes that it is a striking fact, that the year marked by a dearth of potatoes should be chosen for the advent of scurvy.

Dr. Shapter relates several cases which occurred in Exeter in persons who could not obtain fresh vegetables, although they could procure abundance of bread, meat, and beer. Cases also occurred in the Crediton workhouse, the dietary of which was amply sufficient, until boiled rice was substituted for the potato. Dr. Shapter, therefore, considers deficiency of food, and particularly of potatoes, to be the cause of scurvy, although, as he looks on the disease as one "of depraved nutrition," he does not think it caused solely by a deficiency of vegetable food. He also attributes much concurring influence to the effect of cold.

The cases at the Salpêtrière occurred chiefly in old women; they seem to have been badly fed, and we infer from one or two expressions that they had no vegetables at all; but M. Fauvel's Memoir hardly touches on this point.

The outbreak of scurvy on board the United States ship Raritan, and some of the other vessels employed in the blockade of Mexico, seems to have been the facsimile of those severe attacks with which the history of our own navy has rendered us so familiar. Scurvy was more or less prevalent in the whole squadron, but was most severe on board three large ships, the crews of which were so disabled that they were unable to act against the enemy. These three ships were the frigates Raritan and Potomac, and the corvette Falmouth. In addition, two other corvettes and a steamer suffered less severely. In these ships we find a variety of different circumstances, which, as in the analogous outbreaks in our own navy, were present in varying degrees in each instance, and the influence of which, therefore, can be easily determined. The Raritan and the Potomac were sister ships, and were alike possessed of very bad internal arrangements and accommodations both for officers and men; the holds were small, the decks low, the hatches small and not permitting the use of windsails, and the ports very narrow, and only opened in harbour or in the best weather.

The Raritan had been in commission for three years, when she was ordered to Mexico, without being allowed time to refit; she had been cruising on the South American station, and joined the blockading squadron off Vera Cruz with a crew enfeebled by long-continued and severe intertropical service. The Potomac had the same defects of accommodation, but she had recently been in dock, had been thoroughly cleaned and coppered, and "for internal police and cleanliness was unsurpassed by any ship in the service." (p. 38.) The Falmouth, on the other hand, possessed good accommodation and was well ventilated; she had been two years in commission in an intertropical climate.

Although the men were thus differently placed with regard to ventilation, length of service, &c., they all suffered from scurvy; and there was one peculiar condition common to all, viz. the want of vegetables. "During a period of 300 days, the ship's company of the Raritan had fresh meats served out to them but nineteen times, and then

with but a small quantity of indifferent vegetables." The Potomac was in nearly as bad a plight; "from the day of sailing from the dockyard, up to the time of her arrival in Pensacola, with the scurvy on board, salt provisions only were served out, and both the beef and the pork had been long kept before using." The Falmouth was under precisely similar circumstances. Dr. Foltz makes also some important remarks on the use of vegetables:

"Our merchant whaling ships," he says, "are the only vessels in which scurvy prevails to any extent at the present day. These ships continue for many months at sea, and when in port to refit remain but a short time. In many there is but little regard for personal cleanliness, and the ships are often very offensive. On board these whale ships we have seen scurvy in its worst forms, but it always occurred in vessels where their universal antiscorbutic, fresh potatoes, had been exhausted for some time. So long as these lasted, the disease never made its appearance. . . . . . The recent change in the navy ration, which furnishes salt meat every day in the week, instead of allowing one day as heretofore, in which no animal food was served out, has apparently increased the disposition to scurvy on board our ships of war. Agreeably to the last accounts from the East India squadron, the scurvy had made its appearance on board the U.S. ship Columbus, then at the Sandwich islands. We served on that station and in the Pacific some years ago, under the former ration law, when on Fridays sailors lived upon vegetables alone, and although the ship to which we were attached was eleven months out of thirteen actually under way at sea, yet the scurvy did not make its appearance." (pp. 52-3.)

The scurvy was most severe on board the Raritan and the Falmouth; the crews of which had been more than two years at sea. It was less severe among the crew of the Potomac, just shipped, although the accommodation of this ship was inferior to that of the Falmouth.

It is thus apparent, that on board these three ships, which were supplied with no fresh provisions of any kind, and with a very small quantity of lime-juice, citric and tartaric acids, which were almost immediately expended, the causes of scurvy were in no respect different from those which have so often been followed by analogous results in our own and in the Dutch navy, and which have found such able historians and commentators in Lind, Blane, and Rouppe. And it is worthy of deep consideration, whether, in case of a prolonged naval war, we should not see, even in the English navy, a renewal of these attacks.

Dr. Curran's paper is the only one we as yet possess which is devoted solely to the consideration of the scurvy which followed, in Ireland, the failure of the potato crop in 1846, and which, with its attendant plagues, typhus and dysentery, decimated the population of that unhappy island. It is a most able article, and proves how great a loss the profession has to deplore, from its author's untimely death. We may be permitted to pay our passing tribute of respect to the memory of one among that band of self-martyrs, who sacrificed their lives in the performance of their duties, and in the noble attempt to afford such aid as science could give to mitigate the ravages of the "famine fever."

Dr. Curran enters into a long analysis of the causes of scurvy, and sums up finally in these words: "We may safely conclude that neither misery, nor the want of milk, flesh, fish, farinacea, nor any combination of these can be regarded as the cause of scurvy making its appearance." (Op. cit., p. 103.) But he goes on to say that, in no case, and he had notes of 600, "could

he discover that green vegetables or potatoes had formed a part of the regular dietary." (Ibid., p. 110.) This principal cause is aided, Dr. Curran thinks, by cold and moisture, bodily inactivity, and moral depression. As regards milk, the patients in the North and South Union Hospitals, in Dublin, were at the time of attack using a pint of milk daily.

Finally, in some cases observed by Dr. Laycock, at York, potatoes and fresh vegetables had ceased to be used as articles of diet; and Dr. Laycock not only refers the disease to this deficiency as its cause, but makes some very important observations on the prophylactic measures which should be used when there is reason to dread a deficiency in the supply of vegetables.*

When it is remembered that, in this recent outbreak of scurvy, abundant examples are to be found of its occurrence under an abundant fresh meat diet, as under an insufficient meat diet—among the robust navigators and seamen (as on board the Raritan and Potomac), as among the ill-fed weavers and artisans of Dublin, Edinburgh, and Cumberland—among those using milk, and those who never had used it, or had left it off—among those using beer, and those who drank only water—among the residents of towns, as among the inhabitants of the open country; and when it is remembered that there was only one condition common to all these classes, it must be conceded that the result which has been derived from a study of the earlier outbreaks, is abundantly confirmed.

We are of opinion, then, that the correctness of Dr. Budd's opinion has met with additional support; and it may be confidently asserted, that an invariable antecedent of every case of scurvy is a deficiency or absolute want of *fresh* vegetable food. In the late outbreak of the disease, it appears in evidence that, in every case recorded by the writers whose papers appear at the head of our article, the use of fresh vegetables, and particularly of potatoes, was much lessened or was altogether suspended. This fact, derived from individual inquiry, is in accordance with the result deduced by generalizing the observations; by which it appears that the outbreak of scurvy was coincident in this case, as in all former ones, with a failure in the supply in these articles of food.

The converse of this proposition, however, does not hold good; and it cannot be asserted that a want of fresh vegetables is invariably followed by scurvy. Much confusion has arisen from want of care in distinguishing the respective value of these two propositions, as bearing on the cause of scurvy. It is evident that the positive statement, that deprivation of fresh vegetables is an invariable antecedent of every case of scurvy, is of much greater value than the negative one, that deprivation of fresh vegetables is not always followed by scurvy.

Assuming now, as a basis for inquiry, that this deficiency of fresh vegetable nutriment, which, as proved by evidence, is an invariable antecedent of scurvy, stands in the relation of its cause, it is incumbent upon us to show:—1. That there is no other invariable antecedent. 2. That there are sufficient reasons why this antecedent should occasionally not be followed by what we have presumed to be its effect. 3. That this antecedent is a *vera causa,* and really capable of producing the alleged effect; and that the most successful methods of prevention and of cure are in accordance with the hypothesis which assumes it to be the cause.

* Medical Gazette, April, 1847, p. 573.

1. That there is no other invariable antecedent among the causes hitherto proposed by writers, has been sufficiently proved. All other alleged causes are obviously partial and limited. Scurvy does not occur solely in cold countries, nor in humid and marshy places, nor in ill-ventilated abodes, nor is it confined to the ill-nourished inhabitants of towns, nor to those living entirely on salted meats, nor to the participators in the luxuries which modern civilization has engendered, and to which several speculative writers have attributed its existence. These several presumed causes being dismissed, it only remains to be inquired, whether some other antecedent may not yet remain unindicated and undiscovered; it may be supposed that there is some other condition, yet unnoted, but which closer observation may detect. Arguments in favour of such a view may be drawn from two sources: first, from the fact that this deficiency in certain articles of diet should be often incompetent to produce the disease; and secondly, from the general opinion that scurvy should be a disease only of modern times, while the above dietetic cause must have existed to a greater or less extent among the ancients. The former argument will be presently considered. Of the latter we shall only remark, that, although there is no detailed account of scurvy in the writings of the ancients, yet that the descriptions given by Hippocrates, in his 'Prognostics,' of the "ειλεος αιματιτης;" by Pliny,* in his account of the affection which attacked the army of Germanicus, encamped beyond the Rhine; and by Strabo,† in his narrative of the sufferings of the army under Ælius Gallus, in Arabia, can apply to nothing but true scurvy. And, in addition, passages have been quoted from Aretæus, Paulus, Avicenna, Celsus, and others, which prove satisfactorily to our minds that these great observers had really noticed cases of scurvy. That these descriptions were not very minute, may be easily accounted for, by remembering that scurvy would necessarily be more uncommon in the warm climates of Greece and Italy, with their abundance of vegetables and fruits, than in the cold regions of the north, where fresh vegetables must have been in those days, as they were only two hundred years ago in England, procurable with the greatest difficulty. Lind, who has pushed this argument, derived from the supposed exemption of the ancients, much too far, inquires how it was that the method of warfare of the Greeks, their close blockades, and mode of reducing towns by famine, should not have given rise to those dreadful ravages of scurvy, which the comparatively short sieges of modern times have occasionally developed. But, because these scenes did not find a suitable historian, or because the histories have perished, are we to draw so strong an inference from them?—are the histories of the Greeks so complete and copious, that we can safely conclude the omission of any mention respecting a particular event, to be sufficient proof that that event did not occur?

At any rate, scurvy occurs now, although in a mild form, in the Ionian Islands, during the winter months, when vegetables are scarce;‡ and it is a most justifiable supposition that the same cause, less common, it may be,

* Hist. Nat., cap xxv, lib. 3. † Lib. xvi.

‡ Coup d'Œil sur les Maladies le plus importantes qui regnant dans une des îles les plus célèbres de la Grece. (Leucade ou Sainte-Maure.) Par Alph. Ferrara. Paris, 1827. pp. 45-6. In this little work is an ingenious argument in favour of the hypothesis which attributed scurvy to humidity of the atmosphere, and consequent alteration in the form of the red particles of the blood by the action of the pure water supposed to be inhaled into the lungs.

in the advanced civilization of antiquity, must yet have produced then, as now, its peculiar effects. It must be remembered, also, that in later days, as soon as history began to assume a more certain and detailed character, attacks of scurvy are recorded, exactly under the circumstances in which we should have expected, long before there had been any formal recognition of the disease by physicians, and any regular description penned by them. Such an attack was that which occurred in the army under Saint Louis, before Damietta, in the year 1260, when, with fish only for fresh provisions, dispirited by being obliged to act on the defensive, and harassed unceasingly by the Saracens, the Christian army was almost annihilated by a most terrible outbreak of scurvy. It would be against all reason to suppose, that, in the earlier crusades, similar causes did not induce similar consequences, because we have no accounts of them. And it may be noticed as a curious example of the fallacy of arguing merely from the omissions of writers, that in 1847, while scurvy in several forms was raging in Ireland, arguments were brought forward against the opinion that deficiency of fresh vegetables was the cause of the disease, on the ground that the potato crop had failed, and yet that no information had been given of any prevalence of scurvy in that country. The fallacy of this statement was soon afterwards proved by the publication of the excellent papers of Curran and others.

It may then be fairly concluded, that, as there must exist very great doubt of the soundness of the doctrine which considers scurvy entirely a modern disease, therefore this doctrine cannot be used in the way we have supposed against the proposition heretofore advanced. It may also be concluded, that there is no evidence of the existence of any other causes of scurvy than those which have formerly been enumerated.*

2. The occasional failure of the only constant antecedent, in producing its peculiar effects, is no argument against the reality of its possessing this power at other times; since there are sufficient reasons for believing that certain concurring antecedents have great influence in promoting the vigour of its action, or, on the other hand, in lessening its power. Thus, there is no doubt, although our space will not allow us to go into the evidence on the point, that want of sufficient exercise, confinement, great humidity of the air, cold, and mental depression, are all potent concurring causes. Besides these circumstances, the previous occurrence of influenza,† intermittent fever,‡ and cholera have been presumed to give a liability to the disease.§ Very few new facts have been added on these points; what there are we may briefly quote.

Thus, as far as regards inactivity and confinement, Dr. Christison mentions that the period of confinement of the prisoners in the jail at Perth had a very considerable influence on the development of the disease; for, of those under six months, 5·2 per cent. were attacked; under 12 months, 12·4 per cent.; under 18 months, 35·5 per cent.; under 2 years, 28·6 per cent. The apparent diminution in the per centage of the last term, is owing to the small number of the prisoners subjected to so

* We have not referred to the "epidemic constitution," alluded to by some writers, and which Dr. Christison in one place seems inclined to accept and in another to reject. Dr. Christison, however, believes it only to be a "predisposing cause," as he has no hesitation in fixing the essential and always acting cause in dietetic errors.

† Trotter, Med. Naut., vol. i, p. 407. ‡ Lind, p. 210.

§ Budd, Library of Practical Medicine, vol. v, p. 68.

long a term of imprisonment. In the Milbank Penitentiary, where the number of prisoners is sufficiently great to avoid all fallacy of this kind, it was found in 1823 that the increase observed a constant ratio to the period of confinement. Thus, when the unhealthy diet had been 7 months in operation, the percentage of cases was as follows:—under 1 year, 23 per cent.; under 2 years, 47 per cent.; under 3 years, 55 per cent.; under 4 years, 68 per cent.; above 4 years, 78 per cent.

On board the Raritan and the Falmouth, the disease was more severe than on board the Potomac; apparently because the crews of the former vessels were worn out by two years of intertropical service, and were dispirited by the disagreeable duties they had to perform off the Mexican coast, at the time when their service should have ended. Dr. Foltz ascribes much influence to this mental depression. It is remarkable that the most robust men of the Raritan were attacked first, and the most active sailors suffered more than the marines.

It is very certain that, if men be placed under otherwise favorable circumstances, if they breathe good air, take moderate, but not excessive exercise, are not depressed in spirits, &c., they may escape scurvy for some time, although they use no vegetables; but if the powers of the system become weakened by the predisposing causes, or if this deficiency of vegetable matter be too long continued, then scurvy must inevitably result. It is said that the Esquimaux, who live almost entirely on animal food, suffer from scurvy only when the sea-horse is very abundant, and when they not only eat more of it, but use less exertion in seeking for it. Whether this be the case or not, it may serve as an illustration of our meaning. Unfortunately, the exact influence of favorable antecedent circumstances has not been sufficiently investigated by writers. In Cook's voyages, the exemption from scurvy seemed to be owing to great cleanliness, scrupulous dryness, as far as possible, of all parts of the ship, the use of malt, fresh vegetables when they could be procured, and keeping the crew amused. But, in some of Lind's cases, when scurvy did not ensue, although vegetables were wanting, we cannot exactly discover the reasons of exemption. Occasionally, the want of fresh vegetables has been counterbalanced by the use of well-known antiscorbutic agents, as vinegar, sour krout, &c.

Our space will not allow us to go at such length into the evidence as we could wish; but we do not feel any hesitation in stating that the first two propositions are correct, viz. that there is no other invariable antecedent than the want of fresh vegetables, and that there are good reasons why this usual antecedent should occasionally be inadequate to produce its consequent. The truth of the third proposition, that this antecedent is really capable of producing the effect ascribed to it, and that treatment adequate to remove it cures the disease, will be presently considered. We may, however, state that the evidence, as generally believed, is quite conclusive also on this point.

If in every case of true scurvy there has been a previous deficiency or want of fresh vegetable food, and if this be adequate to produce the disease, and if there is no other invariable antecedent, then it is evident that the term "cause" should be applied to this antecedent in some sense different from that in which it is used when applied to the occasional antecedents, such as cold, humidity, mental depression, insufficient nourish-

ment of other kinds, &c., which may increase the action of the invariable antecedent, or may determine that it shall act, but which can never *per se* originate the disease. It is the same with an epidemic disease; many circumstances heighten the intensity of smallpox, permit its action, or modify its course, and so far are accessory causes of the disease; but without the specific poison, the formal and peculiar disease called smallpox could never result. The common distinction into predisposing and exciting causes seems to us adequate if correctly observed; it would be as well, nevertheless, if terms could be invented which would draw a still more definite line between the accessory and the indispensable antecedents.

We must, however, leave this portion of our subject, and extract what has been added of interest to the signs by which the cause or causes of scurvy betray their effects upon the system.

We have already stated that little has been added to our knowledge of the symptoms and external phenomena of scurvy, and that little is not very important. The usual mode in which scurvy commences, according to the observations of the older writers, is generally by a change of colour in the skin, particularly of the face and eyelids; coincidently there are vague, wandering, rheumatic-like pains in the limbs, weariness, depression of spirits, and a longing for fresh vegetables and fruits; this last symptom is sometimes so characteristic, that Trotter was accustomed to diagnose the disease from it alone.

On board the Raritan, the attack commenced almost always with rheumatic pains in the lower extremities, and in the early cases the true affection was not at first detected. It was noticed, however, that with these pains there was no fever, the pulse was soft and natural; the temperature of the body lower than usual. After some days the gums began to swell, and the breath to become fetid; maculæ then appeared over almost the whole body. The most robust and industrious seamen were, contrary to general experience, attacked first; there was another peculiarity also in this attack, that there was little of the customary languor and indisposition to exertion, until the disease was far advanced; then ensued as usual great prostration and tendency to syncope, but even then there was in some cases a disposition to muscular exertion which proved hurtful in several instances. After the disease was fully formed, œdema was the most common symptom, next maculæ, or complete discoloration of the skin, particularly of the legs; in one case, in addition to being black and hard, the skin of the lower extremities became insensible, and dry gangrene was apprehended. The discoloration first showed itself in the situation of old cicatrices and injuries; it varied in colour from that of a light scarlet erythema to a deep almost black purple. Ulcerations were uncommon, but very obstinate when they did occur; they sometimes originated from the patients scratching the petechiæ, and it was noticed that the ulcers spread most rapidly where the discoloration was greatest.

As usual, the appetite remained good for weeks after the teeth were loosened, the gums ulcerated, and the limbs œdematous and discoloured.

In all the aggravated cases two symptoms were prominent; the temperature of the body was lower than usual, and there was great disorder in the action of the respiratory function. In two cases there was intense pain in the region of the heart like that of angina pectoris, and in a great number of cases there was manifest disease both of the heart and lungs,

and it was noticed that these cases recovered very slowly. In fact, although not proved by post-mortem evidence, there can be little doubt that these were attacks of scorbutic pericarditis* and pleurisy. It was observed also that after these symptoms appeared, febrile exacerbations, never previously seen, occurred and assumed the character of double intermittents, there being two exacerbations and remissions in the twenty-four hours. From this state but few recovered. In a few cases there were nyctalopia and hemeralopia.

In the jail at Perth the symptoms appear to have been mild, and of the ordinary kind. Males suffered more than females, (viz. $\frac{1}{5}$ of the former and $\frac{1}{20}$ of the latter,) and adults more than young prisoners. A scurfy appearance of the integuments of the limbs is noticed as an early symptom; and during convalescence, chronic eruptions, resembling diffuse psoriasis, or in a few instances ichthyosis, appeared on the previously ecchymosed limbs.

In the Edinburgh infirmary, ecchymosed, and as if contused, patches were more general than at Perth, and a very common symptom was swelling of the back of one or both hands. In one case there was ecchymosis of the sclerotic. Ulcers occurred only on the seat of old cicatrices. Disorder of the bowels was infrequent. The urine was never red—and only once coagulable in a man who had general œdema, but who eventually recovered from scurvy, dropsy, and all signs of disease in the kidneys. Hemorrhages were uncommon. It was noticed that typhus often combined itself with scurvy, and that when scurvy became prevalent, "simple and enteric typhus" also increased.

Dr. Shapter observed at Exeter that before any declared cases of scurvy had presented themselves, an unusual number of patients had applied for relief at the city dispensary, who appeared pale and sallow, and complained of debility, faintness, and pain in the limbs. Among the earliest symptoms he observed that the gums were often unusually pale and contracted; afterwards they became red and swollen.

Dr. Curran noticed the usual early symptoms. He also observed that occasionally there were febrile symptoms and severe rigors. He agrees so far with Dr. Shapter, that the gums in the early stages were often pale and exsanguine, or with a blue line like that produced by lead, but they were never, he says, natural. He mentions the cutaneous discoloration in addition to the petechiæ, and observes that in several cases there was ecchymosis under the conjunctivæ; the temperature of the mouth was generally lower than usual; nodular and diffused indurations, dryness and scurfiness of the skin, were very usual symptoms. The digestion was at first invariably good, and the appetite was unimpaired. In two cases there was bruit de soufflet, the absence of which in scurvy has been noted as a remarkable circumstance. In many parts of Ireland, scurvy united itself with the epidemic fever, which then acquired an exceedingly low adynamic and malignant character with a heightened tendency to petechiæ and hemorrhages.

Dr. Ritchie describes the customary symptoms; in some cases there was

* The frequent occurrence of pericarditis in scurvy, noticed by many writers, but particularly described by Seidlitz and Kylier, gives us an additional instance of this disease supervening on a morbid state of the blood, in addition to those assigned by Dr. Taylor, in his admirable analysis of the causes of pericarditis.

bruit de soufflet, both venous and arterial—there was unusual throbbing of the arteries. The urine varied in sp. gr. from 1010 to 1028—in one case alkaline—in the others normal.

Dr. Ritchie has arranged the symptoms witnessed by him in certain groups.

"One variety distinguished by anæmia, emaciation, diarrhœa, dejections of fluid blood, dropsy, the circulation little affected, and the more distinctive symptoms of scorbutus wanting. A second by anæmia, often by diarrhœa, rapidity of the pulse, epigastric pain or oppression, great general distress, an urticated crimson efflorescence on the skin, petechiæ, and hemorrhages. A third by pains most commonly along the course of the nerves, but at others situated in a bed of muscle as the gluteal; the cases having a close resemblance sometimes to general rheumatism, and at others to ischias nervosa, morbus coxarius, or disease of the knee-joint; their true nature being manifested only by the sponginess of the gums, or perhaps slight ecchymosis only, the inefficiency of ordinary treatment, and the good effects of a full diet. And lastly, the more ordinary form in which affected gums and legs were the prominent symptoms."

In this description the error seems to be committed that has been charged so often against writers on scurvy, that of including all disorders of nutrition under that term as a generic head. Why, after all the care which Lind has taken to affix the term scorbutus only to cases marked by a certain determinate and peculiar concatenation of symptoms, viz. swollen gums, petechiæ and ecchymoses, should anyone, by a want of discrimination, or by a wish to simplify and generalize in a higher degree than the facts will bear, describe, as in the above extract, cases obviously the result of deranged nutritive action different from that which leads to scurvy, under the name of this disease? The first variety which Dr. Ritchie describes, presented none of the symptoms of scurvy, and was not affected by the measures which in this disease so promptly afford relief. The second variety is not very well marked, and Dr. Ritchie informs us that he only saw three cases which could be held to belong to it. The third and fourth are the cases of true scurvy that originated from want of vegetable food, and not as the former groups, in all probability from insufficient or unwholesome nutriment. We trust Dr. Ritchie will not think our remarks unnecessary and hypercritical; we would recall to his remembrance the great errors which Eugalenus introduced into this subject by confounding so many diseases with scurvy, and the severe condemnation which he has consequently received from subsequent writers. All our great authors have, of late years, touched on this subject, and entered a protest against the application of the term scurvy beyond its legitimate province. Even Sydenham, who himself has an allusion on this point, has been blamed for describing a scorbutic rheumatism; and J. P. Frank is censured by Fodéré for believing he had treated a true scorbutic fever, simply because epistaxis and patches on the skin were common symptoms. If we once stretch the use of the term, what is to prevent us from going back to the divisions of Willis, or to the twenty-six varieties detailed by old Gideon Harvey?

M. Fauvel's cases occurred at the Salpêtrière in females advanced in years; the youngest was 69, the oldest 80 years of age. The first symptoms were wandering pains, feebleness, malaise, and change in the colour of the skin. Fauvel remarks that this hue of the skin is quite peculiar, and resembles neither that present in anæmia, chlorosis, icterus, or the cancerous cachexia; it resembles, more than anything else, the

fading yellow colour of a disappearing bruise. The spots were situated as usual at the roots of the hairs, but in addition there were true purpuric spots scattered about, chiefly on the anterior surface of the limbs. The alteration in the gums could not be confounded with the general swelling and softening of certain forms of stomatitis, but there were fungous vegetations of greater or less size, developed exclusively round the neck of each tooth. M. Fauvel confirms an interesting observation made by several old observers, but not sufficiently insisted on by systematic writers, that in old people without teeth these vegetations do not occur, and the gums remain comparatively unaffected. In one case a single remaining tooth was surrounded by a mass of swollen gum, the tooth was extracted, and the gum immediately became level and firmer, while the other symptoms preserved their intensity. Coupling now this fact with the observation that the swollen and fungous gums have always appeared to be much more intense in cases occurring on board ship, it may be thrown out as a likely suggestion, that the gums are peculiarly affected because they are so much exposed to pressure and attrition. All parts suffer from pressure in scurvy—the merest rub causes an ecchymosis; the slightest possible blow produces an extensive bruise. The gums suffer in common with all parts, but in a higher degree in consequence of their vascularity, and also from their situation, which exposes them to pressure even in the mastication of the softest food, much more so, it may well be supposed, when hard biscuit and dense salted flesh are the articles of diet.

In one of M. Fauvel's cases the saliva was acid, in all the rest neutral or alkaline. The urine was acid, moderate in quantity, without albumen, occasionally rather high coloured, and of high specific gravity, but without any distinctive characters.

Dr. Lonsdale noticed bleeding from the gums as a common symptom—epistaxis less frequently—bleeding from the bowels occasionally, and the rare symptom of bleeding from the bladder in a single case. Menorrhagia was also observed, and in the advanced stages, very embarrassed breathing.

Dr. Anderson of Glasgow met with a case of scorbutic amaurosis.*

After this brief analysis of the prominent symptoms noticed in the late writings, we pass on to the most important part of the recent inquiry—*the condition of the blood.* The negative results, as to textural changes in organs, given by post-mortem examinations, coupled with the evident alterations in the blood, the whole character of the symptoms, and the nature and presumed mode of action of the efficient causes, have always directed attention to the state of the circulating fluid, and nearly all the theories of the disease have been connected with its condition. We need not pause to enumerate the opinions of Boerhaave and Hoffmann on the viscidity of the crassamentum, or the acrimonious salts and oils of the serum, nor the fanciful hypothesis of Willis on the sulphureo-saline, and the salino-sulphureous condition of the blood; nor need we discuss the conjecture of M'Bride, that the blood is deficient in carbonic acid, nor that of Trotter, that it is wanting in oxygen. We cite these half-buried fancies merely to recall the fact, that the older writers knew *where* to look for the seat of the disease, if they did not know *how* to look for it. Indeed Trotter's argument is as complete as it could well be up to a certain point, and when, after an excellent discussion, he arrives at his conclusion, that

* Edinburgh Monthly Journal, Sept. 1847, p. 176.

"the proximate cause of scurvy is still to be sought for from some peculiar state of the blood," (Op. cit., p. 124,) he has said nearly as much as we are prepared to say, even at the present day. Rouppe* and Trotter describe the blood as possessing an unusual black colour; Trotter says it has no particular smell, and that it coagulates. Rouppe also states that it coagulates in the early, and even in the advanced stages; but that then its colour becomes darker, it flows with difficulty from the vein, and after standing some hours, deposits a thick muddy sediment, which subsides from a reddish serum. In the last stage, he says, the blood is quite black, and becomes soon covered over with a thin greenish pellicle, which can be easily removed; the coagulum is greater than that of blood in the early stages, "but," he says, "it always contracted a coagulum, and separated the serum."

Lind's opinion seems to have been much influenced by the description given of the state of the blood in the scorbutic men in Lord Anson's voyage. He not only quotes the well known passage from the Surgeon's Journal, in which the fibrine is said to have been like wool or hair floating in a muddy substance, but he refers to it several times, and in one place denies the "thickening of the crassamentum"—a favorite expression of several of the older writers, on the express ground that in these cases "it was altogether dissolved and broken."† But we cannot find that Lind, from personal observation, was satisfied that there was a deficiency in the coagulation of scorbutic blood. However Lind's opinion and his use of the term "putrefaction" in the first edition, or in the after editions, "dissolution," appear to have combined with a vague half-unexpressed hypothesis that the fibrine was wanting in hemorrhages, to produce the impression that the change of the blood in scurvy consisted in part, at any rate, in some deficiency or dissolution of the fibrine. And yet even lately there have not been wanting writers who qualified this opinion. Thus Fodéré says that the blood was buffed and cupped whenever there was inflammatory complication; in the early stages of the malady without inflammation, it was dark in colour, and coagulated, but did not separate into clot and serum, but on keeping it for some time, the fibrine detached itself, and floated about in a muddy liquid. At the approach of death, again, he describes the blood as entirely black, dissolved, and without fibrine.‡ And as connected with coagulation, it may be noted that Poupart, in 1699, particularly noticed the large coagula found after death in the cavities of the heart.§

Thus, without going into details which our space will not permit, it appears that although the condition of the blood had not been accurately noted, yet that it is a mistake to suppose that it had been described as invariably broken up, dissolved, and deficient in fibrine. And in asserting unconditionally that this was the opinion of the old writers, both Andral and Becquerel have fallen into error.

Nevertheless, it seemed to be generally admitted that this was the case; and when recently the importance of the buffy coat, and of the increase

* De Morbis Navigantium, Transl. p. 180-1.

† Op. cit., p. 56. It should be observed, that Rouppe expressly mentions that, in Lord Anson's voyage, the cases were probably complicated, and that he found blood, similar to that described by the surgeon, only once. (p. 201.)

‡ Dict. des Sciences Médicales, art. Scorbut., p. 225.

§ Mémoires de l'Académie des Sciences, 1699, p. 237.

of the fibrine in inflammations became appreciated, scurvy almost naturally seemed to be the antithesis to this condition; and the observations of Lind and Huxham as to the state of the fibrine, appeared to justify the assumption. And when, in addition, the experiments of Magendie became known, in which phenomena, closely resembling those of scurvy, were produced, by injecting into the veins of animals defibrinated blood or an alkaline solution, the analogical support given to the above hypothesis seemed to be decisive of its truth.

It was, however, noticed nine or ten years ago in this country, that there were serious objections to this opinion, inasmuch as in many cases the blood not only coagulated, but did so with such firmness as to prove the presence of a considerable quantity of fibrine possessed of its usual properties. Three analyses of the blood were also made by Mr. Busk,* in all of which the quantity of fibrine was found to be above its normal standard.

In 1841 M. Andral, ignorant of Mr. Busk's observations, analysed, on two occasions, the blood of a scorbutic patient, and found the amount of fibrine below the standard (1·6 in 1000 parts of blood). This analysis, apparently confirming the previous opinion, not only seemed to point to a rational explanation of the symptoms of scurvy, but also gave rise to a very ingenious theory of hemorrhage generally. It was argued that hemorrhages might be arranged under two classes; a division which indeed, on other grounds, had already been made. Of one class typhus and scurvy were the types, agreeing in the fact of a diminution of fibrine, and a consequent disproportion between its amount and that of the red particles. The antagonistic class of hemorrhages presented a normal quantity of fibrine, but the corpuscles were increased in quantity, thus causing a disproportion of an opposite kind. In both cases hemorrhage was supposed to come on, but in the first instance it was passive, in the second, active. This hypothesis, so simple, and so accordant to all appearance with facts, recommended also by the ingenuity with which it was propounded, and by the prestige of its celebrated author's name and position, met with almost universal acceptance.

However, in 1845, M. Stœber, in a clever thesis on scurvy, opposed to this theory the analyses of Busk, and those of Parmentier and Deyeux, who in three cases found the blood of scorbutics to resemble inflammatory blood. And lately the analyses of Becquerel and Rodier,† Chatin and Bouvier, Marchal, Frick, and others, have proved its incorrectness. M. Andral, on becoming aware of these opposing facts, repeated his analysis, and found it at variance with his previous investigations. He has consequently, in the true spirit of philosophy, at once abandoned his hypothesis, and acknowledged it to have been founded on too limited a basis.

The general results of the analyses lately made of scorbutic blood, may be shortly given as follows:

1. The specific gravity of the defibrinised blood was slightly below the average. The average of Becquerel and Rodier's observations on old women of the average age of 72, makes it 1047·28; the lowest number

* They can be conveniently referred to in a note to Dr. Budd's excellent article in the Library of Medicine, vol. v, pp. 90-1. It must be remembered that this volume was published in 1840.

† The subjects of Becquerel and Rodier were M. Fauvel's patients at La Salpêtrière; and it is from these, among others, that Fauvel derived the materials for his article.

is 1038·3; the highest 1051·7. In the case of MM. Chatin and Bouvier, it was 1·060.

2. The specific gravity of the serum in two of Mr. Busk's cases in young men, was 1·028, and 1·025. In four of the cases of MM. Becquerel and Rodier, the average was 1024·3; the lowest 1020·8, the highest 1025·5. It is therefore also below the average, particularly in Becquerel's observations, which were made on old women.

3. MM. Chatin and Bouvier noticed a slight increase of alkalinity.* Becquerel states that it is inconstant.†

4. In two of Busk's cases, and in one of Fauvel's, the clot was buffed and cupped.

5. Chatin and Bouvier under the microscope observed no appreciable alteration in the figure of the red or the white corpuscles. Dr. Ritchie thinks they were more irregular in outline, and more flattened than usual; we doubt whether irregularity of outline is an important change.

6. The fibrine was in normal or increased quantity, in all but one of the analyses. Mr. Busk's three cases gave 6·5, 4·5, and 5·9 parts in the 1000. The blood of a healthy man analysed in the same way, gave 3·3 parts in the 1000. Becquerel and Rodier's cases make the average rather lower, viz., 3·1 in the 1000; the highest is 4·1, the lowest 2·2. Chatin and Bouvier's analysis gives 4 parts in the 1000 as the amount of fibrine.

Frick's‡ single case makes it 4·204. Andral's second case, in which, however, there were inflammatory symptoms, afforded 4·20 per 1000.§ Dr. Ritchie's case gave only 1·106 of fibrine per 1000. It was analysed by Dr. R. D. Thomson.||

7. In all the analyses the proportion of red particles was much diminished. In Busk's cases it descended as low as 47·8, 72·3, and 60·7 in the 1000, being as much reduced as in intense anæmia. Becquerel and Rodier's average is above any of these numbers, being 86·28, which is within the range of the limits of healthy female blood. The observations of Chatin and Bouvier, gave 86·3 per 1000, which is 24 or 30 parts below the minimum of the health-range in men. Andral's second case gave the lowest amount yet recorded, viz., 44·4000 per 1000. Frick's analysis gave 117·078, being the highest of all the cases.

8. The albumen, as far as quantity is concerned, appears less altered. Busk's analyses gave 84, 76·6, and 74·2 parts in the 1000. Becquerel and Rodier's average of the organic matters of the serum, is 64·3 in 1000 parts of blood, but this includes about 2 parts per 1000 of fats. Chatin and Bouvier's examinations gave 62·3 per 1000. In Frick's case the solids of the serum were 87·045 parts in 1000 of blood, which would give about 77·5 of albumen, being nearly the normal rate.

* Journal de Chimie Médicale, Mars, 1848, p. 141; Comptes Rendus, Fevrier, 1848, p. 171.

† Gazette Médicale, 1847, pp. 513-14.

‡ American Journal of the Medical Sciences, Jan. 1848, pp. 33-4. In the same table, Frick gives the analyses of two cases of purpura hæmorrhagica. We have excluded these, as we are quite certain that scurvy and purpura are quite distinct diseases, and possess symptoms absolutely pathognomonic. With this, all who have seen the true "morbus maculosus Werlhofii," will at once agree. We regret much that our limits will not permit us to enter on the question of their resemblances and differences. Dr. Frick notices a decrease in the quantity of lime; but this is in his two cases of purpura, not in the case of scurvy.

§ Gazette Médicale, 1847, p. 534.

|| In Dr. Ritchie's analyses, the globules and albumen in one case, and all the organic constituents in the other, are clubbed together, so that we have been unable to use them.

9. The quantity of the salts has not yet been accurately determined; it has been given at both above and below the standard. Chatin and Bouvier found 16·3 parts per 1000 of soluble non-coagulable principles. Busk's three cases gave 9·5, 11·5, and 10·9 parts of salts per 1000. Becquerel and Rodier's average is 6·7 parts per 1000, which is below their own healthy standard, and that of Lecanu, Nasse, and Andral.

Frick gives the following numbers: iron, ·721 per 1000 parts of blood; lime, ·110; chlorides, 6·846; and phosphates 1·126 per 1000. The quantity of iron is thus above the standard; and on carrying out the calculation to determine the proportion of iron to 127 parts of globules, Frick found it ·782, which is also above the normal quantity.

Becquerel noticed, on the contrary, that the iron appeared to be diminished. In his five analyses it is represented by the figures 0·522, 0·277, 0·391, 0·427, and 0·290, giving a mean of 0·381, while the normal standard in women is 0·5. In Dr. Ritchie's two analyses, the salts are given as 6·44, and 6·820 per 1000.

10. The quantity of water is of course increased in all the analyses. In Mr. Busk's three cases it is 849·9, 835·9, and 846·2 parts in 1000; while his method of analysis gave, for healthy blood, 788·8 parts. In Becquerel's five cases it is 810·9, 813·7, 807·7, 811, and 854·4; while the normal standard given by his method is 780 parts in 1000. Chatin and Bouvier state the water and loss at 831·1, and Frick at 791·69 per 1000 parts.

With regard to the existence or quantity of other matters in the blood, such as urea, uric acid, kreatine, &c., none of the analyses speak.

It is not easy to draw an average from these analyses: some were in old women, others in young men; they were performed in various ways; and in several, two or more ingredients are clubbed together. But with the view of being as definite as possible, we may give the following as an approximate average of scorbutic blood in all ages in both sexes, and at all periods of the disease: water, 830·25; fibrine, 4·214; red particles, 80; albumen, 73·15; salts, 9·336; fats, extractives, &c. 3·050. But this average must be considered as not having any claims to great accuracy.

Any general results from the above analyses must be drawn with great caution; the facts are few in number, and the discrepancies of statement are great and obvious. But it seems clear that diminution in the quantity of fibrine forms no part of scurvy, and that the disease may exist in the greatest intensity when this component is much above the standard. The very varying proportion of fibrine, on the other hand, forbids the supposition that the disease is connected with its increase; occasionally the fibrine has been certainly below the standard, with coincident severe symptoms, and on several occasions it has been in its natural quantity. Besides, augmentation of the fibrine occurs every day in inflammations, without giving rise to scorbutic symptoms. Becquerel appears to attach very little importance to this condition, and Marchal* does not hesitate to affirm that we are to look to the reactions of the tissues irritated by the effused blood, and to the latent inflammations so common in scurvy, but not proper to it, as the reasons why the fibrine is kept at its normal figure, or is augmented above this.

The change in the quantity of corpuscles appears to be a more constant occurrence. In every case the quantity is so greatly lessened, even in

* Archives Générales, Septembre, 1847, p. 122.

cases where there have been no external hemorrhages, or very slight ones, that, taken with the colour of the skin, and the dark colour of the blood so much dwelt upon by the old writers, it seems a most plausible supposition that it is in the composition of these bodies that some important change is to be found. Under the microscope their outline appears normal, and their component iron is perhaps not much changed, as it has been found in diminished quantity by one, and in augmented quantity by another observer. It is obvious that the mere lessening of the quantity of globules will not produce scurvy; otherwise, in anæmia scorbutic symptoms should be present, whereas they are hardly ever seen, and never in consequence merely of the causes of anæmia.

The other components of the blood have not yet been sufficiently studied; the salts are augmented in some analyses and diminished in others; so that if we may draw any conclusion at present, it would be that their varying proportion plays no important part.

If we accept the above results, or at least such of them as may be considered most certain,—viz., a trifling augmentation of the fibrine, a considerable lessening of the red particles, a normal ratio of albumen and salts,—as the only ones which we are to receive from chemistry, we cannot perceive that our knowledge of the pathology of scurvy has been much increased thereby. All these changes may be, and indeed often are, present, separately and collectively, and yet there are no symptoms of scurvy. Nor do these changes help us to explain in any way the phenomena of the disease; the peculiar change of colour in the skin, the effusions of blood, exudation-matter, and serum, the interruption to the circulation evidenced in the lowered temperature, and the slow, feeble, impeded pulse, all symptoms which are referable to the blood, and not to any change in the vessels, as will be presently shown, are not to be explained by any observations which have reference merely to the quantity and ratio of the components of the blood.

This explanation has been already tried, and it has failed; in the varying quantity of fibrine and red particles, Andral sought to find the physical reason of hemorrhages. The first observation made after the enunciation of the theory, scattered it to the winds. Plausible as it seemed to be, it was incorrect; and its fate has taught us once more, that which experience is ever repeating, that the most ingenious theory should be mistrusted when its chief support is derived, not from facts and observed phenomena, but from its accidental agreement with our previous hypotheses and prejudices, those cherished "idola," with which, in the infancy of science, the observer has to wage continual war.

Apparently, then, the examination of the blood has so far been serviceable, rather by clearing away old errors, than by establishing new truths. We are prepared to say what the symptoms of scurvy do *not* depend upon; we know that there is no "dissolved" state of the fibrine; we know that there is no solution of the particles in the serum; we know that there is no deficiency, nor, on the other hand, any great preponderance, in the saline materials; but the diminution of the quantities, and the alteration in the relative bearings of these ingredients, have not, as far as we can see, given any clue as to the manner in which the pathognomonic symptoms of the disease are induced.

The acute eye of Magendie some time ago perceived, and his opinion

has been quoted with approbation by Heller,* that the determination of the mere quantity of the ingredients of the blood, apart from the changes in properties or chemical composition which certainly often accompany changes in quantity, is of comparatively little use. And Becquerel, after stating that the only positive modification in scorbutic blood, is a considerable diminution in its density, a diminution out of proportion even to the lowered figure of the solid materials, closes his examination in these words:

"Is this diminished density the result of some unknown modification of the solid principles of the blood? Does it play an important part in the production of the malady; and is it the point of departure of the sanguineous infiltrations? We know not, and we restrict ourselves to a notice of the fact, without attempting to interpret it."†

With these opinions, and with the inferences to be drawn from them, we entirely accord; the statement that the fibrine, or any other component, is increased or diminished in quantity, is *per se* a piece of chemical knowledge, evidently insufficient to deal with the mysteries of disease. These facts, important as they are, are so chiefly because they form the basis of future operations; they give us a fresh point of departure, from which we are to start with a surer reckoning on our voyage of discovery; a voyage which must have these two great questions as the objects of its search, viz. what change in chemical composition is coincident with the alteration in quantity of any given ingredient of the blood, and what change in physical condition is necessitated by such alteration in quantity and presumed change in chemical composition. For change in physical condition there must be, when fibrine and red particles exude from their vessels, and infiltrate between muscular fibres, and detach the periosteum from its bone.

The few scattered observations which have been yet made known, of difference of composition accompanying change in quantity, such as the buffy coat being more highly oxidized than healthy fibrine, and facts of the like kind, do not at present aid us much in this inquiry. But as far as scurvy is concerned, a good beginning has been made by Chatin and Bouvier, who have observed two facts, which, if confirmed, will prove of great importance.

They noted that the albumen of the blood in their case of scurvy did not coagulate under a temperature of $+74°$ Cent., or $165{\cdot}2$ Fahr.,‡ whereas the usual temperature, with an equal quantity of healthy albumen, would be from 5 to 8° Fahr. below this. So that, although the albumen did not appear much altered in quantity, in this as in other cases of scurvy, it had undergone some change requiring further investigation, but which appeared to have increased its solubility.

Chatin and Bouvier also noticed that the "force of cohesion of the fibrine" was diminished so much, that they were almost unable to isolate it thoroughly from the red globules; or to use the words of the reporter of the 'Comptes Rendus,' the "plasticity" of the fibrine was lessened.

This last observation seems to us particularly interesting, because it was this very attraction, so to speak, of the fibrine and the red particles, which

* Archiv. für Physiologische und Pathologische Chemie und Mikroscopie, 1847, p. 340.
† Gazette Médicale, 1847, p. 514.
‡ Comptes Rendus, Fevrier, 1848, p. 171; ou Journal de Chimie Médicale, Mars, 1848, p. 144.

some of the early writers attempted to describe, by saying that the blood was "agglutinated," and that the crassamentum was "viscid" and "thickened," terms which Lind rejected, on account apparently of their vagueness and hypothetical character.

But this observation is besides of immediate practical importance; for if it be proved that the ease with which the fibrine and the red particles can be separated, varies with the physical condition of either of these ingredients, then the analyses to scorbutic, and indeed of all kinds of blood, require to be repeated with a view to this point alone. The estimation of the fibrine is in any case a most delicate operation; Chatin and Bouvier state that they consider it also a most uncertain one; it becomes still more delicate and uncertain if the facility with which it is performed varies at different times. Indeed Chatin and Bouvier believe that the proportion of fibrine has been, in all the analyses hitherto made, underestimated; they have obtained the fibrine by mixing the human blood with the blood of the ox or pig, the fibrine of which is exceedingly plastic, separates very readily, and in separating entrains with it the human fibrine. The quantity of pig's fibrine being determined by a simultaneous analysis of another portion of blood, the mere increase of weight which the addition of the human fibrine gives, enables them to estimate its quantity. By this difficult, though it appears very delicate, operation, they have found in two cases of typhoid fever, in one of rheumatism, and in one of healthy blood, that the proportion of fibrine was considerably above that indicated by the ordinary method simultaneously performed. Thus in one case of typhoid fever the proportion was raised from 0·0024 to 0·0032.

This would not be a matter of so much importance, if the fibrine could be always separated to an equal extent in the same way, for then the loss of weight arising from the imperfect method would be the same for all analyses, and the unindicated quantity of fibrine would be a constant quantity, and therefore comparatively unimportant; but if the increased cohesion of the fibrine, as in rheumatism, or the diminished cohesion, as in scurvy, causes a difference in each individual case, in the ease with which the fibrine and the red particles can be isolated, then all the analyses hitherto made have indicated this, as well as the absolute quantity of the fibrine. In some diseases, therefore, as in rheumatism, the quantity of fibrine has probably been under-estimated; and in other diseases, as in scurvy, it has been over-estimated; so that if this observation of Chatin and Bouvier be confirmed, all analyses hitherto made of the fibrine, are approximative only. And when we observe in several of the analyses of scorbutic blood, that the quantity of fibrine seems to be increased in an equal ratio to the diminution of the red particles,* it is not easy to avoid the surmise that this has arisen from the difficulty with which these principles are separated, arising from the adherence of the red particles to the fibrine, or, in other words, from the "diminished plasticity" of the fibrine.

* Thus, in Busk's cases, the highest proportion of fibrine, 6·5, agrees with the lowest of the red particles, 47·8—the next highest, 5·9, with the next lowest 60·7; and the lowest proportion of fibrine, 4·5, is with the highest proportion of globules, 72·3. In Becquerel's cases, the mean quantity of fibrine is less than in these cases, and the mean quantity of red particles greater; that is to say, both ingredients are much nearer to their normal standard than in. Busk's analyses. In Frick's case, however, the fibrine is increased (4·204), without proportionate diminution in the red particles, 117·078. Chatin's case is 4 to 86·3, nearly the same as Busk's third case.

We may indeed here remark, that in practical medicine, all analyses of the blood, even as to *quantity* only of the component parts, are to be received with great limitation. They are all vitiated by certain assumptions, such as the estimate of the fluid of the red and white particles with the serum, and they are performed in a manner comparatively rude and objectionable. The discordancies of chemists even as to healthy blood, are so great as to prove to us either that the ingredients can vary considerably without losing the standard of health, or that some such differences of properties, as those to which we have just referred, interfere with the uniformity of the analyses. Thus, in the comparative analysis of arterial and venous blood, in five analyses of Prevost and Dumas, and two of Lecanu, there appears more water in venous than in arterial blood; in three analyses of Hering and two of Simon (in animals), the contrary is asserted. Hering has found more fibrine in the arterial blood of the ox and sheep, and less in the horse, than in the venous blood; Simon, on the other hand, has found the arterial blood in the horse once richer and once poorer in fibrine. Prevost and Dumas, Denis, and Lecanu found arterial blood richer in globules than venous; while in four out of five analyses, Hering and Simon found more globules in venous blood.

And in disease, Poggiale, in a case, lately published,* of erysipelas with head symptoms, found the globules to be diminished to a greater extent in the arterial than the venous blood; so that the analysis of the venous blood alone, as in the usual method, would have given a wrong estimate of the quantity of this component in the whole mass of blood in the body.

Taking all these circumstances into consideration, the difficulties of even coarsely analysing healthy blood, the discrepancies and contradictory statements thence arising, and bearing in mind that in scurvy there are not above a dozen analyses that can be depended upon,† and that even these differ among themselves, we are forced to the opinion that chemistry has been as yet chiefly of advantage in negation rather than in affirmation. And when we also remember that the most difficult part of the analysis of the blood,—the question whether the several ingredients vary not only in their relative proportions to each other, but in their absolute ultimate composition,—has not yet been even touched by chemists, and that this is necessary, before the pathology of a disease can be discussed, we shall see cause for believing that a more advanced chemical investigation is necessary before we can understand the different phases between the action of the cause and the external phenomena by which it is indicated. And yet even in what has been already done by chemistry, we have made a great advance; the ground has been cleared of obstacles and superstitions; the pathway is open for those who can walk in it; the goal can be discerned, although still overshadowed by the obscurities of distance and space.

* Journal de Chimie Médicale, Avril, 1848, p. 235.

† We must also remark, that some of these analyses would be almost useless in discussing the pathology of scurvy, because the clinical details of the case are not sufficiently minute. To estimate the essential changes of the blood in any given disease, we require not only a great number of analyses, but analyses connected with the particular stages, and the progressive steps of the malady, so that we may arrive at what is constant and necessary. And particularly in a disease where there are hemorrhages, by which, as such, the blood is altered, we require accurate clinical details.

Such is our own faith; but, in order to see whether it is not unnecessarily cautious, and whether we may not at once hazard some explanation, we must enter very shortly into the examination of the hypotheses which have been formed to explain the action of the cause and the succession of the phenomena.

In the first place, however, we must remind our readers of certain elementary but important facts in the history of scurvy:

1. Scurvy is a disease which does not originate in any primary affection of the digestive organs; the appetite is not wanting until the very last stages; the food taken into the stomach is dissolved; the different stages of primary assimilation are properly performed; the chyle passes as usual into the circulation; in the pure form of the disease there is no diarrhœa, and after death the mucous membrane of the alimentary canal presents no constant evidences of disease. It may be remarked, also, that in true dyspepsia from whatever cause, when the stomach rejects its contents, or digests them with great difficulty, and with the evolution of various morbid products, scurvy is hardly ever seen, although there may be rapid emaciation from an absolute deficiency in the introduction of nutriment into the system. Scurvy is then different from any of the other forms of depraved nutrition.

2. The process of secondary assimilation is also in some sort rightly performed. There is no want of nutritive power, so to speak; the patient does not become emaciated, even when the quantity of food is much lessened by reason of the difficulty of swallowing. It is a curious remark also, that in certain cases the deposition of fat should both precede and accompany many cases of scurvy. As an instance of it, we may relate an interesting observation of Trotter, to which we shall also have to refer immediately on other grounds.

In 1783, Trotter was serving on board a slaver on the Gold Coast; the cargo was got on board slowly, and it was noticed that the slaves soon after being put on board were becoming very fat. The food consisted of beans, brought from England, and rice and Indian corn bought on the coast. These articles were boiled to the consistence of a soft paste, and then palm oil, Guinea pepper, and salt were added, so as to make the mess as similar as possible to the ordinary food. A vessel containing fourteen or sixteen quarts of this mixture was served twice a day to a mess of ten. The allowance of water was unlimited, but the slaves were confined below sixteen hours out of the twenty-four, and when on deck were allowed no exercise. After two or three months, a corpulent young negro complained of a hardness in the supinator radii longus of the right arm; this did not pit on pressure, nor was there any swelling. The next day the hardness had extended to all the muscles of the forearm, and there was some rigidity of the elbow-joint. The parts were not very painful, nor at all swelled. In this way the hardness extended up the arm, to the face, causing trismus, and to the chest and abdomen, till the man seemed bound in iron. Stupor came on, and while endeavouring to open the mouth, the gums were discovered to be black, spongy, bleeding, and falling off in fetid black masses. The man rapidly died. The slaves were now carefully examined, and it was found that the fattest and apparently best-

conditioned among them, were nearly all more or less affected with this singular induration, which was most marked in the extremities; the gums were also all swollen, bleeding, and dark; some had lost teeth; there was weakness of the limbs, and a peculiar sluggishness and indisposition to move; shortly after this, in some cases, there was bleeding from the nose and bowels, loose stools with griping; the appetite was good. After this it was noticed that whenever a negro became rapidly fat, he was nearly sure to have scurvy. The sailors were quite free. The ship put to sea, and the scurvy increased to an alarming degree; they therefore ran up to Jamaica and served out fresh vegetables which at once cured the disease.

In addition to this occasional tendency to form fat, it should be noted that in scurvy there is, as it were, an exaggerated or distorted repair of breaches of continuity. In a bad case of scurvy the skin does not, *per se*, ulcerate, but an accidental scratch spreads into a large unhealthy sore. But simultaneous with this destruction of tissues, there is present a most remarkable process of reparation—enormous granulations form, fill up the wound, and project beyond the surface; there is certainly no destruction of the reparative process, but merely a perverted application of it. The tissues of the body, also, are not merely nourished to a certain extent, but they seem to be decomposed in a proper way, and their effete particles pass out of the body as in health; there is no retention, as far as we know, of kreatine, urea, uric acid, or any other completely expended substance. Indeed, the exemption of the urinary organs from hemorrhages, and of the kidneys from albuminous or fibrinous exudations, is a very remarkable circumstance.

3. It is also a very important observation, that so far from there being any weakness or want of tonicity in the small vessels in scurvy, the capillaries passing between bone and periosteum, and stretched by the effusion of blood in that situation, have been injected, and have not allowed any portion of the injection to escape; so that the blood which had exuded from them had not done so from any brittleness or rupture of their parietes. And with regard to effused blood, it may be noted that new vessels and cell-formations take place in this; and whether this occur in the clot itself, or in exudation-matter thrown out into its meshes, is of no consequence to the present inquiry; we cite it only as an additional proof of the integrity to a certain extent of the formative agencies.

4. Another important fact is the freedom of the nervous system in all its parts from anything more than an exceptional manifestation of altered function.

5. Finally, it must not be forgotten that this disease, so fatal when left to itself, is cured with the greatest facility. Symptoms apparently most grave and serious vanish as if by magic, and without leaving behind them any serious injury to the constitution. The sanious discharge from scorbutic sores has been known to change colour, and to become healthy in a few hours after the commencement of treatment.

All these facts appear to be in accordance with the inferences drawn from post-mortem examinations, that in pure cases of scurvy the blood, and the blood only, is in fault; the powers of the constitution are intact; and it is indeed very surprising, from the evident alteration of the pabulum of the tissues, that some more serious injury should not be done to

the several organic structures. That some mischief is done, particularly to certain tissues, is evident; but that this is easily reparable, is proved by the rapidity of cure.

It seems, therefore, a legitimate inference from the facts derived from negative as well as positive observations, apart altogether from the presumed cause of scurvy, that the blood is altered in composition, either by the addition to it of some ingredient or ingredients which should not exist in it, or by the want of some ingredient or ingredients which ought to exist in it. And the deficient ingredient may be either one of the ordinary constituents of the blood, or some principle or element entering into their composition.*

It requires only a moment's consideration to show that the latter of these suppositions is the only one which will bear scrutiny. For it is evident that scurvy is caused by deficiency in certain articles of diet; it is as equally clear that it is cured at once by supplying these articles; and we may remark, that although scurvy may be very generally diffused over a country, in consequence of the universal deficiency in these dietetic necessaries, it is a mistake to call it therefore epidemic; the use of the term epidemic should be restricted to the class of diseases to which the somewhat objectionable word "zymotic" has been applied—diseases which depend upon the presence of specific poisons which enter the blood, and which, therefore, are common, in a greater or less extent, to all classes of the community, not merely to those who suffer from deficiencies of food.

The question as to the ingredient or ingredients which may be deficient in the blood has not been solved at present by direct analysis. We must in this assertion so far anticipate the objections we shall make to the views of Aldridge and Garrod, and ask our readers to receive it for the present without examination. We can then only argue as to the nature of the deficient principle or principles, by examining the nature of the article of diet which is wanting, and the nature of the remedial agent which can supply its place. The first question we have already answered, by assuming that the want of fresh vegetables is the cause of scurvy. We must, however, retrace our steps, and allude to some points which in the former portion of the article we could not so well discuss. We shall not occupy our space with a refutation of the views of those who hold that an exclusive animal diet, without vegetables, causes scurvy, because the vegetable proteine and other principles are absent; this opinion is inconsistent with the fact that scurvy, as in the case we quoted from Trotter, can come on under a diet exclusively vegetable, if the vegetables be not fresh.

We shall not, also, stop to refute the opinion which looks upon scurvy as only one of the manifestations of diseased nutrition, induced by any kind of insufficient or unwholesome food. If this were the case, scurvy would be much more common than it is.

* We cannot see that any benefit is gained by saying, with Dr. Ritchie, that in scurvy there is spanæmia of the blood. In the first place, we object altogether to the term, as too indefinite even to be applied to a class of diseases. Secondly, in scurvy, the blood, if impoverished, is so only in red corpuscles, as far as is yet known—it is enriched in fibrine. Thirdly, if the term spanæmia is to be applied only to blood deficient in corpuscles, and thereby acquires a definite meaning, it ought not yet to be applied to scurvy, as, from the few analyses yet known, we are not entitled to count on an invariable diminution of corpuscles, still less to consider this the whole disease. If the diminution be invariable, there may be still something behind, more essential to the disease, which the term spanæmia would not include.

Another opinion, which may be considered a modification of the one last referred to, holds that there are articles of food whose absence induces scurvy, and whose presence removes it, and which, therefore, are entitled to the term antiscorbutic, but these articles of food are not considered to be exclusively vegetable. The main supporter of this view, among late writers, is Dr. Christison.

The prevalence of scurvy in the Perth prison is referred by Dr. Christison to the absence of milk. It appears that in March, 1845, treacle water was issued instead of milk to all the prisoners, except the infirm, at supper, and that in May of the same year it was served out also at breakfast, so that milk as an article of diet was altogether discontinued. Now, says Dr. Christison, the third rate of the Perth prison standard diet comprises 25½ ounces of nutritive proximate principles in the anhydrous state, of which 6 ounces are nitrogenous, and of these 6, 2 ounces are from the animal world. The change from milk to treacle involves a reduction of the total nutriment to 24¼ ounces, of which 4¾ ounces are nitrogenous, and of these 4¾, ½ oz. is animal. The loss of nitrogenous principles from the animal world induced scurvy.

At first sight, and coupled with the fact that the reissue of milk arrested the disease, the case appears a strong one; but there is, on close inspection, what appears to us a certain proof, that there is something wanting in the report. It appears, that although the substitution of treacle for milk commenced in March and was completed in May, 1845, no case of scurvy occurred till June, 1846, or fourteen months subsequently. But, surely, it is in the highest degree unlikely that this great daily deficiency in nitrogenised food, which Dr. Christison has so neatly worked out, should have taken more than 400 days before its effects became perceptible. Dr. Christison, himself was staggered at this, as appears from the following observations:

"The food not being defective in quantity, a long time, and the aid of concurring circumstances were necessary to develop the disease. It is very possible that the prisoners might never have suffered, had not some other cause co-operated besides confinement and peculiarity of food. What this concurring cause may have been it is not easy to say. . . . . But the question may be left here, as there are no data for proceeding farther with it. And the utmost induction the facts will bear is, that a diet too purely saccharo-farinaceous tends to engender scurvy, that it requires the aid, however, of some co-operating cause or causes hitherto unascertained." (Op. cit., p. 885-6.)

This seems to us like a considerable qualification, if not an abandonment, of the milk theory. The absence of milk is not, *per se*, efficient; some other circumstance must aid it. What this circumstance may have been, Dr. Christison cannot determine; we observe, however, an entry made by him that,

"In ordinary years potatoes were occasionally substituted for oatmeal porridge, but in 1846, these were necessarily given up as they were scarce and dear." (Op. cit., p. 876.)

The omission of the potatoes, therefore, agrees in point of time with the appearance of scurvy; and as we know from Dr. Baly's important remarks, that at Millbank, the military prisoners, dieted like the other prisoners except that they used no potatoes, were alone attacked with scurvy, until potatoes were supplied to them, when the disease ceased,

Dr. Christison would have been entitled to attach a little more importance to this absence of potatoes than he appears inclined to do. Dr. Christison says, however, that the disease was entirely removed by a free allowance of milk. We have no doubt that the nutritious qualities of milk are often of great avail. Fodéré and others have praised it highly, but Dr. Christison must know fully as well as we do, that in the literature of scurvy are recorded many cases, in which it has been found ineffectual to prevent and impotent to cure scurvy. Even since his paper was written, we have seen Curran in Dublin, Lonsdale in Cumberland, and Ritchie in Glasgow, recording many instances in which a liberal allowance of milk was in use the very time the disease appeared. We believe, also, that Dr. Christison has not succeeded in proving a sufficient deficiency in the nitrogenised diet of all the affected railway labourers; the daily nutriment of the first labourer was 26½ ounces, of which 4½ oz. were nitrogenous, and of this 1½ oz. was animal in nature; so that the nitrogenous animal food is as only ½ oz. below the Perth healthy diet. In the second case, the daily nitrogenous food was a little above this, but still below the standard; but in the two next cases the quantity is not half an ounce below the health-allowance of the English navy, or the customary food of labourers in Ireland; and the fifth and last case had an allowance of nitrogenous food above the health-range of any class of men except the Berwickshire reapers. To account for this, Dr. Christison intimates that in these last cases this nitrogenous nutriment was chiefly gluten. He says of one of these cases, "the mere quantity of total nutriment was quite adequate, even the nitrogenous nutriment was scarcely defective in quantity; the food was therefore faulty in some other more special respect." (p. 11.) This special respect, Dr. Christison seems to place in the deficiency of animal nitrogenous principles. In other cases, he seems to think the deficiency is to be looked for among the vegetable nitrogenous principles. Dr. Christison concludes his paper in these words:

"It is impossible to carry these inferences farther with confidence, because there is a want of adequate facts as to the exact composition of dietaries productive and not productive of disease. But it is probable that a saccharo-farinaceous and fatty diet cannot be continued for a long time, however large the quantity of gluten in it, unless it be combined with articles of food containing either animal casein or vegetable albumen. It is probably to *the latter principle*, and not, as some suppose, to small proportions of salts with vegetable acids, that the ordinary *succulent vegetables* owe *their undoubted antiscorbutic* properties." (Op. cit., p. 22.)

If this then be Dr. Christison's theory, that scurvy arises from the want of vegetable albumen or animal casein, we must inquire to which of these principles he attributes the greatest influence; will the deficiency in either produce scurvy equally, or is the conjoint deficiency indispensable?* We feel quite certain that deficiency in animal principles, nitrogenous or non-nitrogenous, is not always and only active in its production; we have only to read the diet of the excavators in Cumberland, which we have quoted from Dr. Lonsdale, to convince us of this fact, without referring to the numberless analogous cases.† We are also quite certain that

* It is evident that some of Dr. Christison's patients had taken sufficient *nitrogen*, only it was not presented in what he considers the proper form.

† The worst case Dr. Curran saw in Dublin was in the wife of a shopkeeper, who had been using, for three months previously, beef tea with bread and butter for breakfast, roast meat, broth, and bread for dinner, coffee and bread and butter in the evening; together with Guinness's porter and stout. There are numerous other examples recorded in the late attacks.

vegetable albumen may be abundantly supplied, and yet that scurvy may come on; as the account we have drawn from Trotter sufficiently proves. It is not then the simultaneous want of these principles, which is the necessary condition; as either separately may be supplied, and yet the disease ensues. It must be the want, indifferently, of one of them; the same chemical result follows from two different causes. Now, this is in the first place unlikely; and, secondly, it is not in accordance with two well-known facts:—1st. That both meat and farinacea have been sufficiently supplied, and yet scurvy has resulted; so that when scurvy has appeared when either has been deficient, it is not to be ascribed to this deficiency, but to some other unindicated condition. 2d. That when the diet, under which the patient has become scorbutic, is otherwise sufficiently nutritious, or even is not so, a rapid cure has been effected without the addition either of vegetable albumen or animal casein, but by the simple administration of unripe fruits, of lime-juice, or of pure citric acid. On this argument we shall presently dilate. It appears to us absolutely conclusive against Dr. Christison's theory, even if it were not supported by the deficiencies, as we consider them, in his argument.

It appears to us also that those who refer scurvy to some defect in the nutrition of the solid part of the body, make the question unnecessarily intricate; the disease does not reach so far; it is not the muscles or the nerves which are in fault; it is astonishing how well their nutrition is carried on, considering the manifest alteration in the blood; we need not talk about the number of grains of sulphur wasted during exercise, and the necessity for supplying an equal quantity; it is in the stage prior to these destructions and renovations, that we are to search for the deficient principles.

Dr. Aldridge's theory is very different from Dr. Christison's, although we have necessarily alluded to it more than once. Dr. Aldridge does not hold that a deficiency in nitrogenised food causes scurvy; but that the real cause is a deficiency in certain minerals which enter into, and are essential to, the existence of nearly all the proximate principles by which the animal structure is built up. These principles are phosphorus, sulphur, lime, potash, and soda; the daily waste of sulphur is calculated to be about 20 grains, and that of potash and soda 84 grains, in an adult of ten stone weight. Now Dr. Aldridge finds that both seeds and flesh are usually deficient in sulphur and the alkalies. The quantity of wheaten flour which could supply the waste of nitrogen, carbon, oxygen, and hydrogen during a single day, can only supply 17 grains of sulphur, and 43 grains of the alkalies. In the same way a quantity of peas which can supply the waste of the four proteine-elements, can only give 11 grains of sulphur and 55 of alkalies. The herbaceous parts of vegetables, however, cannot supply nitrogen in sufficient quantity, but they contain the minerals in abundance; while the potato contains both organic and inorganic principles in just proportion to compensate for the necessary waste.

Dr. Garrod's theory is again somewhat different; he does not attach so much importance to the absence of sulphur and soda; in fact, it is evident that, as in the case of salted meat, soda and probably sulphur must enter the system in sufficient quantity, but he is inclined to think that it is the absence of potash only that produces scurvy. He has

worked out his hypothesis, for he does not assume it to be more, with great ingenuity, and we shall briefly detail the points which appear to us most worthy of attention in his argument. After some preliminary considerations, he states that in all scorbutic diets potash is deficient. He proves this by analysis of bread, flour, potatoes, mutton, &c., determining the amount of potash in each, and then calculating the amount which would be taken in a certain time, according to the supply of these articles of food. In the Crediton union, for example, the weekly supply of potash under the ordinary diet was 186 grains of potash for the men, and 181 for the women; but when rice was substituted for potatoes, the amount was reduced to 51 and 46 grains. In salt meat also he finds a diminution of potash, produced apparently by the action of the brine, causing exosmosis of the potash salts. He then states that all bodies proved to be antiscorbutic, contain a large quantity of potash. All fruits contain this salt,—potatoes, milk, fresh meat, pickles, sour krout, spruce beer, malt liquors, the lighter wines, &c. Alluding particularly to fresh meat, Dr. Garrod states that the carnivora must take a sufficient amount of potash to prevent scurvy.

The next statement is, that scorbutic blood is deficient in potash; this opinion is founded on a single analysis of the serum. From 100* grains of dry healthy serum, Dr. Garrod obtained 1·582 grains of the double chloride of potassium and platinum; while from the same quantity of scorbutic blood he obtained only 0·627 grains. In a single analysis of the urine, he also found a less quantity of excreted potash. We must remark that this most important part of the argument needs much greater illustration and examination, before we need even discuss it. The blood always contains so small a quantity of potash, that the diminution of it in this particular case may not have been beyond the limits of the healthy range.

Dr. Garrod next states that cases of scurvy recover, when a few grains of potash are added to the food which previously had given rise to the disease. To this point we shall immediately return.

Finally, Dr. Garrod points out the importance of potash, and he refers the muscular weakness to the want of the potash salts, which, as Liebig has shown, are essential to the action of muscle. As we shall not return to this point, we may state that such an argument is only admissible when the case is nearly proved in other ways, and that in opposition to it, men in advanced scurvy, as in the case of the sailors of the Raritan, have laboured violently till even the approach of the fatal syncope which evidently proceeds from weakened action of the heart.—We may conveniently consider Dr. Aldridge's and Dr. Garrod's theory at the same time.

The facts these gentlemen have pointed out, the deficiency particularly of potash in certain articles of food, &c., may either be solely and entirely the cause of scurvy, or it may have a share only in its production, or it may be an accidental coincidence. We conceive that against the first of these opinions there are insurmountable arguments; between the second and third it may seem difficult to decide; but we are ourselves strongly inclined to think that the third opinion is the correct one, and that the absence of potash or sulphur in the food, and of the former substance in

* The quantity of serum is stated at 100 grs.; but we surmise that this is a misprint for 1000.

Dr. Garrod's single analysis of the serum of the blood, was a mere coincidence.

Our reasons for this opinion are shortly these.

1. In the first place, the facts brought forward to prove the hypotheses are very few in number, and are, so to speak, indirect and analogical; it would be wrong to do more on such scanty premises than consider the hypothesis worthy of investigation; they derive no support from the analyses yet made.

2. As regards potash in particular, it does not appear to us to play so important a part in the blood, that its diminution or absence should give rise to such serious effects. According to Dr. Garrod's own statement,* in the serum of health, the quantity of chloride of potassium in 1000 grains of serum of the male is only 0·356 grains, and of the female 0·316.

Nor is it easy to conceive how the entire absence of this small quantity of potash should affect the fibrine, the red particles, and perhaps the albumen, to the degree we find them altered in scurvy. The absence of the sulphur or phosphorus would be a more important change.

But these two arguments might be easily put aside; the objections which seem to us fatal to both hypotheses are the following.

3. If scurvy be caused merely by deficiency in sulphur, potash, &c., it follows that the proper mode of cure is simply to supply to the blood the missing ingredient. A few doses of potash, if Dr. Garrod's view be correct, and as he indeed asserts, would eradicate the disease. It cannot be argued that the potash must be introduced gradually, in small quantities, and in combination with other ingredients, for this reason; we find that cases of scurvy liberally supplied with fresh vegetables, orange or lime juice, &c., begin often to improve *in a few hours*, so that the curative principle has not required length of time to manifest its effects; why then should potash, if this be the curative principle, require to be introduced so gradually, and demand such lengthened periods? Now it is not found that potash will cure scurvy with anything like the rapidity and certainty with which fresh vegetables cure it; it is true that nitre has been employed now and then with benefit, since it was recommended by Paterson, and it has been lauded by one or two enthusiasts like Henderson, men whose opinions are to be received with great suspicion; but there is no doubt that it has often failed and that those of the profession best informed on this subject think it of little value. We shall give immediately evidence on this point. Sulphur, on the other hand, is notoriously of no use in scurvy.

4. But, in addition, if the absence of sulphur or of potash be the cause of scurvy, the disease can of course *never* be cured, unless these principles are restored to the body. Now if we could find any substance which contains neither potash nor sulphur, and yet can cure scurvy, we might feel quite certain that the absence of those principles, even if it be proved by further investigation (which we very much doubt), had little or nothing to do with the disease. It becomes then a very important point to

* Lancet, July 1st, 1848, p. 3.—According to Frick, the proportion of the chlorides of potassium and sodium, and of the phosphates of potash and soda, varies according to the season; being at its minimum in hot and its maximum in cold weather. The difference is as much as 3 parts in 1000 of blood.

determine if uncombined citric acid can cure scurvy. We need not demand that it shall be an infallible cure; all we need ask is evidence that it exerts an unquestionable effect in certain cases. Dr. Garrod has foreseen the necessity of denying the effect of citric acid; he even appears to underrate the influence of lemon-juice, for he places it low in the scale of antiscorbutics, in conformity with the small quantity of potash it contains. On these points, however, we hold that Dr. Garrod has not made his case good; and we shall now proceed to adduce some grounds for this assertion. Before doing so, however, we must say that we have not been able to find so much evidence in authors as we could have wished, but we have fortunately been able to supply the deficiency from another source. Our readers are no doubt aware that in the last century, before ventilation, cleanliness, short voyages, an improved diet, and a liberal supply of lemon-juice, had nearly banished scurvy from the royal navy, pure citric acid had not been isolated, or was procured only in small quantities, and at a great expense; consequently no trial of its powers was made. Since 1795, when scurvy nearly disappeared, and the general mortality of the navy fell in that sudden and remarkable manner which must be attributed in some measure to the introduction of lemon-juice,* there really has been little opportunity for testing its powers.

Trotter always held the opinion that the real antiscorbutic principles in fruit and vegetables were the vegetable acids, particularly citric, oxalic, and malic acids.

To test the accuracy of this opinion, he made, in 1800, a series of comparative trials between lemon-juice and pure citric acid.† Both these remedies were furnished to eight or ten ships, and reports are given of their effects. The first report contains a case of scurvy so well marked aad reported, and cured so rapidly by the use of citric acid, that we shall quote it entire.

It is entitled "Case of Scurvy treated by the Concrete Citric Acid; Superb, July 5th, 1800."

"Robert Brown, æt. 21, seaman; labours under an advanced stage of scurvy; the principal symptoms of which are spongy, livid, and ulcerated gums, which bleed frequently; contractions of both hams, but particularly the right one, in which there is a considerable degree of rigidity, hardness, tumefaction, and livid discoloration, with a number of large blotches upon the leg; has used no medicine except two ounces of lemon-juice. Omit lemon-juice.

"℞ Sal. concret. lemon (acidi citrici), ʒss.
Aquæ, ℥viij; hodie sumend.

"His *diet* to-day has been bargou;‡ salt beef and pudding for dinner; bread, butter, and cheese for supper.

"*July 6th.* No bleeding from the gums since yesterday; no other alteration to be perceived. ℥vi of the solution to-day.

"*Diet.* Bargou for breakfast; salt pork and peas for dinner; bread and cheese for supper.

"July 7th. Gums more florid; hardness and tumefaction of the ham somewhat

* Sir Gilbert Blane's able paper on this point may be conveniently consulted in the sixth volume of the Medico-Chirurgical Transactions.

† Medicina Nautica, vol. iii, p. 391. London, 1803.

‡ Bargou is ground oats, boiled to a consistence with water.

receded. Medicine as yesterday. *Diet.* Bargou for breakfast; salt pork and peas for dinner; bread and cheese for supper.

"July 8th. Gums begin to assume a fine florid appearance, they have not bled since he began the use of the salt, and are now beginning to adhere again to the teeth; tumefaction in the ham considerably reduced; dark livid colour almost gone; he appears in much better spirits. Med. as before. *Diet.* Bargou for breakfast; fresh beef and soup, with vegetables in it, for dinner; drank some tea in the afternoon; supper as before.

"July 9th. Knee more flexible; and all the other symptoms continue to mend. Med. as before. *Diet.* Breakfast and supper as usual; fresh beef and soup for dinner.

"July 10th. A few small ulcerated points remain about his gums, but which are clean, and of a healthy appearance. Med. and diet as before.

"July 11th. Symptoms disappearing fast. Med. and diet as before.

"July 12th. Gums in a perfectly sound state; rigidity of ham almost gone; it is still a little thickened, but without any discoloration, pain, or even uneasiness; has regained his usual strength, and says that he is in as good health as ever he was in his life, and fit for his duty as a topman; to be convinced of which I have just seen him run up to the main topmast head. He takes his medicine for this day, and goes to his duty to-morrow.

"Remarks. The proportion of an ounce of the concrete salt of lemon to a pint of water, as used in this case, tastes stronger of the citric acid than the common lemon juice does; it is much more agreeable and palatable, being without any of that musty taste which the lemon juice always acquires. On arriving in Cawsand Bay, this man had no money, and from all his messmates being in the same state of poverty, he has been totally deprived of any assistance from fruit or vegetables, except what he might have eat with the ship's soup since the 8th instant; it will be observed from the report of that day, what amendment had then taken place. Several of the scorbutics on board with a milder disease, and who complained previous to this man, and who have regularly taken six ounces of lemon juice daily, continue now upon the sick list, and will remain for some days yet to come. Such appear to be the antiscorbutic powers of the concrete salt of lemon.

(Signed) Thomas Watherston,
Surgeon, Superb."

Now even this single case seems to us amply sufficient; no reasonable man can entertain a doubt, that citric acid really and solely cured this patient. Let us proceed, however, to the additional evidence. Trotter gives reports from seven or eight other naval surgeons, who all state that they found the citric acid more efficacious than lemon-juice. A certificate from Jamaica, on the contrary, affirms the superior virtues of lemon-juice.

The disappearance of scurvy from the Royal Navy has not allowed any further experience of citric acid; but on board the Australian convict-ships the disease has occasionally appeared. Sir William Burnett, whose attention has been directed for many years to this subject, as it has been to everything connected with the interests of the service, has been in the habit of supplying convict ships with citric acid and nitrate of potash, as well as with lemon juice, in order that comparative trials might be made of the relative value of these remedies. All documents which bear on this point have been with the utmost liberality submitted to our scrutiny;* and we shall now proceed to lay some very valuable evidence before our readers, as to the treatment of scurvy by all these three agents.

* We must express our grateful thanks to the eminent chief of the Navy Medical Department, for his great kindness and liberality.

The first document we shall quote is an extract from a letter dated Sydney, August, 1845. It appears that towards the latter end of the voyage of the convict ship, Mount Stuart Elphinstone, 40 of the prisoners became affected with scurvy.

"In a great majority of these the general health was not affected, the appetite continued unimpaired, and the animal functions were regularly performed. The symptoms were sponginess of the gums; an eruption of purple spots on the body, and burning of the skin; in one case induration of the tissues of the leg; in another hemorrhage from the gums. Thirty-six of these cases were selected, arranged in three classes, and treated in the following way: care was taken that the patients in each class should possess symptoms similar to each other, and that they should correspond as nearly as possible in age and constitution. To the first class, one ounce of lime-juice, and one ounce of sugar, mixed with three ounces of water, were given three times a day. To the second, citric acid in half-drachm doses, dissolved in three ounces of water, and sweetened with sugar, was given thrice a day. To the third class, one drachm of nitrate of potash dissolved in water, was given also three times a day. I also deemed it advisable to stop their allowance of salt meat, and in lieu thereof, directed an extra quantity of flour, suet, and raisins to be issued. After a fortnight's trial of these three modes of treatment, during which all the prisoners partook of the same diet and exercise, it was evident that those who had been treated with lime-juice and citric acid in solution, had much improved, and in a few the scorbutic symptoms had entirely disappeared, while in those who had been subjected to the influence of the nitrate of potash, no improvement was apparent: on the contrary, the scorbutic symptoms were aggravated in some, particularly in a case in which there was ulcer of the leg. I therefore determined to abandon the further exhibition of the nitrate of potash, and to divide those belonging to the third class between the first and second, after which their improvement was manifest. With regard to the relative efficacy of the common lime-juice and a solution of citric acid, in arresting the course of, and ultimately curing scorbutus, I am inclined to prefer the former; but the latter undoubtedly possesses high antiscorbutic virtues, and has this advantage over the lime-juice, that it can always be procured fresh, while the other is prone to decay."

This report is by far the most circumstantial and precise; but in addition, since 1840, the surgeons of ten convict ships have reported scurvy on board. We shall insert merely the general result of their experience.*

1. The first reporter states that the patients were relieved more speedily by citric acid than by lime-juice or nitre.
2. The reporter states that citric acid and nitrate of potash possess equal virtues, and that both are preferable to lime-juice.
3. Only 4 cases; no benefit from any of the articles.
4. The reporter's experience was limited, but he has no faith in the nitrate of potash.
5. Lemon-juice most useful; nitrate of potash objectionable.
6. Nitre and citric acid equally useful; both more so than lemon-juice.
7. Citric acid and nitre both ineffectual and hurtful.
8. Only 3 slight cases; the citric acid was mixed with nitre.
9. Citric acid most effectual; nitrate of potash next; lemon-juice also useful, but less decidedly so.
10. Only 3 cases; the benefit from the citric acid much more evident than from the use of nitre.

* One report has been for special reasons excluded.

In addition to these reports, the journals of all the convict ships were carefully gone over by the officers in Sir William Burnett's department, and the following is the abstract they have made. In 11 ships scurvy had more or less prevailed.

1. 11 cases of slight scurvy; the cases yielded to lime-juice, citric acid, or nitre.
2. 24 cases of scurvy; all the plans were effectual, and all the cases were recovering when the ship made the land.
3. A few cases; all cured by lime-juice, and, in some instances, sliced raw potato and vinegar.
4. 18 cases; citric acid and nitre both completely failed. The lemon-juice had been expended owing to the length of the voyage.
5. 34 slight cases; all cured by issuing preserved meat and lemonade.
6. The cases were not numerous; both citric acid and lemon-juice seemed to have considerable power in arresting, not removing, the morbid deposition of the gums. The reporter "is not inclined to place any reliance on nitrate of potash."
7. 1 case; not benefited either by citric acid or nitre.
8. 1 case; benefited by citric acid.
9. 4 cases; in 2 citric acid was tried, but the reporter cannot speak in its favour; the nitrate of potash with sugar is a much more valuable remedy, if the dose be increased daily.
10. 5 cases; too few, the reporter thinks, to determine.
11. A number of prisoners embarked with scorbutic symptoms; in eight or ten days the citric acid removed them.

With reference more particularly to the effect of lemon-juice, a note has been affixed by Dr. Bryson to these documents, which is of great importance:

> "As a proof," it is remarked, "of the utility of lemon-juice as an antiscorbutic, it appears, on reading over the journals for 1842, that in *eleven* ships, where lemon-juice was issued daily in the usual manner, only 56 cases of scurvy are reported; whereas, in *two* vessels, where lemon-juice was not issued to the prisoners, for reasons specified by the medical officers, upwards of 100 cases, many of a severe nature, occurred before they reached the Cape of Good Hope."

We cannot conceive that anything can be more convincing than the above evidence; the efficacy of citric acid is clearly proved, and the nitrate of potash is evidently inferior to it in power. It is incontrovertible, therefore, that scurvy may be cured without potash, more rapidly than with. This appears to us completely to upset Dr. Garrod's theory, as well as, for other reasons, it overturns the opinions of Drs. Aldridge and Christison.

But there is another point. Dr. Garrod appears to us to underrate a great deal too much the powers of fresh lemon-juice; let him look back to the older authors, and he will find it impossible, as Sir Gilbert Blane says, to over-estimate its astonishing effects. Now, the virtues of lemon-juice may be owing to citric and other acids, or to potash;—independent of all the previous evidence we have quoted, we are inclined to think it must be to the acids. The antiscorbutic principle, whatever it may be, is in greatest amount in unripe fruits;* it lessens gradually as they ripen,

* Trotter's well known experiment proves this, as well as general experience.

and if the juice is obtained, it disappears when fermentation occurs. It is therefore some principle which is destroyed by the fermentation. Now, we presume that this is not the potash, which must remain in normal quantity. We know, on the other hand, that when lime-juice becomes musty, a mucilaginous principle is developed at the expense of the citric and malic acids. This is proved by several analyses, particularly by some which Sir William Burnett has had made, and which show very clearly the gradual decrease in the per centage of the citric acid.

Every one knows the immense difficulty of preserving lemon-juice, and how often it becomes ineffectual at the time it is needed most. We need not wonder, then, if these circumstances are not properly estimated, that the reputation of lemon-juice as an antiscorbutic has at times suffered. In a document in Sir William Burnett's office, it is stated that scurvy broke out on board a convict ship; the lemon-juice was in casks and bottles—that in casks had fermented and become thick; it had more the appearance of pea-soup than lemon-juice; the patients "loathed it," instead of manifesting as usual the greatest avidity for it. It was, however, used; but was ineffectual. The lemon-juice in bottle was good, and was employed with the best effects in the worst cases.

We may state here, that we have no doubt that good lemon-juice is more effectual than pure citric acid; this may be from its containing malic and tartaric acids, besides citric, or, as is very probable, from the citric acid being in the form most easily absorbed and decomposed. The Materia Medica gives us many analogous examples of the superior efficacy of a medicine in its *natural combinations*.

There is another point which we would recall to Dr. Garrod's remembrance,—the first two and most important clauses of his argument apply almost as well to the vegetable acids as to potash.

Thus, we may safely say, in all scorbutic diets, which are those without fresh vegetables, the vegetable acids are deficient.

We may also say, that the bodies proved to be powerfully antiscorbutic, contain the vegetable acids. Thus, according to Einhoff and Vauquelin, quoted by Baly, potatoes, the most certain antiscorbutic we know, contain a large quantity of vegetable acid, either the tartaric or the citric. Wheat, barley, and oats contain no organic vegetable acids. With regard to potatoes, Dr. Garrod finds they lose potash by boiling without their skins, and they lose, he says, their virtues in a commensurate degree. Dr. Baly says, boiled are as good as unboiled potatoes. We observe that Foltz states, that unboiled potatoes are the best.

To sum up our conclusions.

We have not a doubt that the whole bearing of the facts we have brought forward, tends to this conclusion,—that true scurvy is caused by a deficient supply of the organic vegetable acids, or salts of fresh vegetables. This has long been the prevalent opinion in this country, and it has by far the strongest arguments in its favour. Whether occasionally the absence of potash or of sulphur (which we have no doubt produces its own peculiar symptoms, although these are not what we term scorbutic), may not occasionally aid in the development, we shall not decide. The peculiar forms of dyspepsia, which may, however, result from such a deficiency, are deserving of the deepest study.

Nor shall we attempt to determine how the organic vegetable acids act. We have little doubt that it is by virtue of some decomposition which they undergo. Trotter long ago remarked, that their utility seemed to be measured by their facility of decomposition. But whether, when decomposed, they supply constituent materials to the fibrine, red particles, or albumen of the blood, or whether they exert, as Dr. Anderson has surmised, some catalytic power, which promotes the assimilation of the nutritive part of the food—an action which may be compared to the effect of iron in anæmia,—we cannot undertake to say. Nitre may undergo some similar decomposition in the cases where it proves useful.

We are really sorry that our critical duty will not allow us to accord with the hypotheses, so well put and so forcibly urged by Aldridge and Garrod. It would have been a great step if we could have done so; but, until they disprove our facts, and bring a great many more of their own to back their opinions, we must refuse our assent.

We observe that Dr. Williams, in his very excellent 'Principles of Medicine,' has passed over the subject of scurvy, as being too little known to be advantageously discussed.* But from this opinion we must dissent. We know as much of scurvy as of most diseases. We know its cause; we have traced its distinctive symptoms; we can cure it at once, if we can procure the necessary remedies. What more do we know of smallpox or of scarlatina? We know that the cause is a specific poison; we are certain when that cause is acting, by peculiar signs; experience has taught us how best to manage the disease and the accidents most prone to occur during its course. But of the internal changes in the blood and tissues induced by the poison of smallpox, and evidenced by the external signs, we know no more than of the internal changes occurring in scurvy:—not even so much, for chemistry has been more active in one than the other case. In both cases, chemistry, assisted by the clinical observation of the physician, and guided, as it were, by his knowledge of the progress of disease, derived from countless observations, is the science to which medicine must apply for many facts necessary to deduce the pathology of certain affections. Chemistry has already done something for us in even the very few analyses of scorbutic blood which have been made. We have in them an earnest and a proof that more will yet be done, and that, if we are cautious in restricting chemistry to its legitimate province, the greatest benefit will result from its employment in the investigation of disease.

* Principles of Medicine, &c. By Charles J. B. Williams, M.D. F.R.S., &c. London, 1848. Appendix, p. 515.

ART. IX.

Practical Observations in Midwifery, and the Diseases incident to the Puerperal State. By ALFRED H. M'CLINTOCK, M.D., F.R.C.S.I., Vice-President of the Dublin Obstetric Society, and Lecturer on Midwifery, &c. in the School of Medicine, Park-street; and SAMUEL L. HARDY, M.D., F.R.C.S.I., Vice-President of the Dublin Obstetric Society.—*Dublin*, 1848. 8vo, pp. 368.

THE authors of this work officiated as assistants of the Dublin Lying-in Hospital during the first three years of the mastership of Dr. Johnson; and by the permission of the latter gentleman, they have taken the cases which occurred during this period as the materials of the present publication. It may hence be looked upon as a report of no less than 6634 cases of labour, occurring between January 1842 and January 1845; and as combining the positive experience of this number of cases, with all their diversity of complication, and the particular practice of Dr. Johnson. Undoubtedly the most valuable record of obstetric facts which has been published of late years, is contained in Dr. Collins's practical treatise on midwifery, founded on the result of 16,654 births occurring in the Dublin Lying-in Hospital during his mastership of seven years; and it is no slight praise to our authors to say, that they have taken Dr. Collins's treatise, in many essential particulars, as their model; and for the truthfulness of their facts, the clear management of their tables, and the general soundness of the practice which they inculcate, their work may fairly be said to imitate his. The professed object of the authors has been to render their volume a practical clinical treatise; and although it contains but few new or original views, and in this respect, therefore, does not materially improve the art of midwifery, yet the bed-side experience which it includes, affords an admirable test of points of practice which have been advocated by distinguished obstetric authors, and on which the profession has been wont to rely.

The introductory remarks contain some interesting results of the 6634 cases of labour. The number of children born was 6702; and of these 3550 were males, and 3151 females. Of this number of children born, there is a considerable preponderance of deaths during parturition amongst the males; which confirms Dr. Simpson's idea that the danger to the child is greater in boys than girls; while the number of children born in a putrid state was somewhat greater amongst the females than the males; a fact which the statistics of Dr. Collins confirm. The number of primiparous women was no less than 2125; which is nearly one third of the whole amount.

Our authors notice the fact, that in premature labours, the pelvic extremity of the child is far more commonly the presenting part, than when labour comes on at term. Out of 108 premature children, 17 presented with the breech, 12 were footling, and 9 arm presentations; making in all 38, or in the proportion of nearly one in three cases. It certainly is difficult to account for this, which is a fact quite in accordance with general experience, excepting on the supposition that the fœtus, up to the seventh month, enjoys and exercises extensive movement within the womb; until, towards the middle of the seventh month, it becomes fixed by the increased length of the

womb, and the corresponding increase of the vertical diameter of the fœtal ovoid; so that the latter cannot pass by the transverse measurement of the uterine sac, and it becomes definitely fixed with the head downwards. We have ourselves been struck, in auscultating a pregnant female near the seventh month, to find the fœtal heart clearly above the central line for two days in succession; and a week afterwards, when examining it again, to find it as clearly below the central line, in its usual situation in the first cranial position, in which at labour it has presented. But why the head should so uniformly occupy the lowest part, we know no more than why the beak of the embryonic chick should be directed towards the folliculus aeris; and we agree with our authors, that M. Dubois' notion of an instinctive impulse, or again of the influence of gravity, as the causes in operation, are equally unsatisfactory.

In the chapter on natural labour, the use of the binder, in aiding the equable contraction of the uterus and the favorable separation of the placenta, is strongly insisted on; and it appears to have been employed in every case. A very important fact is mentioned as the supposed result of this practice; that for the three years comprised in the report, and for two years previously, the hand had only to be introduced once for the removal of a placenta retained from hour-glass contraction, and but rarely in consequence of inaction.

The number of cases of natural labour,—which, after Denman's definition, is strictly limited to head presentations completed within twenty-four hours without artificial aid,—amounted in the three years to 5852; and of these 16 died, whose cases are detailed. Of these, 4 died of uterine phlebitis, 2 of peritonitis, 2 of phthisis, 1 from mania, 1 of arteritis, 1 of sloughing of the uterus and vagina, 1 of laceration of the peritoneal coat of the uterus, 1 of pneumonia and bronchitis, 1 of scarlatina, 1 of anasarca, 1 from a tumour compressing the trachea.

Uterine phlebitis. In connexion with the cases of uterine phlebitis, the authors have appended some short observations on the causes, pathology, symptoms, and treatment of this fatal disease; which they appear to have studied with much care. Following the analogical illustration derived from phlebitis in the arm, they have divided uterine phlebitis into three stages. The *first* stage is that of local inflammation, characterised by uterine hardness and pain, with rigor, rapid pulse, coated tongue, depraved or scanty lochial discharge, recession of the milk, &c.

The *second* stage, which announces the presence of a material poison in the blood, is marked, in addition to the symptoms already enumerated, by the recurrence of rigors, an excited action of the arteries (most visible in those of the neck), gradually increasing debility, profuse sweats, foul and generally dry tongue, a disagreeable nauseous smell of the breath, most resembling raw meat, muscular tremors, and occasionally low delirium. The *third* stage, we conclude (although the authors are somewhat obscure on this point), is represented by the various secondary and distant affections, the result of this contamination of the blood; such as arthritis, inflammation of the pleura, pericardium, or lungs, diffused cellular and muscular inflammation and suppuration, inflammation of the eye, cutaneous eruptions, &c.

Although a careful study of a series of cases fairly justifies this attempt to discriminate these three stages of the disease, and make them coincide

with the reception and spread of a puerperal poison, yet in practice, as the authors notice, the first two stages sometimes run into one another in such a way as to render its detection difficult, until the signs marking the second stage have appeared. We have not unfrequently been struck by the fatal facility with which inexperienced or unobservant practitioners have overlooked, as unimportant, the first warnings of this formidable disease, and have been only awoke to the conviction of its presence, by the near approach of the death of the patient. The authors have described, with much practical precision, the different signs of this malady, taking them singly, and their relative diagnostic value; and the paragraphs on the rigors and pulse are especially deserving perusal. Our own experience quite accords with theirs, in ascribing the greatest importance to the occurrence of a rigor, when it does not distinctly proceed from milk-fever or 'weed,' especially if associated with a rapid vibrating pulse; and the occurrence of either or both of these, ought to arouse the practitioner to the most watchful attention. "A short time before the fit of shivering, the pulse usually falls in frequency; so that if reckoned by the physician at this time, he might be incautiously led to give an opinion, which the occurrences of the next half hour would induce him to retract." In some cases, as we have ourselves witnessed, both the local and constitutional symptoms are very obscure and imperfectly marked; and it is not until the disease has made considerable progress, that the peculiar expression of the patient, more than her complaints, have announced the dreaded evil, which has been still more masked by the perfect unconsciousness of impending danger in the mind of the sufferer herself.

We do not gain from this report any new hints for the treatment of this disease. If the first stage is recognised—and it is everything to detect the mischief at its onset,—a strict antiphlogistic treatment, such as "general and local bleeding, the warm bath, and the administration of mercury so as to affect the system," is considered most deserving confidence. The indications in the second stage are: "1st, to relieve or mitigate any urgent symptoms, such as diarrhœa, sleeplessness, vomiting, &c.; 2d, to support the strength by diet as mild and unstimulating as possible; and lastly, to enjoin the strictest rest of mind and body, to confine the patient to the horizontal position in bed, to prevent every source of excitement, moral or physical, and in short to adopt with unremitting vigilance every means calculated to tranquillise the system, and to abate or ward off inflammatory action."

"For checking diarrhœa and procuring rest," say the authors, "opium is the sheet-anchor." At the same time they deprecate the use of stimulants, which in their experience has appeared to aggravate the febrile symptoms. Most obstetric practitioners, who have seen uterine phlebitis on the scale, which a large public charity can alone exhibit, must accord with the authors in deploring the utter insufficiency of our known remedial means in combating it. At the same time, we are not disposed to reject the use of stimulants in the summary manner of our authors. We have ourselves of late years found more good, speaking generally, from a free, almost a lavish use of opium, with ammonia and other stimulants, than from the employment of mercury; and we believe that this primary dependence on opium is gaining ground amongst obstetric practitioners of repute in this metropolis.

We pass by the remarks on phlegmasia dolens, and puerperal mania, to notice the observations of the author on *Tedious and Difficult Labours.* A great and highly practical distinction is drawn between delay before the os uteri is dilated,—in other words, delay in the first, and delay in the second stage of labour; a distinction which is well insisted on by Dr. Churchill, and which, in our own experience, forms an important fundamental division for the student of midwifery clearly to appreciate. The labour of dilatation is free from the dangers of the labour of expulsion. In the former, the head of the child is retained above the os uteri, and the stress of uterine action is felt only upon the yielding circle of the womb. In the latter, any impediment to the passage of the child acts by detaining the head in the cavity, causes serious pressure on soft parts which are not able to sustain it, and exposes the sexual passages and pelvic organs to congestion, inflammation, and gangrene. If the general precepts for the management of labour,—such as ventilation of the room, attention to diet, and leaving the parts alone,—be judiciously enforced, it is remarkable to see the tolerance of the first stage of labour. Twenty-four, forty-eight, sixty, or even seventy hours may be occupied in it; and yet no sign be present, either in the sexual organs or in the system, to awaken anxiety. Such a delay, however, in the second stage, especially when the head does not recede freely after a pain, is far more serious; and the fear of exhaustion is much to be apprehended. Our authors refer to 259 cases of tedious and difficult labour, on which their practical remarks on this subject are founded; and they have tabulated them very carefully, so as to convey at a glance several important particulars, and amongst them the stage of labour in which delay occurred. The table referred to is defective in one point, in which respect it is inferior to Dr. Churchill's; viz. that the relative length of the two stages of labour is not remarked, which would have been very instructive. The single cause of delay in the first stage, which is noticed and commented on by the authors, is a rigid unyielding state of the os uteri; which usually occurred in primiparous women, in whom the membranes had ruptured at an early period of labour. The effect of age in increasing this condition of structural rigidity, is mentioned and admitted; but "to the rule that the difficulty and delay will be proportioned to the age of the patient," say the authors, "there decidedly is a limitation; for experience has shown that the maximum of resistance in the soft parts is between the ages of about thirty and thirty-eight. After this period, the tone and firmness of the animal fibre begins to diminish; and hence the os is not so capable of offering that degree of resistance, which it frequently does at an earlier age." The management of lingering labour from rigidity of the os uteri, is strictly in keeping with acknowledged rules of practice; consisting of venesection in women of robust plethoric habit, tartarised antimony in others whose powers do not admit of general bleeding, and the warm bath. The latter expedient was only used when the two former had failed to relax the os; and the authors throw out a hint that it exerts a deleterious influence on the fœtus, although in our judgment without sufficient reason. In cases where the anterior lip of the os uteri was caught between the head and the pelvis, "cautious and gentle attempts were made to push it up above the head in the absence of pain, and to retain it up by the finger until the recurrence of uterine action." Belladonna was used in

only one case of rigidity of the os; as Dr. Johnson, like most other practical obstetricians, has no confidence in its efficacy. The cause of delay in the second stage may be uterine inertia, or a want of adaptive relation between the fœtal head and pelvis; or, again, there may be a mixed case "where there is just sufficient locking of the head to prohibit the use of the forceps, but not such as that it could not be overcome by good uterine action." The great uterine stimulant for the second stage of labour is ergot of rye; which, with the use of the vectis, forceps, and perforator, are fully considered. The remarks on the ergot of rye, embodying as they do the views which Dr. Hardy has already published,* are eminently useful and practical. The deleterious effect of the ergot on the fœtal circulation, in reducing the number of the heart's beats, and then causing them to intermit and become irregular, and finally to kill the child, is an observation which goes very far to define and limit the employment of this drug. Both Dr. Beatty and Dr. Hardy concur in thinking two hours as the limit of safety to the child; although a fatal result may occur in from twenty minutes to half an hour. We know that there are numerous practitioners who ridicule this assertion, and who appeal with confidence to a large experience in the use of this drug, to justify an opposite opinion. We have ourselves tested it, however, by carefully listening to the child's heart, and numbering the maternal pulse, when ergot (a good recent preparation having been selected) has been exhibited; and although we have met with more exceptional instances than Dr. Hardy, yet generally we quite agree in the foregoing conclusions. We do not hesitate to say that the practice of midwifery is more shamefully abused in the reckless ignorant use of the ergot of rye, than in anything else; and we believe that a recognition of its poisonous influence on the child will do much to correct this abuse. The authors insist on the paramount importance of listening to the fœtal heart during tedious labours; and the value which they set on the information so obtained, when ergot has been administered, may be gathered from the following quotation.

"From the foregoing observations it may be plainly seen that a very high degree of importance is here attached to the use of the stethoscope in all cases of tedious labour where ergot is employed; in fact, its administration is never undertaken without the utmost care being used in the auscultatory examination of the fœtal heart, both at the time and subsequently; for by making successive stethoscopic examinations at short intervals after giving the ergot, and closely watching any change that may take place in the character of the fœtal heart's sounds, we shall have the earliest intimation of threatened danger to the child, and, by the timely intervention of art, be able to rescue it from a position which will inevitably prove fatal, unless prompt delivery be effected. We have repeatedly witnessed the advantage of auscultation in these cases, and had the gratification of seeing children extracted alive, who, we are satisfied, would have perished had there been no such unerring guide to indicate the precise moment at which to interfere, and beyond which any delay was attended with infinite hazard." (p. 81.)

There are three classes of cases in which ergot was employed. The most common were those where the head was arrested from inert uterine action. The second class of cases, which were rare and occurred generally in primiparous women, "were those in which the fœtal head, without any discoverable pelvic deformity to account for it, became arrested in the

* Vide Dublin Medical Journal, vol. xv.

brim of the pelvis, and remained in this position until bad symptoms began to develop themselves;" ergot was then given to bring the head within reach of the forceps. The third class were instances in which unfavorable symptoms came on—demanding that delivery should be hastened—where the fœtal heart was audible—but the forceps and vectis were inadmissible. We cannot do more than thus to indicate the practice at the Dublin Hospital in the administration of the ergot; but we are only doing justice to the authors in saying, that their researches on this subject, and their remarks on the use and value of auscultation in lingering labours, are most instructive. We will merely notice that out of 173 cases of tedious labour delivered without instrumental assistance, 30 had ergot of rye, and only 10 of the thirty children were born alive; although, in accordance with the rules just stated, the vitality of the child was ascertained before the drug was administered, and in the great majority labour was completed within two or three hours after it had been taken.

Out of the 259 cases of tedious and difficult labour, *fifty-two* were delivered by the perforation and crotchet, *eighteen* by the forceps, and *sixteen* by the vectis.

We cannot but advert to the fact, that Dr. Johnson and the authors repudiate altogether the use of the long forceps. This, we know, is quite in accordance with the practice of other masters of this institution before Dr. Johnson; and the only thing in its favour, is that men of such high repute should hold so tenaciously to the rejection of this instrument. To those who, like ourselves, have learned by experience the application of the long forceps to deliver the head when detained at the inlet of the pelvis, and who are fully satisfied of the safety and success of this operation, the doubts, not to say prejudices, of Dr. Johnson and his predecessors, will appear to be fanciful and fallacious. In common with most of the accoucheurs of this city, who have an extended field of public practice at their command, we are disposed to set a very high value on the judicious use of the long forceps; and we cannot but see that some of the cases of tedious labour in this report might have been abridged greatly, to the comfort and safety of the mother; and we are persuaded, too, that the crotchet cases would have been numerically lessened, had the long forceps been employed in time. It is only within the last fortnight that we delivered a living child by the long forceps, where the head was unable, from contraction of the conjugate diameter, to pass the brim; and we had the good fortune to persuade a gentleman, who had imbibed the views of the Dublin school, where he had studied midwifery, in whose charge the case was, that this instrument might be used without injuring the maternal structures, in short, with safety to the mother and the child. We are quite willing to admit the dangers which may follow its employment by inexperienced and ignorant persons; but the same objection attaches to the short forceps or crotchet; and we state our own conviction, that, in adhering so pertinaciously to the rules which make the short forceps and vectis the sole extracting instruments, Dr. Johnson and those who think with him must sometimes lose the chance of preserving fœtal life, and occasionally expose the mother unnecessarily to the pains and penalties of protracted and exhausting labour. This is hardly the place for us to enter more fully into the discussion of this important practical question; and we avoid this the more willingly, as the employment

of the long forceps has been advocated by many authors and obstetric teachers of acknowledged talent both here and abroad, and the profession generally is sufficiently familiar with their views.

Preternatural labours. The preternatural presentations which the report includes, are those of the upper and lower extremity, which amount together to 227 case; this number includes 62 cases (39 breech and 23 footling) occurring in twin births. All these cases are clearly tabulated, which adds materially to the value of the record; and those of particular interest (and there are many) are narrated at length. The general management of breech and footling cases is much the same as that laid down by authors. In the delivery of the head, some stress is laid on the elevation of the occiput by pressing it up; and where children are premature, it is recommended that the arms should not be withdrawn, but allowed to remain up, which proceeding prevents the os uteri from spasmodically grasping the head and delaying the birth of the child. In about one third of the footling cases, the face of the child was directed forwards towards the mother's abdomen; but there was no necessity for any effort to turn the child's head under the notion of rectifying it, as this rotatory movement was effected naturally.

In the management of arm or shoulder cases, the authors agree with Dr. Collins and Dr. Joseph Clarke, in objecting to an attempt at version where the child is dead and there is difficulty in doing so; as they consider that opening the thorax and abdomen, and fixing the crotchet on the pelvis, so as to bring it down first, is less injurious to the mother. Tartar emetic, in one sixth or quarter-grain doses, was found to be very beneficial in relaxing the os uteri and quieting uterine action, preparatory to introducing the hand for the purpose of turning.

The practice of bringing down a knee or a single foot, in preference to grasping both feet, is advocated, in accordance with the opinions of Dr. Breen and Dr. Radford, and in conformity with the advice of most obstetric writers of recent date.

Complex labours. Under this division are included hemorrhages in the later months, convulsions, rupture of the uterus, plurality of children, and funis presentations.

However different *accidental* and *unavoidable* hemorrhage are in their cause and attendant circumstances, yet it is not always easy to discriminate between them, when first the bleeding comes on, and the os uteri is closed and high up. Not only does accidental hemorrhage occur independently of any immediate shock, but placental presentations sometimes cause profuse bleeding, without the establishment of uterine pain. Hence every source of diagnosis deserves and requires accurate testing; and the authors have been thus led to inquire, whether two supposed signs of unavoidable bleeding, noticed by M. Gendrin, will hold good practically. The first of these is a fœtal pulsation, synchronous with that of the child's heart, to be felt at the os uteri; and the other is the difficulty of detecting the passive movements of the fœtus by ballottement, on account of the interposition of the placenta between the head and the os uteri. With reference to the first, the authors sought for it carefully in a case of central implantation of the placenta, but in vain; and the second sign is notoriously inadequate as a diagnostic mark.

The treatment of accidental hemorrhage is by rupturing the membranes,

which beautiful and simple expedient is, as the authors testify, and all practitioners agree, rarely ineffectual. When much blood has streamed away, leaving the patient very weak, the bleeding having been subsequently controlled by rupturing the membranes, it becomes a consideration, say the authors, whether labour may not be postponed for a time, by administering a full opiate. Of this practice they speak favorably; although we must confess that we are hardly disposed to adopt it. Two fatal cases, which occurred to Dr. Johnson in private, of that rare but interesting form of accidental hemorrhage, where blood is effused between the placenta and uterus, without appearing externally, are noticed. In both, the placenta, excepting at its extreme margin, was entirely detached from the uterus; and the cavity or interspace between the two contained an enormous quantity of partially coagulated blood. The practice, in cases of placenta prævia, is in keeping with the ordinarily received rules; neither has Dr. Johnson shown any partiality for the proposed plan of Drs. Simpson and Radford. The authors tell us that "Dr. Johnson entertains very strong objections to the practice; not only because it necessarily destroys the child; but also from a conviction of its inapplicability to cases of rigid os uteri, which is the chief or almost only obstacle to the performance of turning in placenta prævia cases." The management of unavoidable hemorrhage is by rupturing the membranes in partial, and turning the child in complete, placenta presentations; while time is gained, and the bleeding arrested, in cases where the os is not open and yielding enough to admit the hand, by the use of the plug. In this, as in accidental hemorrhage, the authors speak of the value of opium; and as their views are practically important, we transcribe them.

"It has already been stated under what combination of circumstances the use of opium in accidental hemorrhage is calculated to prove serviceable. Its power of checking uterine action, however, generally renders the sphere of its utility very limited in this species of flooding; whereas in unavoidable hemorrhage this rarely constitutes so decided an objection to its use, but, on the contrary, often renders it peculiarly eligible.

"Judging from what we have seen of its employment in this latter class of cases, it may, we think, be exhibited with a prospect of benefit, where there has been an alarmingly profuse loss before the mouth of the womb has dilated to such an extent as to admit the passage of the hand. The tampon can be used in conjunction or not, according to circumstances. In examples of this kind, the opiate acts beneficially in two ways, first, by recruiting the patient's strength; and secondly, by diminishing the hemorrhage, which effect is directly dependent upon its success in producing a temporary suspension of the pains. Should unavoidable hemorrhage take place in consequence of a threatening of premature confinement, the administration of a full opiate is well worthy of trial, in the hope of its tranquillising the uterus, and thereby postponing the accession of labour, as it is particularly desirable, in cases of placenta prævia, that gestation be far advanced before parturition sets in.

"Again, where the os uteri is fully dilated, or nearly so, but yet the patient is so extremely low as to excite apprehensions of a fatal result, if the operation of turning be attempted, a full opiate (provided that the hemorrhage be not actually going on) will probably be found of the most signal service, by giving time for the administration of nourishment, and for rallying her weakened energies, after which there will be a better chance of a favorable issue to the operation. We have seen opium administered under the circumstances just described, and with the most satisfactory results. Such cases, doubtless, are extremely rare, but they are about the most perplexing and disagreeable we can meet with." (pp. 201-2.)

Without adverting further to the paragraphs on *hemorrhage after the birth of the child*, beyond admitting the practical wisdom of the precepts which they inculcate, we must refer for a moment to the treatment of *hemorrhage after the delivery of the placenta*. The indication which is peremptorily required in these cases, is to excite and secure the contraction of the uterus; and our main resources for this purpose are "friction and pressure over the uterus, the application of cold, the exhibition of ergot of rye, the use of electro-magnetism, and the introduction of the hand into the uterine cavity." Each of these means is separately considered by the authors. We quite accord with them in estimating the value of external pressure by the hand over the uterus so highly in these cases, as to supersede the last-named expedient, viz. the introduction of the hand. Dr. Johnson but rarely adopted the latter practice, which, as the authors say, "is dangerous in two ways: first, if the patient be extremely weak, it may extinguish life at the moment of its performance; or, secondly, if she survive the operation, and recover from the immediate effects of the flooding, she is very liable to be subsequently attacked with puerperal phlebitis." Amongst the advocates for this practice, which Dr. Johnson, as we think, wisely reprehends, is no less an authority than a former master of the hospital, Dr. Collins;* who, in speaking of this subject, says, that "if the uterus be *much relaxed* and *distended*, these means (viz. pressure and cold applications, &c.,) will at times be found insufficient; in such cases the *cautious* introduction of the hand is decidedly the best mode of proceeding, and almost always successful." In applying cold, the authors prefer the douche of cold water with a towel over the pudenda, nates, and sacrum, to the pouring water from a height. Sometimes a draught of iced water at the commencement of an attack of bleeding, seemed to excite the uterus, and an enema of cold water, plain or with the addition of some red wine, was found to be eminently useful.

The ergot was extensively used by Dr. Johnson to combat this form of hemorrhage; but, from its depressing influence on the mother's heart, it is not regarded as an admissible remedy, when the patient's powers are much exhausted.

Of electro-magnetism, as a uterine excitant, the authors speak only theoretically.

Opium was usually exhibited when any alarming symptoms of prostration began to be developed, and when restlessness or inquietude appeared; and our authors, like most obstetric practitioners, set a high value on its efficacy in this form of hemorrhage.

Retention of the placenta. It is usual in the Dublin Hospital to wait two hours before using any active interference for the extraction of a retained placenta; and as the liability to phlebitis is justly dreaded, after the placenta has been peeled away from the uterus in consequence of adhesion, a mild mercurial course is at once commenced; and either increased, if inflammatory symptoms come on, or suspended about the third day, if the patient be free.

Our space will not allow us to enter upon the two valuable chapters on puerperal convulsions, and rupture of the uterus. There were 13 cases of the former, and 9 of the latter complication; and the practical remarks of the authors well deserve an attentive perusal.

* Practical Treatise on Midwifery, p. 154.

Plural births. Ninety-five women were delivered of twins, and one of triplets, during the three years of the report; and of these two died, one from hemorrhage after delivery, and the other of uterine phlebitis. The statistical results of these cases are valuable. The author refers to the diagnosis of twins, by hearing the two fœtal hearts at different parts of the abdomen, and by the want of correspondence in the rhythm of the beats. We have successfully diagnosed twin gestation by this means; and we agree with the authors in thinking the ordinary signs of twin pregnancy very equivocal. The practice of the hospital in the management of twin cases, is as follows:

"After the birth and separation of the first child, the binder is applied with a moderate degree of tightness. A vaginal examination is then made, and if the second fœtus be found presenting favorably (that is, with the head, or lower extremities), its membranes are forthwith ruptured. This is a slight deviation from the practice usually laid down by authors; but Dr. Johnson is of opinion that it is better to let off the liquor amnii at once, than to wait any time before doing so; and we certainly think that this measure tends to produce a firmer and more permanent contraction of the uterus subsequently. Great faintness or exhaustion of the patient might render some delay advisable; but such a condition has very rarely occurred at this period. Should any part of the upper extremity of the second child present, the operation of turning ought to be undertaken immediately, before the pains have time to be renewed, or to have acquired much strength. If good uterine action did not come on within forty minutes or an hour after the rupture of the membranes, a stimulating injection was thrown up the rectum, and this, with a little friction over the uterus, seldom failed to increase the energy of the pains. On a few occasions, the above means were inadequate to excite sufficiently-powerful uterine action, and in these instances the ergot of rye was administered, and with favorable results. In each of them the fœtal head had descended more or less into the pelvic cavity, before exhibiting the medicine. This circumstance is mentioned, because some doubts may be entertained as to the propriety of giving ergot before the head has been fairly engaged in the superior aperture of the pelvis." (pp. 321-2.)

The embarrassment which is occasionally produced from one part of both children presenting together, and mutually obstructing the process of delivery, is considered at some length; and a case in which the right leg of the second fœtus descended into the pelvis with the head of the first, is related amongst the twin cases at the hospital, and other recorded cases referred to. The practice recommended under these circumstances, which was successfully adopted in the above-mentioned case, where both children were born alive, is to push up the extremity which comes down with the head; and if this should fail, to push up the head and bring down the breech.

Funis presentations. There were 37 cases of prolapse of the cord during the three years of the report. Twelve children were born alive, of whom 9 presented with the head, 2 with the feet, and 1 with the arm. In 5 of these, the funis was returned above the head; in 1 case delivery was effected with the vectis, and in the remaining 6, the management was not materially affected by the complication. The authors distinguish four periods when the funis may descend. 1. It may be felt within the membranes during the first stage of labour. 2. It may escape into the vagina. 3. It may descend beyond the presenting part in the second stage. And, lastly, it may occur when the head is fairly engaged in the pelvis, or even pressing on the perinæum. This complication, so exclusively perilous to

the fœtus, appears to destroy life when the funis is compressed, almost as quickly as an adult is killed when the lungs are obstructed.

Dr. Johnson does not perform version in these cases, as was recommended by Mauriceau, and practised when detected early, and the soft parts admit of it, by Hamilton, Denman, Gooch, and others. The following plan for rectifying this displacement, is recommended and adopted by the authors:

"The plan that we have been in the habit of following for the reduction of a prolapsed funis may now be described. The patient is placed as much as possible across the bed, upon the side *opposite* to that on which the procidentia exists; thus, if it be towards the right sacro-iliac junction (as happened in nearly all the cases we have seen of this complication), she reclines on her left side in the usual obstetric position; but if the descent has taken place at the left sacro-iliac symphysis, she is made to lie on her right side. This is the first point to be attended to; next is the hand. In preferring one hand to the other, our object is to use that whose dorsal surface can most conveniently be kept near the sacrum, for much greater facility will be thereby obtained in accommodating the fingers to the concavity and direction of the pelvis: if therefore the woman be lying on her left side, the left hand is used; and if on the opposite side, the right hand.

"These preliminaries having been arranged, the index and middle fingers are introduced into the vagina, during an interval between the pains, and the funis is drawn gently forwards, in order, if possible, to bring it to a shallow part of the pelvis. We then endeavour to pass it up, beginning with the most dependent portion, and afterwards elevating the remainder by little and little, until the whole has been pushed up out of reach of the fingers." (p. 342.)

In the fourth class of cases, it may be necessary to help the head forward, either by exciting the womb to stronger efforts by stimulating enemata or ergot of rye, or by using the vectis or forceps.

Two short but useful chapters on the management of stillborn children, and the treatment of ophthalmia neonatorum, conclude the book.

In bringing this brief review to a close, we must express our sense of the merits of the authors, in the clear arrangement of their work, and the sensible way in which they have treated the different subjects which compose it. With the exception of the strange reluctance to the use of the long forceps, which appears to be traditional at the Dublin Lying-in Hospital, we discover nothing but the soundest principles of midwifery, advocated in a luminous and conclusive manner. Perhaps with such an unrivalled field for observation, we might have expected some improvements in the practice of midwifery and the treatment of puerperal diseases—something which should bear the stamp of originality; but the Master seems proof against innovations, and his great experience has been directed rather to the consolidation of the art of midwifery by the selection of the best amongst known rules of practice, with slight individual variation, than to the more alluring and, in some respects, higher object of extending its boundaries, and augmenting its resources. It is with some knowledge of the sympathies and wants of obstetric practitioners, that we recommend this book to their attentive perusal; and we confidently hope that, if the succeeding Master is unwilling to go through the toil of collecting the cases which occur during his term of office, he will, like Dr. Johnson, encourage his assistants to accomplish this honorable and useful task in his stead.

ART. X.

Chemie und Mikroskop am Krankenbette. Ein Beitrag zur medizinischen Diagnostik mit besonderer Rücksicht auf das Bedürfniss des praktischen Arztes; bearbeitet von DR. MARK AUREL HÖFLE.—*Erlangen,* 1848.

Chemistry and the Microscope applied to Clinical Medicine. A Contribution to Medical Diagnosis, with especial reference to the Necessities of the Practical Physician. By DR. MARK AUREL HÖFLE.—8vo, pp. 683.

THE position of assistant to the Medical Clinique in the University of Heidelberg, occupied by the author of the above-named work for nearly four years, gave him an opportunity long desired of studying with zeal the chemical and microscopical branches of diagnostics. A residence of several months in Paris in the autumn of 1843 allowed him also to profit by the instruction of Drs. Gruby and Lebert, and, more especially by that of his countryman, Oberhaüser. Thus qualified for the task, he resolved to undertake the present work,—an attempt to embody those results obtained by chemical analysis and the microscope, which bear upon diagnosis, as a study distinct in itself; just as the results obtained by the stethoscope and auscultation generally have been grouped together, and separated from what are called the rational signs or tokens of disease. The work opens with a table of the chemical constituents, elementary and compound, organic and inorganic, of the human frame. Then follows a succinct account of the apparatus and reagents requisite in the chemical investigations subsequently described, and brief directions concerning the proper management and application of the microscope. These preliminary matters discussed, we are introduced to the essential subject of the treatise. The first section is devoted to an examination of the surface of the body. Under this head is included the natural-history description of the epizoa and epiphyta, of the itch insect, of the acarus folliculorum, of the sporidia or sporules, beaded corpuscles, thallus-threads, and granules, developed in various skin diseases, as favus, herpes tonsurans (porrigo decalvans), pityriasis menti, and plica polonica. Under section 2 the digestive system is examined. The healthy and diseased secretions of the tongue, the sordes collected round the teeth, the fungus-cells developed in the diseased secretions poured out in thrush, the exudation peculiar to diphthérite, and the matters discharged by vomiting and by stool, form the subjects of the chapter. Section 3 is occupied in discussing questions connected with the blood. First are considered the properties and constituents of healthy blood. Next are noticed the various methods of chemical investigation and quantitative determination of its composition. The author then passes on to an examination of the phenomena of spontaneous coagulation, and of the buffy coat; proceeding afterwards to inquire whether urea, bile, sugar, and carbonate of ammonia are normal constituents of healthy blood, and whether reported cases of it, in which this was present in the blood, be deserving of credit. We are next presented with a classification of those diseases in which the constituents of the blood undergo a notable change; a classification which, disregarding all other affinities and relations of the diseases classified, is based solely upon these chemical changes in the blood. The following is the arrangement adopted:

I. Diseases in which the composition of the blood deviates little or not at all from the normal standard.—Homoiocrases. Fever, plethora.

II. Diseases in which the composition of the blood deviates from the normal standard only in an increase or diminution of single constituents. Heterocrases:

α. Hyperhydroses. The quantity of water is strikingly increased, and the quantity of the fixed constituents, but especially of the blood-corpuscles, correspondingly diminished.—Oligæmia (after loss of blood), chlorosis, scrofula, lead-poisoning.

β. Hyphydroses. The quantity of water is strikingly diminished, and the sum of the fixed constituents increased.—Asiatic cholera.

γ. Hyperinoses. The quantity of fibrine increased as a rule.—Inflammations, rheumatism, erysipelas.

δ. Hypinosis. The quantity of fibrine diminished as a rule.—Cerebral congestions and cerebral hemorrhages, putrid fever, scurvy.

III. Diseases in which, as a rule, the blood contains constituents which have hitherto not been positively proved to exist in healthy blood. —Heterochymeuses: Bright's disease, jaundice, diabetes mellitus. This table is followed by a detailed account of the state of the blood in the diseases just enumerated.

Section 4 treats of the various kinds of sputa; section 5 of the saliva; section 6 of the secretions of the sebaceous and perspiratory glands; and section 7 of the urine in its normal and abnormal state; of the qualitative and quantitative analysis of this fluid; of its healthy and diseased microscopical ingredients; of the different kinds of sediment and deposit; and of the changes in the urine in particular diseases, as albuminuria and melituria. Section 8 is dedicated to an investigation of the milk; section 9, to an investigation of the secretions of the genital organs; section 10, to an investigation of calculi generally—urinary, biliary, intestinal, salivary, and so forth.

In glancing through the work, we find nothing of particular interest until we arrive at the chapter in which the author discusses the subject of vegetable parasites. This subject is introduced by Dr. Höfle in the following manner:

"The plant parasites which infest the surface of the body, have as yet received as little attention from botanists as those found in the interior of the body; and their position in a systematic classification is therefore still uncertain. For this reason, and since, even at the present day, their vegetable nature is strongly doubted by certain physicians and botanists, it will be best in the present case not, as in the epizoa, to commence the discussion with a consideration of their systematic characteristics, but first to quote without commentary the observations upon which the proof of the vegetable nature of these parasites rests, and then, as far as may be at present possible, to attempt to determine their true systematic location. The microscopical investigation of those diseased structures, which belong to the supposed class of vegetable parasites (under a magnifying power of 300—500 diameters), exhibits the following elementary forms. I. Roundish or oval corpuscles of some 1-600''' diameter, cohering sometimes in twos or threes, or forming little groups. Certain of these corpuscles exhibit a constriction, so that afterwards two halves of unequal or very nearly equal size arise. All these forms are denominated sporidia or sporules. II. Moniliform rows, formed of corpuscles undistinguishable from those just described, except by the fact of their aggregation. (These are named by Remak sporule-bearers.) III. Fibrils of almost the same or less diameter than the roundish and oval corpuscles, and of various lengths, sometimes completely transparent,

sometimes filled with dark granular contents, commonly provided with transverse partitions, sometimes simple, sometimes branched, or furnished with lateral offshoots. Occasionally they seem to anastomose with each other. These structures are called thallus-fibrils. IV. Granules, which on account of their minuteness, present no distinction of form. (Elementary granules.) These constituents, when they appear all together, form a thick, confused network, which becomes more transparent on the addition of a little acetic acid. The corpuscles above designated as sporidia, are seldom found seated on the twigs of the fibrils or in their angles; they commonly lie loosely around, frequently they are observed to possess a peculiar rotating motion." (p. 48.)

These facts supply us with the following proofs of the vegetable nature of the parasites in question: 1st. No constituent tissue of the human body, normal or abnormal, at all resembles or agrees with the above-described forms, considered in all their grades of development. 2d. On the contrary, the development of the roundish or oval cells into moniliform rows, and their multiplication by spontaneous division, points out a decided analogy with the yeast plant (torula cerevisiæ, Turpin) of the family of conio-mycetes; while the simultaneous appearance of thallus-fibrils with sporidia constitutes the character of the thread-fungi (family, hyphomycetes), to which, for example, belong the various forms of mould. 3d. Direct experiments, with pieces of the favus (porrigo) scab, show that the favus-fungus germinates even when removed from the human body, and developes itself after the manner of other fungi (Remak).—The cutaneous diseases, in which only fungi have been hitherto discovered, are favus (true contagious ringworm, porrigo favosa, and scutulata), herpes tonsurans (porrigo decalvans), pityriasis menti, pityriasis versicolor, plica polonica, and crusta serpiginosa. At page 64 Dr. Höfle, continuing the subject of vegetable parasites, speaks thus:

"The fungi (in general entophyta) which characterise certain exudations from the mucous membrane of the mouth, pharynx, and œsophagus, occur under two forms. Either certain extremely simple growths, exactly resembling the yeast-fungus, are found alone; or at the same time manifest thallus-fibrils, similar to those of favus, are also developed. Between the former variety and the ferment-fungus there exists, properly speaking, no difference of any kind; for both (under a 3-400 magnifying power) exhibit a congregation of cells of about 1-600‴ diameter, partly round, partly elliptical or oval, isolated or arranged in rows of 2, 3, 5. Sometimes they are nucleated, and sometimes their contents are completely homogeneous. They multiply both by spontaneous division and endogenous cell-formation."

The author proceeds to quote the case of a female child, æt. 12 days, the subject of thrush in a mild form on the under-lip, but otherwise healthy. The diseased secretion, examined under the microscope, exhibited these appearances:

"1st. Round and elliptic cells of a diameter of 1-300—1-600‴, transparent and homogeneous; similar cells, provided with an eccentric nucleus. Many of the round and elliptic cells presented on their periphery a little excrescence, separated in some cases from the parent cell by a more or less complete constriction, in other cases wholly disjoined. These cells were either isolated, variously grouped, or formed (especially the elliptic cells) moniliform strings of from 3 to 6 members. In the interior of the cells, in a certain light, a certain molecular motion seemed to obtain. 2d. Thread-like structures of a considerable length, on an average 1-600‴ in diameter. They formed a confused meshwork in which were distinguished three varieties of form, for the fibrils were either simple cylindrical canals, provided with septa, or they were branched, and exhibited constrictions and partitions at

the joints, or they were mere canals without either branches or partitions. All these three varieties were indifferently either straight or bent, with forked branches at their extremities or sides, whereby I could evidently perceive that that cell of the branch which was nearest to the stem was separated by a proper membrane. The interior of the fibrils was filled either with transparent globules, which did not touch the walls of the canal, or with minutely granular contents, which were also disposed more in the middle of the canal. Transition-forms from the round and elliptic cells to the moniliform rows, and from these to the complete fibrils with parallel margins, were unmistakeably present. In addition to these vegetable cells, the morbid secretions examined contained numerous plates of pavement-epithelium, a structureless organic substance, and two kinds of monad." (p. 65.)

Connected with the subject of parasitic plants an interesting question arises. We are induced to inquire whether the parasitic plants, as they occur in the secretion of thrush, of porrigo favosa, of plica polonica, &c., are in reality distinct species or genera, different from the closely-allied yeast-fungus, and distinct one from the other. Hitherto this investigation has been conducted solely by pathologists, who, actuated by certain preconceived theories, have declared that such distinction does exist, have maintained that these vegetable forms constitute the essence of the disease in which they appear, and have even in some cases undertaken to revise the ordinary nomenclature of disease in accordance with this principle. Thus Grüby, among other barbarous innovations, hisses into our ears such terms as porrigo-phyta, rhigo-phyto-alopecia, and mentagrophyta. On the other hand, it may be that all these supposed numerous "morbophyta" are one and the same. Nay, they may even be identical with the yeast-plant and the simple fungus developed in a putrescent albuminous fluid. On this supposition, the presence of vegetable parasites in any morbid secretion ceases to possess diagnostic value; the parasites are there, as it were accidentally, and that clue to the intimate nature of certain diseases, of which at length we were so sure, totally fails. If the vegetable cells of favus, as Busk and Wilson maintain, originate in the decomposing smegma; if these cells and fibrils be identical with those found in the morbid secretion of thrush, and in a putrefying albuminous fluid; how little interest will these structures have in future for the student of disease! The mycologist will have to undertake the investigation, declined by the pathologist.

At page 26 of the observations appended to the work, Dr. Höfle gives at considerable length the details of two comparative experiments instituted for the purpose of watching the development of the yeast-plant, and of the fungus generated in an albuminous solution. From the results of these experiments he was induced to believe, that the torula cerevisiæ and the torula albuminis (as it might be called) are the same. Comparing the microscopical appearances and properties of these torulæ with the fungi of morbid secretions, he maintains that these also are identical.

At the 100th page of the work, Dr. Höfle commences his disquisition upon the blood, and in the 255th he brings it to a conclusion. Through these 155 pages we have diligently waded, hoping to make ourselves acquainted with new experiments, and perhaps even original views; but there appears to be a resemblance in one respect between hæmatology and ancient mythology, for whenever one begins to speak of the blood, "still the old instinct brings back the old name—'Tis *Andral and Gavarret* that

give us all that's sound, and *Becquerel and Rodier* all that's trustworthy." A few analyses by Franz Simon, Denis, Popp, and one or two others, complete the list. In fact, the 155 pages are rather a summary of what has been effected by others, than an original essay by the author.

Recent experiments upon what is termed milky serum possess a certain physiological, though but little pathological value. It had been observed that, after coagulation, the blood drawn in pregnancy, or shortly after a meal, exhibits a peculiar white or milky appearance, while the crassamentum, generally firmly contracted, and swimming in the serum, appears sometimes white only through the serum that covers it; sometimes, however, its surface is covered with a thin, cream-like layer. This must not be confounded with the true buffy coat, which, however, is not unfrequently present. The bulk of the crassamentum is always dark red, as in ordinary blood. After standing some time, the opacity of the serum increases to a certain degree, and, notwithstanding filtration, which does not render the serum more translucent, a kind of cream forms, which sometimes exhibits a thickness of several lines. Sometimes globules of fat are perceptible to the naked eye, floating on the surface of the serum; but this is not very frequently the case. Under the microscope no distinction can be traced between the creamy layer and the serum itself. In addition to the accidental admixture of blood- or chyle-corpuscles (under a magnifying power of 3-500), fluid or finely divided fat, or a very minutely granular precipitate, is observed. The fat appears in globules of various sizes, which powerfully refract and partially reflect light. The careful addition of ether causes them to run together into larger drops, and finally to dissolve entirely. When no fat is present, but only an amorphous precipitate, it appears as a fine, grayish powder, precisely similar to that urinary deposit which consists principally of lithate of ammonia. In this grayish powder, ether effects no change. Dilute acetic acid also has little power over it, while concentrated acetic acid and caustic potash dissolve it completely. These properties suggest the conjecture, that the grayish powder in question is a protein-compound. It has received from Zimmermann the name of "molecular fibrine."

"Near the end of February, 1845," says Dr. Höfle, "I drew 16 oz. of blood from a very powerful man, æt. 36, who had never before during his whole life suffered from any important disease. Five days previously he had received a violent blow beneath the left breast, in consequence of which an acute local pain and difficulty of breathing declared themselves. The blood formed a tolerably large clot, with a manifest tough buffy coat, and an almost milk-white opaque serum. This change I first observed five hours after the bloodletting, during which time the blood stood in a heated chamber, but near the window. I poured a portion of the serum into a cylindrical glass, during which operation it became slightly red, from the unavoidable admixture of blood-corpuscles. After it had stood a day, and the blood-corpuscles had wholly subsided, the fluid appeared milky, and was covered with a manifest, white, creamy pellicle, a line in thickness. Under the microscope this pellicle presented precisely the same characteristics as the serum itself. It exhibited a great multitude of little granules (of about 1-1000‴), which, when treated with ether, diluted acetic acid, and weak caustic potash, remained in great part unchanged; and were only gradually dissolved by concentrated acetic acid and caustic potash. The serum itself, shaken with ether, yielded a proportion of fat scarcely exceeding the normal quantity. In this case, therefore, the opacity of the serum was manifestly exclusively due to the presence of a protein-compound." (p. 185.)

Commenting upon this case, Dr. Höfle states, that the person from whom the blood was taken, was somewhat addicted to drinking; but that, according to the testimony of his wife, who was constantly near him, during the five days which intervened between the receipt of the injury and the bloodletting, he had scarcely tasted any intoxicating liquid. His diet had been spare, consisting of soup and vegetables. On the day of the bloodletting, he asserted that he had taken no food whatever. The symptoms from which he suffered were not those of a general pleurisy, but were limited to those which appear in a simple case of fractured rib. The patient soon completely recovered. On the 26th of January, 1846, consequently about eleven months after the accident just related, Dr. Höfle was again called to the same individual, who was suffering from cerebral congestion, induced by intoxication. Under these circumstances, Dr. Höfle thought it right to abstract blood; but a careful investigation of its properties showed that it differed now in no respect from ordinary blood. Dr. Höfle gives the following enumeration of diseases in which the milky serum has hitherto been seen: Ophthalmia epidemica (Zimmermann), Tracheitis (Nasse), Pneumonia, Pleuritis, Peritonitis, Puerperalis, Enteritis, Hepatitis, Splenitis, Nephritis (Lauer), Febris intermittens, Plethora (Lauer), Asphyxia (Rayer), Dyspnœa c. hæmoptysi (Lecanu), Phthisis pulmonalis, Morbus Brightii, Diabetes mellitus, Congestiones cerebrales, Delirium tremens (Kastner). As a general rule, subject, however, to many exceptions, we may state that, in acute inflammations, in congestions of the head, and in Bright's disease, the milk-white serum is commonly caused by the presence of "molecular fibrine;" while in phthisis pulmonalis, diabetes, and delirium tremens, the milk-white serum is merely an evidence of an increased quantity of fat in the blood.

The remainder of the section on the blood offers nothing which merits quotation. We are informed for the hundredth time, that, in inflammations, in rheumatism, in erysipelas, the quantity of fibrine is increased (the exanthematous inflammations of smallpox, scarlet fever, and measles, however, do not augment the quantity of fibrine); that in typhoid fevers the clot is soft and fragile, and sometimes even absent; that in chlorosis the red corpuscles are diminished in number, while the fibrine maintains or even exceeds its normal proportion; that in Bright's disease urea is present in the blood, and the quantity of albumen diminished. We pass therefore at once to consider the section devoted to the investigation of the urine. It is well known that Liebig has lately declared, that all urine contains hippuric acid, and indeed, that this acid is present in equal quantity with uric acid. In order to satisfy himself on this point, Dr. Höfle instituted two experiments upon his own urine. Previously to undertaking the experiments, he confined himself for a few days to a particular diet, consisting of animal and vegetable food, with a little beer. He also took moderate exercise. In the first experiment, upon 12 oz. of urine, made up of that passed in the morning and that passed in the evening, he adopted Liebig's method of analysis; but he did not succeed in detecting a trace of hippuric acid. In the second experiment, the method advised by Dr. Golding Bird was pursued, but with the same want of success. Dr. Höfle, however, regards this last method of analysis as insufficient; since, if hippuric acid really existed in the urine, it would, when submitted to the joint action of heat and hydrochloric acid

(as in Dr. G. Bird's method), be resolved into benzoic acid and glycocoll (Dessaignes). But if Liebig's assertion, that hippuric acid always exists in urine, be true, it is, to say the least, very strange that hippuric acid has never been found in urinary calculi.

It is asserted by Wöhler, and the assertion has been implicitly received, that, when we partake pretty freely of acid fruit, the urine becomes alkaline, and that such alkalinity depends upon the presence of a carbonated alkali, the tartaric, citric, or other vegetable acid undergoing conversion into carbonic acid during its passage through the economy. Certain experiments, however, instituted by Dr. Höfle, render the universality at least of this conversion very questionable. In the autumn of 1846, the following experiments were made. For the first experiment the author selected 1 lb. C. G. (German weight) of grapes, but of this 3 oz. consisted of stalks and skin. In the other experiment, pure tartar was employed. Both before and after the experiments his health was perfect; and the urine which he passed immediately before each experiment distinctly reddened litmus-paper. The experiments were conducted on the following plan. At seven o'clock in the morning he took, fasting, the matters above mentioned, and took nothing else, not even water, from that moment till one o'clock, p. m. Except that, after the tartar, the author had several stools in a few hours, he experienced no marked difference from the ordinary state of health. Every half-hour during this time he passed water, and placed the samples beside each other, in order to observe the differences in colour and other changes in the urine. The following results were derived from both cases. All the subsequent samples were much paler than the urine passed previous to the experiment; the intensity of the acid reaction was diminished, but the acidity was never entirely destroyed. Exactly in proportion as the urine contained more water, and exhibited a lighter colour, it appeared to act with less intensity upon litmus-paper; for, when the urine passed before the experiment, which possessed a strongly acid reaction, was gradually diluted with water, the intensity of its acid reaction diminished in an exact proportion with the diminution of thei ntensity of its colour. Besides litmus-paper, Dr. Höfle employed every time muriatic acid and chloride of platinum (with alcohol)—the latter in a solution of one part to twenty-four parts of water—as reagents. But, neither did the muriatic acid cause effervescence, nor the chloride of platinum a more copious precipitate than in ordinary urine. In the summer of 1847, Dr. Höfle repeated the experiment with black cherries, but with as little success as before. C. Schmidt also had equally failed to verify Wöhler's observations. He had never been able to render his urine alkaline by the consumption of considerable quantities of salts of vegetable acids.

Speaking of the cylindrical casts of tubuli uriniferi found in the urine, Dr. Höfle conjectures that these are of two kinds, having, like previous observers, discovered them in other diseases than albuminuria. He found them on one occasion, during two days, in the slightly albuminous urine of a patient labouring under pneumonia, who afterwards fully recovered. They were accompanied by a great quantity of epithelium-scales, and themselves appeared to consist of nothing else. On the other hand, the cylindrical tubuli found in Bright's disease are fibrinous exudations.

The sections in which questions connected with the urine and the

various concretions are discussed, present but little novelty. We may quote, however, one original observation, the subject of which was a præputial concretion, which appeared to have been derived from the smegma præputii. In a student, the subject of congenital phimosis, there appeared between the prepuce and glans more than a dozen of flexible, platter-shaped concretions, 3-4''' long, 2-3''' broad, and ¼''' thick.

"I received," says Dr. Höfle, "two of these concretions from my colleague, Alt, for investigation. When macerated in water, they appeared to be composed of several concentric, membraniform laminæ, which included an amorphous, crumbling mass. The membraniform laminæ presented all the physical and chemical properties of the epidermis. In the fluid in which the concretion had been macerated, neither boiling nor by the addition of nitric acid, nor ferrocyanide of potassium (with acetic acid), indicated the existence of a protein-compound. On the other hand, hot alcohol extracted from an unmacerated portion of the concretion a glittering white fat (margarine and cholesterine), which, under the microscope, appeared partly in tufts, partly in plates, and the residue left, on calcination, an inconsiderable white alkaline ash. To another portion of the concretion, the test for uric acid (nitric acid and ammonia) was applied, but with a negative result." (p. 483.)

On the whole, we consider that Dr. Höfle's book answers pretty completely the end for which it was written. It exhibits a tolerable summary of the present state of knowledge on the subjects of which it treats. It makes little pretension to originality. Dr. Höfle does not possess the progressive mind, which delights to wander, centuries in advance, through the uncleared forests of primæval ignorance, happy if only, on its return, it can exhibit a handful of gold-dust, collected from the sands of the mountain stream. Dr. Höfle is rather retrogressive; he would prefer to haunt familiar places,—to complete and to embellish property acquired,—to establish firmly this point, still somewhat uncertain,—to demolish that edifice, hastily constructed,—to execute repairs neatly and expeditiously. Dr. Höfle is a chemical sceptic; and, indeed, never was there greater need for scepticism than at the present day, when, encouraged by the brilliant success of the regular army in the new territory of organic chemistry, a thousand marauders have transported their arms thither, and issue from it each day their lying bulletins of victories achieved and territories conquered.

Art. XI.

The Dodo and its Kindred; or the History, Affinities, and Osteology of the Dodo, Solitaire, and other extinct Birds of the Islands Mauritius, Rodriguez, and Bourbon. By H. E. Strickland, M.A., F.G.S., President of the Ashmolean Society, &c.; and A. G. Melville, M.D., M.R.C.S. With Eighteen Plates.—*London,* 1848. 4to, pp. 142.

The handsome quarto which has recently come forth from the press of Messrs. Reeve, devoted to the collection and scientific exposition of all the information respecting the Dodo and its allies, is the very *beau ideal* of a complete and well got-up monograph; and we have great pleasure in laying before our readers a brief account of its contents. There are none of them, we trust, who need to be informed that a peculiar interest invests

everything which relates to the dodo, in the eyes alike of the zoologist, the palæontologist, and the natural-history *littérateur*. The dodo, first made known to Europeans by the Dutch voyager Van Neck, who visited the Mauritius in 1598, appears to have been *entirely extirpated* within a century from that time; and some naturalists have even doubted, as fabulous, the accounts which were given by Van Neck and his successors of this strange-looking bird. The conservation, however, of a head and foot in the Ashmolean Museum at Oxford, of a foot in the British Museum, and of a head in the Museum at Copenhagen, belonging to no known existing bird, and corresponding in dimensions and in configuration with the most complete descriptions and figures given of the dodo by contemporary observers, are facts quite sufficient to remove all doubt on this score; but whilst doing so, they open up another most interesting question, namely, the zoological position of this anomalous, and by some supposed to be fictitious, creation,—a question on which several of the greatest zoologists of the day, such as De Blainville, Gray, and Owen, have not thought it beneath them to exert their highest abilities. By the palæontologist, the dodo has been treated as one of those links which connect the present with the past inhabitants of the globe; the entire extinction of the species, almost within our own time, having been dwelt on as a type of that earlier disappearance of particular organic forms, which the study of the succession of life in the various epochs of the earth's history is continually forcing upon our attention. And the literary naturalist, who delights in hunting through the records made by elder observers, and who hails the discovery of a new source of antiquarian information as a collector welcomes a new species, finds in the search after materials for the history of the dodo one of the most attractive objects that can engage his attention.

Combining all these characters within himself, in a degree rarely equalled, it was impossible that so accomplished an ornithologist as Mr. Strickland should be indifferent to the various questions remaining for solution in regard to this remarkable bird; and we gather from various indications that the subject has occupied his attention for a considerable period. The existence in the museum connected with his own university, of the most perfect relics which time and ignorance, the ruthless foes of science, have left to us, may perhaps have added zest to his labours; at any rate, he was thereby enabled to carry his investigations to a point which we believe that scarcely any one else could have attained. For, through his influence (we believe), the curators of the Ashmolean Museum permitted the skin, &c., to be removed from one side of the dodo's head, so as to admit of a minute examination of its osteology; and a similar inspection having been formerly made of the foot, under the direction of Professor Kidd, all the information has now been obtained which the existing relics seem able to furnish. A very elaborate anatomical description of the bones of the head and foot has been supplied to Mr. Strickland by Dr. A. G. Melville, a young anatomist of high promise; and these bones are delineated in what we can unhesitatingly designate as the most perfect osteological lithography we have ever seen.

Having said thus much by way of preface, we may now give a general summary of the results of Mr. Strickland's investigations. In the first place, he has collected and compared all the *original* accounts which he has been able to ferret out, of the great brevi-pennate bird inhabiting the

Mauritius; he has shown that these are so perfectly conformable with each other, as to leave (when the independence of the testimony is considered) no reasonable doubt of their general accuracy; and he has also accounted satisfactorily for the complete disappearance of the bird soon after the colonization of the island, so that the present inhabitants have not even a traditional knowledge of it, except such as has been imported from Europe. The narratives of the early voyagers are in several instances accompanied by rude delineations of dodos, some of them most amusingly quaint; but besides these, we possess certain oil-paintings of this bird, by artists of great merit, who apparently aimed only at correctly representing the object before them. The best known of these paintings is that which is preserved in the British Museum; unfortunately it has neither name nor date; but there is strong internal as well as traditional evidence that it was drawn from the life, evidently by a superior artist. From its strong resemblance to other pictures of the dodo by Roland Savery, an eminent Dutch animal painter in the beginning of the 17th century, and by his nephew John, Mr. Strickland infers that it was executed by one of these artists. From this picture nearly all the current delineations of the dodo are copied, either directly or indirectly. The two most authentic pictures are contained in the Royal Gallery at Berlin and the Imperial Collection at Vienna; they are both the work of Roland Savery, and they bear the dates, the former of 1626, the latter of 1628. Beautiful coloured copies of these are given by Mr. Strickland.

With regard to the actual relics of the dodo, already enumerated, the question naturally occurs, why more complete specimens have not been preserved? The history of that contained in the Tradescant Museum (which seems to have been exhibited alive in London) may probably serve as a type of the rest. *Ex uno disce omnes:*

"The stuffed specimen of the dodo mentioned in the Catalogue of Tradescant's Museum, 1656, was bequeathed with the rest of his curiosities to Elias Ashmole, the munificent founder of the Ashmolean Museum at Oxford. Here it remained in an entire, if not a very perfect state, till 1755, when the Vice-Chancellor and the other trustees, to whose guardianship the worthy Ashmole had confided his treasures, came in an unlucky hour to make their annual visitation of the Museum. In those days, Oxford presented the still-existing anomaly of a university, in which zoology was not publicly taught as a science; the Royal Society had long removed to the metropolis, the Ashmolean Society was as yet unborn, and the Taylor Institution had not opened a door to continental literature. The literary and scientific ardour which Lister, Plott, Aubrey, Ashmole, Wood, Llhwyd, and others had awakened in the 17th century, had now subsided, and the university seems to have relapsed into the scholastic torpor of the middle ages. We need not wonder, therefore, at the fate which befel the LAST OF THE DODOS. The unhappy specimen, then at least a century old, had, it appears, become decayed by time and neglect; and according to a record now extant, was, with many others, 'ordered to be removed at a meeting of a majority of the visitors.' By a lucky accident, however, a small portion of this last descendant of an ancient race escaped the clutches of the destroyer. The head and one of the feet were saved from the flames, and are still preserved in the Ashmolean Museum." (p. 32.)

The data which we possess regarding the dodo would lead us to figure it to ourselves as a massive clumsy bird, ungraceful in its form, and with a slow waddling motion. "We cannot form a better idea of it," says Mr. Strickland, "than by imagining a young duck or gosling enlarged to

the dimensions of a swan." And this condition of "gigantic immaturity" seems to have prevailed through the whole plan of its organization; for it might be characterised in regard to its plumage and locomotive organs as a *permanent nestling;* being clothed with down instead of feathers, and having the wings and tail so short and feeble as to be utterly unsubservient to flight. We are not, however, hence authorised to conclude that the dodo was less perfectly adapted than any other animal to the conditions of its existence. On the contrary, the abundance of the species, when the Mauritius was first visited by man, affords ample proof that it was well fitted to maintain its place in the natural fauna of the island to which it was restricted, until a new set of enemies appeared on the scene,—namely, man, and the animals that followed in his train. Notwithstanding that the flesh of the dodo was somewhat tough, the facility with which it might be procured would doubtless recommend it to the early settlers; and the dogs, cats, and swine, which accompany man in his migrations, and are speedily naturalised in the forests, probably found a dainty treat in the eggs and young of this unfortunate bird. Wherever the human race extends itself, it exerts an important influence on the living inhabitants, whether animal or vegetable, of the countries over which it spreads; but it will be only when a species is peculiarly circumscribed as to its geographical distribution, that this influence will speedily lead to its entire extirpation. Mr. Strickland mentions certain other species of birds, which will probably ere long undergo the fate of the dodo, namely, the *Nestor productus*, a parrot originally from Phillip's Island, near Norfolk Island, where it is now destroyed, though a few individuals, which refuse to propagate, still survive in cages; the two (not improbably three) species of *Apteryx* in New Zealand, the puny representatives of the gigantic *Dinornis;* and the almost equally anomalous burrowing parrot, *Strigops habroptilus*, of New Zealand. That these birds should disappear before the inroads of human civilization cannot be regarded as a mark of imperfection of structure; and we should be equally unphilosophical in deducing any such inference from the early disappearance of the dodo. The crude and profane ideas entertained by Buffon and his disciples on such subjects, can only excite our smiles and our pity. Bory de St. Vincent speaks of the dodo as "un oiseau bizarre, dont toutes les parties portaient le caractère d'une conception manquée;" and fancies that this imperfection was the result of the youthful impatience of the newly-formed volcanic islands, implying that a steady old continent would have produced a much better article!

Having thus traced the general outlines of the dodo's eventful history, we pass on to notice the most original portion of Mr. Strickland's disquisition, namely, its place in ornithological classification; as to which he has adduced some new and most important considerations, which lead to a view different from almost any previously expressed. The large size of the dodo, and the shortness of its wings, have caused it, in spite of the anomalous character of its bill, to be usually referred to the *struthious* group, composed of the ostrich, cassowary, &c.; and with these it was associated by Cuvier. Mr. Vigors, however, was strongly impressed with its *gallinaceous* character, and regarded it as a link of connection between the struthionidæ, and the Linnæan genus *Crax*. By M. de Blainville, on the other hand, it was regarded as a *raptorial* bird, allied to the vultures; and this view was sanctioned by the authority of the eminent ornithologist, Mr.

Gould, who formed his opinion chiefly from the compression of the beak, and the nudity of the face; and by the anatomical comparisons instituted by Professor Owen between the foot of the dodo dissected by Dr. Kidd, and the feet of raptorial birds on the one hand, and of gallinaceous species on the other. It was evident, however, says Mr. Strickland, "from the many counter-arguments, which both De Blainville and Owen with great impartiality adduced, that their conclusions as to the raptorial affinities of the dodo, are far from being absolutely demonstrated. If there are objections to the gallinaceous hypothesis, there are at least as many to the raptorial one; and the systematic zoologist finds no more satisfaction in the one conclusion than in the other. If, however, we look a little further into the field of ornithic creation, we shall find a family of birds ready to claim relationship with this pedestrian outcast, and to admit him among their kindred." The family referred to is that of *Columbidæ*, or pigeons, a group very isolated in its own position; for though it has characters in common, on the one hand, with the *insessores*, or perching birds, and on the other with the *gallinaceous* order, it cannot be properly referred to either. We find in this group some genera that live wholly in trees, and others which are entirely terrestrial; while the majority, of which the common wood-pigeon is an example, combine both these modes of life. They present much diversity in the forms of their beaks, according to the size and mechanical structure of the seeds on which each genus is destined to live; and there is a genus of tropical pigeons, termed *Trerons*, in which the beak is much stouter than in other pigeons, the horny portion being strongly arched and compressed, so as (like that of the dodo) greatly to resemble the structure of the beak of certain rapacious birds, especially of the vulturine family. This peculiarity, apparently connected with the nature of their food, which consists of the large fruits and berries of various kinds of palms, figs, nutmeg, &c., is carried to its greatest extent in the genus *Didunculus*, a very singular bird of the Samoan islands in the Pacific, which received its generic name from the resemblance which it bore, in the eyes of its first discoverer, to the comparatively gigantic dodo.

The idea of referring the dodo to the neighbourhood of the pigeons originated with Professor J. T. Reinhardt of Copenhagen, the discoverer of the imperfect cranium which lay hid in the Gottorf Museum; who had pointed out to many Swedish and Danish naturalists, previously to leaving Denmark in 1845, on a voyage round the world, "the striking affinity which exists between this extinct bird and the pigeons, especially the trerons." This view was adopted by Mr. Strickland, who succeeded in bringing round several eminent naturalists to his opinion; and at the meeting of the British Association at Oxford in 1847, he formally laid the subject before the zoologists and comparative anatomists there assembled, his position (which chiefly rested on external characters) being strengthened by the minute osteological comparisons instituted by Dr. Melville between the remains of the dodo, and the corresponding parts of various columbidæ. As no objection was raised on that occasion by any of the numerous continental and British naturalists then present, to the new view thus expounded to them, we presume that the question may now be considered as settled as to all its essential points; and that the dodo is henceforth to take rank as the "colossal, brevipennate, frugivorous

PIGEON." No comparative anatomist, we think, can continue to entertain a doubt on the subject, after perusing the elaborate descriptions of Dr. Melville; indeed the mere inspection of the figures, in which the corresponding parts of the dodo, didunculus, and other columbidæ are brought into proximity, might be sufficient to convince any unprejudiced observer.

But in thus establishing the true place of the dodo in the zoological series, Mr. Strickland has not by any means accomplished his whole purpose; for he has essayed to prove that the dodo was only one of *several* species of birds, constructed upon the same general plan, which inhabited the little group of volcanic islands to the east of Madagascar. It appears from the narrative of François Leguat, the commander of a party of French protestant refugees who settled upon the island of Rodriguez (about three hundred miles to the east of the Mauritius) in 1691, that he found there a large apterous bird, which he named the *solitaire*, from the circumstance of its being "seldom seen in company, though there are abundance of them." He gives a tolerably full description of its external aspect, and this was accompanied by a figure, which at once shows that the solitaire was a very different bird from the dodo; the accuracy of this figure is attested by the fact, that in a landscape which forms the frontispiece to Leguat's work, and in two maps which accompany it, no fewer than twenty-eight small figures of solitaires are introduced, all of which very closely correspond with the principal figure.

From the description and figure of Leguat, we might perhaps refer the solitaire to the struthious family, rather than regard it a congener of the dodo. The legs and neck appear to have been longer, the beak shorter, and the wings, though useless for flight, somewhat more developed than in the latter bird. The short arched beak, and the defensive structure of the wings, would remind us in no slight degree of the cassowary. By many, however, the description and figure of Leguat were considered more or less apocryphal; and among those who were disposed to allow them some foundation in truth, it seems to have been generally imagined that they were distorted representations of the dodo. Distinct osteological evidence is furnished, however, by existing remains, that the solitary of Rodriguez was not the dodo, but a bird nearly allied to it, and decidedly not struthious. We shall not detail the several discoveries of ornithic remains which have been made from time to time in that island; nor the strange mischances that have befallen the precious relics when transmitted to Europe. Suffice it to say that two sets of these bones are at present in existence; one of them in the Andersonian Museum at Glasgow; the other in the Museum of Natural History at Paris: these Mr. Strickland has been enabled to compare with each other, and with the remains of the dodo at Oxford. The Paris bones consist of five, viz., a femur, a tarso-metatarsal, a humerus, the medial portion of a sternum, and a portion of the cranium. Unfortunately they are incrusted uniformly over with stalagmitic deposit, which prevents all examination of the surface of the bones, or any minute description of their structure. They nevertheless supply us with several important elements to guide us in reconstructing the skeleton of this lost bird. The Glasgow series of bones consists of three femora, a tibia, and two tarso-metatarsal bones. On comparing those which are common to both sets, there is every indi-

cation that the two collections belong to one and the same species of bird; and as we know that they were all brought from the small island of Rodriguez, where no bird now exists to which they can be referred, we have a right to assume that they belong to the extinct species described and figured by Leguat as the solitaire. On comparing the bones of the lower extremity with those of the dodo, it becomes evident that with such a difference in proportions as corresponds with the difference in the figures and descriptions of the two birds, the characters which especially distinguish the dodo are repeated in the solitaire; these characters are shared by both with the pigeons, and exist in no other known families of birds. Unfortunately the cranium of the supposed solitaire is very imperfect, and the anterior portion is entirely wanting. With such incomplete data, it may, therefore, appear premature to assert the generic distinction of these two birds. But, as Mr. Strickland justly remarks, it seems certain that, from the greater length of the legs, and the less development of the beak, as indicated by Leguat, the dodo and the solitaire would be classed (according to the present standard of zoological characters) in two distinct genera.

"There is only one remarkable character in the skeleton of the solitaire, which seems opposed to the supposition that it belongs to a brevipennate bird. In ordinary birds the power of flying requires great size and strength in the pectoral muscles, and a largely developed keel upon the sternum for their insertion. But in the ostriches, where the wings are short and feeble, the pectoral muscles are exceedingly small, and the sternum is destitute of a medial keel. Now in the sternum of the solitaire we find a considerably developed keel, such as would almost indicate volatile powers. The shortness of the humerus, however, no less than the positive testimony of Leguat, prove that the bird was wholly unable to rise from the ground. The presence of a sternal keel would therefore appear anomalous, were it not for a circumstance mentioned by Leguat, namely, that the bird used its wings for self-defence, and was able to inflict considerable blows with these members, for which end a corresponding strength of the pectoral muscles, and enlargement of the sternal keel would be required. It is, moreover, evident from the figures handed down to us, both of the dodo and the solitaire, that the wings of these birds, though too short for flight, were yet considerably more developed both in size and structure than is the case in the struthionidæ."

For having thus identified the bones brought from Rodriguez with the description and figure of Leguat, and determined their relation to the dodo, Mr. Strickland deserves, and we doubt not will receive, the cordial thanks of all who are interested in this department of zoology.

It is not easy to assign the exact period at which the solitaire became extinct. Leguat's residence at Rodriguez continued only for two years; and he seems to have left the island as destitute of human inhabitants as he found it. The present inhabitants have lost all traditions of the bird's existence, except such as are founded upon the bones which have been occasionally dug up in caves; and it seems not improbable that the extermination of the species may have taken place before the island was regularly inhabited, through the agency of the cats which were left by the Dutch (who first landed at Rodriguez) to destroy the rats which annoyed them,—the young birds having been devoured by these cats as soon as they were hatched.

But it would seem that the island which forms the third of this interesting group, was not without its didine birds; for strengthened in his confidence in the trustworthiness of the early voyagers by the verification of Leguat's account, Mr. Strickland ventures to affirm that Bourbon also

is proved by indubitable evidence to have been inhabited by two species of birds, whose inability to fly, and their consequent rapid extinction, brings them into the same category with the dodo of Mauritius and the solitaire of Rodriguez. One of these was a brevipennate bird called the solitaire, whose white or light-yellow plumage and woodcock-like beak prove it to have been distinct from the dodo of Mauritius, and from the solitaire of Rodriguez. And the second brevipennate species was distinguished by its blue colour and its speed in running. There is evidence that one, at least, of these singular birds continued to inhabit Bourbon until nearly the middle of the last century; a specimen having been sent home by M. de la Bourdonnaye, who was governor of the isles of France and Bourbon from 1735 to 1746. No collateral evidence at present exists, of a nature to confirm the testimony of the early voyagers. No delineations of these creatures appear to be now extant; and their osseous remains have never yet been properly sought for, and have consequently never yet been found. The precise affinities, therefore, of these extinct species cannot be determined. We naturally look to the little-known island of Madagascar, as the region most likely to contain birds allied by affinity to those of Bourbon; and though no recent travellers have alluded to the existence of any struthious or brevipennate birds in Madagascar, it appears from a description of that island published nearly two centuries since, that a bird somewhat resembling an ostrich, and peculiarly shy in its habits, then frequented the region of Ampatres, a province at the south extremity of Madagascar. "This brief indication," remarks Mr. Strickland, "may perhaps guide the future explorer of Madagascar to a discovery of great zoological interest."

In concluding this notice of Mr. Strickland's researches, we cannot refrain from cordially thanking him for the zeal and liberality which he has displayed in the publication of this beautiful volume, which does the highest credit to all concerned in its production. Nor must we omit our acknowledgments to his able coadjutor, Dr. Melville, who has left nothing to desire in the anatomical descriptions and comparisons which he has contributed to the work. We would hint to him, however, that in expressing his difference of opinion from Professor Owen as to the philosophical interpretation of certain parts of the ornithic cranium (p. 87), he uses language scarcely respectful enough towards one who has done so much for comparative anatomy, and who has so profound an acquaintance with the vertebrate structure, as must be conceded by every one to the distinguished author of the Report on the "Archetype Skeleton." We are far from wishing to discourage independence of thought and expression; but when a young man is making almost his first appearance before the public, it is scarcely prudent in him—to say the least—to speak in terms of flat contradiction, in regard to the decisions of one who ranks as *facilè princeps* in this department.

We return, however, to Mr. Strickland; with whose summary we shall bring our analysis to a close.

"On a review of the various historical and osteological evidences which I have now brought together, it seems sufficiently clear that the three oceanic islands, Mauritius, Rodriguez, and Bourbon, which, though somewhat remote from each other, may be considered as forming one geographical group, were inhabited, until

the time of their human colonization, by *at least four* distinct, but probably allied species of brevipennate birds. This result at once reminds us of the analogous case of the New Zealand group of islands, where the scientific zeal of Messrs. Cotton, Williams, Colenso, Mantell, and others, has brought to light a mine of osteological treasures, from which the consummate sagacity of Professor Owen has re-constructed two new genera of brevipennate birds. Seven species of *dinornis* and two of *palapteryx* have been clearly established and elaborately described by Professor Owen; while in the still surviving genus apteryx, of which Mr. Gould has very recently described a *second* species, we see an almost expiring member of the same zoological group.

"The extraordinary success of the naturalists of New Zealand, in procuring from recent alluvial deposits a series of osseous remains which have more than doubled the number of struthioid birds previously known, should encourage the scientific residents in the islands of the Indo-African sea to make similar researches. I feel confident that if an active naturalist would make a series of excavations in the alluvial deposits, in the beds of streams, and amid the ruins of old habitations in Mauritius, Bourbon, and Rodriguez, he would speedily discover remains of the *dodo,* the two '*solitaires,*' or the '*oiseau bleu.*' But I would especially direct the attention to the caves with which those volcanic islands abound. The chief agents in the destruction of the brevipennate birds were probably the runaway negroes, who for many years infested the primæval forests of those islands, and inhabited the caverns, where they would doubtless leave the scattered bones of the animals on which they fed. Here, then, may we more especially hope to find the osseous remains of these remarkable animals.

"Should any copies of this work find their way to Mauritius or Bourbon, they may perhaps incite the lovers of knowledge in those islands to investigate further the subject which has been diligently but imperfectly pursued in this volume. And I shall feel rewarded for the trouble it has cost, if my researches into the history and organization of these birds, aided by the anatomical investigations which Dr. Melville has introduced into the second part of the work, shall have rescued these anomalous creatures from the domain of fiction, and established their true rank in the scheme of Creation."

We trust that our own diffusion of the information collected by Mr. Strickland, may contribute towards procuring him that which he still desiderates.

Art. XII.

The Philosophy of Animated Nature; or the Laws and Action of the Nervous System. By G. Calvert Holland, M.D., Physician Extraordinary to the Sheffield General Infirmary.—*London,* 1848. 8vo, pp. 512.

The inconsistency between the first and the second parts of the title of this book must be apparent to every one who knows anything of the present aspect of physiological science. "The Philosophy of Animated Nature" would lead us to expect a disquisition on those phenomena which are common to all living beings, some general views of their mutual connexions, and of their relations with the inorganic universe. This we soon find, however, to be far from the author's intention; for we are told immediately afterwards that *his* idea of this philosophy is restricted to "the laws and action of the nervous system;" in other words, that all the manifestations of life are in some way or other the results of its operation. Nor is this

notion contradicted in the treatise itself; for although, perhaps, it is nowhere formally stated, we find numerous hints that all the organic functions of animals are as dependent for their continuance upon a constant supply of nervous fluid, as they are upon the circulation of the blood: and that there is a vast department of "animated nature," in which these organic functions go on with the greatest energy and constancy, and in which there is no reason whatever to impute them to nervous agency, seems altogether to have escaped Dr. G. C. Holland's consideration.

We have been truly grieved to find a man of the author's standing as a sound practical physician quitting, from time to time, the path in which he is best qualified to make progress, for the sake of giving to the world his lucubrations on certain questions of abstract science, with which no one ought to grapple who has not time and mental calibre to enable him to master the whole subject. We should have thought that the small amount of attention given by the profession to Dr. G. C. Holland's previous physiological treatises, might have given him a significant hint that his vocation did not lie in this direction. But there are some men who are callous to any indications of this kind, perseveringly imputing their want of success in authorship rather to the bad taste of the public than to their own demerits; and of these our author seems to be one.

It would be a waste of our readers' time and our own, for us to inflict upon them any detailed account of the production before us; which, with a few changes in phraseology, and the omission of certain quotations from modern authors, might be very well dated back 100 or 150 years. For its purpose is to uphold and establish the doctrine current at that period, of a circulation of "nervous fluid,"—the "animal spirits" of the older authors,—the brain standing to it very much in the relation of the heart to the sanguiferous circulation, and the nervous trunks, like the arteries and veins, conveying it between that centre and the parts to which it is distributed. This "nervous fluid" is thought by Dr. Calvert Holland to be probably identical with electricity; but he does not furnish any more satisfactory grounds for the belief than those advanced by his predecessors, and contents himself with disputing the validity of the objections raised to the doctrine by Müller and other physiologists. Of Matteucci's experimental investigations he does not seem to have heard, for he makes not the slightest reference to them. This circulation of nervous fluid accounts in the most convenient manner for every vital phenomenon; like the "vital principle," it is always at hand when wanted, to solve every physiological problem; but the solution is more like the cutting, than the patient disentanglement, of the Gordian knot, and leaves us no wiser than it found us,—in fact, really less wise, because it has deceived us into the idea of our having gained a step, when we have in reality lost our clue.

We shall best give our readers an idea of Dr. G. C. Holland's manner of treating his subject, and at the same time exhibit his doctrines in the manner most just to himself, by giving a few extracts which most characteristically present them. We have searched in vain for any formal exposition of his peculiar tenets; they are everywhere implied, but nowhere distinctly expressed. The following is the clearest and most concise account of them which we have been able to find:

"Whatever sense is excited, the nervous principle, in consequence of which it acts, is increased. In vision it flows from within, outwards, not only in the

direction of the visual organs, but towards every portion of the human countenance, producing an expression in harmony with the predominant feelings of the mind.

"That phenomena result, which may be explained on this view, will not be doubted; and we shall prove in the following pages, that it is in this manner only that numerous important facts in connexion with the nervous system admit of a satisfactory elucidation. To deny the transmission of a something along the nerves, which is capable of accumulation and dispersion, is to involve the disputant in difficulties and absurdities.

"We do not imagine that the nervous principle is increased by immediate production, when the organ in which it exists is suddenly aroused to powerful action. This principle will flow where it is required, and the knowledge of this circumstance is fraught with the highest interest to the physiologist. Though rich in speculative materials, it abounds far more in truths of a practical nature.

"It is a law of the animal system, that every predominant action draws to itself the vital energies from parts less excited. We observe this in regard to the circulation, and the fact, though less obvious to ordinary observation with respect to the nervous system, may be equally well established by profound investigations. The phenomena which crowd upon the mind in illustration of it, perplex from their variety.

"We will briefly allude to the following:—It is well known that suckling, in a number of instances, sufficiently great to make it the rule, is a bar to conception. How does this arise? Does the fact admit of no easy explanation? It is a law of the animal economy that two inordinate vital actions rarely coexist in health. The sympathy between the mammæ and the uterus is exemplified at every epoch of female life. In pregnancy the breasts become sensitive, and are gradually enlarged. After birth, the copious secretion of milk is at the expense of two powers—the blood and the nervous energy.

"The excited action of the mammæ gives a tendency to both to flow in a liberal current in such direction, and this accordingly will interfere with what might otherwise have been in part distributed to the uterus; hence the comparatively rare occurrence of conception under such circumstances.

"The inferior animals occasionally neglect their young, from the premature return of the amative propensity. The maternal feeling yields to the sensual desire. Each has unquestionably its particular cerebral organ; and the one, which has again become active, withdraws from the other the nervous principle essential to its operation.

"This, as well as conception during lactation, are exceptions to the rule; nevertheless, they throw light on the changes of the nervous system, and furnish facts in elucidation of phenomena that would otherwise be inexplicable.

"These views are further illustrated by the results flowing from the action of the mental faculties. When the mind is engaged in intellectual pursuits—absorbed in abstruse inquiries, which make a demand on the entire energies of the brain, there is a corresponding diminution in the sensual feelings, and equally so in the ability of the organic powers to perform their respective functions. There is no mystery in these changes. The activity of the intellectual faculties can be maintained only by a large supply of the nervous principle, and this can be furnished only at the expense of the rest of the nervous system.

"It is precisely with this principle as it is with the blood. They are both equally susceptible of modifications in their direction. They will flow wherever an excited action exists in quantity proportionate to the demand. If towards the sensual organs of the brain, and especially if these are gratified in harmony with the strength of the feelings, the intellectual will in the same ratio decline. They will become unequal to any vigorous effort of the understanding." (pp. 61-4.)

Our author considers that his views are incontrovertibly established by the phenomena of mesmerism, his belief in which extends as far as the

excitement of the phrenological organs,—how much farther he does not tell us. He informs us that the facts which he brings forward are the results of his own personal investigation, aided by others of high intellectual attainments, men who commenced the inquiry, actuated solely by the desire of discovering truth. The subject, we are assured, was tested vigorously in all its bearings; no word was breathed during the experiments to suggest the anticipated effect, even had the individual been conscious; and numerous other precautions were used, which as completely shut out the possibility of imposition as if the person operated on had been a statue. "We laboured," continues our author, "not to establish a doctrine, but to ascertain the foundation, if any, on which it rests. From sceptics we became believers. And, if the present work possess any merit or originality, it is owing to the phenomena which mesmerism disclosed—the wondrous insight which it gave into the actions of the nervous system." We apprehend that our readers will need some better testimony than Dr. G. C. Holland's, before they commit themselves to a belief in phreno-mesmerism. It is quite evident that the marvels which he saw fitted-in too well with his preconceptions, to render him a very impartial observer; and he must forgive us for remarking that his whole cast of mind, as revealed in his writings, seems to us unfavorable for the purposes of that rigid and penetrating scrutiny, which (as every one knows who has ever taken part in the inquiry) phenomena of this class preeminently require. —The application of phreno-mesmerism to the proof of the doctrine of the circulation of nervous fluid, is set forth in the following extract:

"That the brain is a congeries of organs exercising independent functions, is proved by mesmerism, and the evidence establishes the existence of a nervous principle. If we have the power of awakening any cerebral organ to the manifestation of its function, drawing out its natural expression while the individual is wrapt in unconsciousness, in what manner shall we explain the phenomenon? It admits of one solution only. The principle which animates the nervous system exists equally in the two parties—the mesmerized and the operator. In the latter, whether concentrated by a mental effort, or otherwise, it has the power, through the medium of touch, of at once exciting the organ to which it is directed. If this be the seat of destructiveness—of veneration—of benevolence, or of any other mental quality, the effect is invariably in strict correspondence with the doctrine of phrenology.*

"The manifestation of the faculty is instantaneous, and in some cases the organ may be roused to a degree, of which we have no parallel except in insanity.

"If destructiveness or combativeness is excited, the result is occasionally appalling. But in the midst of the induced madness, the same touch that has given rise to the most violent of passions, can as quickly calm the troubled scene, by directing its influence to an organ, the seat of a different faculty. The change from the worst expression of our nature, to that which is the most beneficent and ennobling, passes with the rapidity of thought.

"These facts corroborate, in an extraordinary manner, the remarks we have already made in reference to the animating principle of the nervous system. We have stated, that it is susceptible of increase and diminution, and, what naturally

* "We remember one case, which may perhaps be regarded as an exception to this general rule. In touching firmness, which was very largely developed in the head of a female, we invariably excited combative qualities to a most extraordinary extent. The organ after a time became so exceedingly sensitive, that if we offered to touch it, when she was conscious, she begged we would not, as she could not avoid striking, and such was indeed the fact. The force and suddenness of the blow, and the straight line in which it was delivered, were indeed remarkable. The functions of firmness and combativeness are nearly related, and are often associated in action."

follows from this doctrine, that an organ is not endowed with a fixed quantity, in virtue of which it acts, but that the quantity varies, and with it the power of the organ. Mesmerism confirms these important deductions. When we touch benevolence, the violence of destructiveness at once subsides; or if we arouse the latter, the former as quickly disappears. We can rarely keep in simultaneous and vigorous operation a series of organs, if these are large and influential, except they are organs in the same region, and frequently associated in action; time, tune, and language are often observed in combination with some leading faculty." (pp. 155-7.)

Now we will undertake to prove, by facts of this kind, that combativeness exists in the fist, and gloomy ill temper in the corrugator supercilii; for we have seen Mr. Braid, over and over again, excite the most violent combative propensity by gently doubling the fingers of his hypnotised subjects, change their mirth into melancholy by drawing the eyebrows downwards and towards each other, and restore the hilarious state by separating the corners of the mouth. We have seen, too, an hysterical female thrown into violent convulsive paroxysms by the simple act of pointing at her great toe. All such phenomena are, of course, as easily explicable as those of phreno-mesmerism on the hypothesis of a circulation of nervous fluid; but we submit that they place the latter in a light rather different from that in which Dr. Holland views them. Let us, however, again hear him:

"There are other phenomena to which we have not yet adverted, viz. the attractive and repelling power of the operator. He has a perfect control of the whole body. He can either draw forward the head or cause it to recede, and the same with the trunk and the extremities. He can compel the individual to move in any direction, unconscious of all external impressions. He can induce complete rigidity in the limbs, which no ordinary force can overcome.

"We have often tested the attractive power in persons awake, and when unconscious, by placing a weight upon the arm, and drawing the limb in a rigid state in the direction of our own fingers, at the distance of seven or eight inches. The attraction was able to raise nearly a stone and a half in weight with perfect ease; and if the experiment was performed on the mesmerised arm of one awake, the weight was not at all felt.

"Do not such facts establish the existence of a nervous principle? On what other view is it possible to explain them? They are as unequivocal in their indications as the influence of the magnet on the needle, and we may with as much propriety question the reality of the one as the other." (pp. 163-4.)

We can beat this too; for we have seen Mr. Braid drag a sensible gentleman, who was wide awake all the time, round and round the room, by the adhesion of his finger to a magnet,—a fact for which Dr. G. C. Holland is perfectly welcome to quote our authority in corroboration of his hypothesis. Other classes of mesmeric phenomena are explained by it with equal facility.

"The mesmeric influence not only occasionally imparts a high degree of acuteness to the sense of hearing and smell, but also to the touch, by which objects are discriminated, that would otherwise not have been perceived. There is only one mode of explaining these phenomena; viz. *the modifications in the distribution of the nervous principle.* By the aid of these all is simplicity and order. The natural, as well as the marvellous, manifestations of the nervous system are brought within the department of strict physical investigations, and are involved in no mystery, but what belongs equally to all objects in nature." (pp. 178-9.)

But phenomena of quite a different class equally bear testimony, in his opinion, to the correctness of his doctrine.

"It is well known, that severe pain gradually exhausts the vital energies, and causes death. On this point there is no difference of opinion.

"How does pain give rise to these effects? It is not to be ascribed to the mere mental sensation, but to physical changes induced in the nervous system. When an immense focus of irritation is set up, the nervous principle rushes in such direction, and hence it becomes a source of expenditure incompatible with the well-being of the animal economy. It disturbs the balance of the circulating nervous fluid. The accumulation of it, under these circumstances, is at the expense of the rest of the vital powers. It affects these in a manner somewhat analogous to long-continued muscular exertion.

"We may regard the nervous system as possessing a certain capital of the animating principle, and which, in health, is distributed in various proportions to the different organs of the body, subject only to those changes which the daily diversified activity of their functions require.

"Therefore, when disease exists, accompanied by severe pain, the nervous fluid, as in muscular exercise, flows to the seat of the exciting cause, in quantity according to its severity, and in such proportion diminishes the amount that would be distributed to different parts of the animal economy. The mental sensation, independently of this effect, will have a prejudicial influence." (pp. 180-1.)

It is surely unnecessary for us to devote any further space to extracts of this kind. The doctrine of a circulation of nervous fluid will never, we venture to say, be admitted by men of really philosophic habits of thought, until it shall have been demonstrated by evidence of a very different kind from that which is here adduced in its behalf. The author seems to be so little acquainted with the present aspect of physical science, as to be unaware that, although the terms "electric fluid," "galvanic fluid," "magnetic fluid," &c., are employed in common parlance, and although these fluids are often said to travel along conducting wires, and to circulate between remote points, they are no longer quoted in really philosophic discussions; it having at last come to be perceived that there is no proof of their existence as distinct *entities*, and that the assumption stands very much in the way of that rigorous analysis of the phenomena, which is the necessary preliminary to the process of scientific induction. The darkness in which his own mind is involved, causes him to mistake a misty undefined glimmer for a luminous view of his subject; and those who, discarding theory, are content to collect and generalize facts, are spoken of by him as "abhorring the broad path of scientific inquiry, rarely grasping a principle in its comprehensive relations." In this category he includes Dr. Marshall Hall; of whom he asserts,(p. 251) that "the field which he surveys is always exceedingly limited, and he never rises from particulars to enlarged philosophical views." When Dr. G. Calvert Holland shall have himself made any original contribution to our definite knowledge of the phenomena or laws of the nervous system, he will be more entitled to speak as he thinks proper of the labours of one who has done more than any other living physiologist for the advance of neurological science.

Art. XIII.

1. *Popular Lectures on the prevailing Diseases of Towns: their Effects, Causes, and the Means of Prevention; recently delivered at the Brighton Literary and Scientific Institution. Published by general request.* By William Kebbell, M.D., Physician to the Brighton Dispensary. With Fifteen Woodcuts.—*London,* 1848. 12mo, pp. 196.
2. *Sanitary Ramblings. Being Sketches and Illustrations of Bethnal Green; a Type of the Condition of the Metropolis and other Large Towns.* By Hector Gavin, M.D., F.R.C.S.E., &c. &c. With Maps, Tables, &c.—*London,* 1848. 8vo, pp. 118.
3. *A Report of the Sanatory Condition of the Borough of Bolton, in a Letter addressed to T. R. Bridson, Esq., Mayor.* By John Entwisle. —*Bolton,* 1848. 8vo, pp. 91.

Sanitary publications are now "as plenty as blackberries;" and yet we cannot desire, by even hinting at a redundancy, to interpose the least check to the onward advance of the stream of public opinion. These publications, of course, have much in common. They all advocate the same principles, and have to tell of the same results of neglect or violation of them. The same fearful array is everywhere presented; and the same promises are held out of benefits to be derived from improvements in ventilation, drainage, supply of water, &c. &c. But there are usually some local details which render them acceptable to a particular class of persons, and which procure attention to the more general statements from many who would otherwise treat them with careless disregard. The multiplication of these publications, therefore, has the effect of extending the number of those who are led to feel an interest in the subject, and must consequently do much good. But the good is not altogether unmixed; for if the expectations of beneficial results from sanitary reform are too highly raised, they will be most assuredly and most grievously disappointed. If we are to believe some of the enthusiasts in the cause, there is scarcely a physical or moral evil which is not in some way or other traceable to deficient sewerage, foul air, and want of water; and which is not therefore to be eradicated, by the establishment of water-closets, ventilation, and aqua pura. Not only are the sickness and mortality of our great towns to be brought down to the average of rural districts,—an end whose attainment we regard as quite feasible,—but drunkenness is to be extirpated, debauchery to be extinguished, education to spring up and flourish, employment is to be constant, and provisions cheap; in a word, St. Giles's and Bethnal Green, Whitechapel and Lambeth, are to become what Sam Slick terms his good town of Slickville an "airthly paradise." Now we fully recognise the bad sanitary condition of our great towns as *one* of the causes of the depraved moral state of the great mass of our labouring population; but it is no more to be accounted the *sole* cause, than are want of education, uncertain or deficient employment, and half a dozen others that could be named, each of which has at least as close a connexion with the deplorable results. Consequently, it is absurd to look for all these grand improvements from the removal of one single cause or set of causes of the present degradation; and nothing,

save the simultaneous operation of other measures of elevation, will produce effects at all satisfactory to those, who have based their hopes upon such one-sided and imperfect representations.

We should be wrong in saying, however, that either of the publications before us errs on this head. Dr. Kebbell's Lectures contain a very sensible and temperate exposition of the subject, with especial reference to the sanitary condition of Brighton. This is very far from what it should be; for, although few large towns are more advantageously situated as regards facilities for drainage, Dr. Kebble states that, whilst the total length of streets having a common sewer is 6½ miles, the length of those altogether undrained is 26 miles. He dwells also on the abominations of slaughter-houses and pig-sties, and states from his own knowledge, that these are the *foci* of infectious fevers. Few save those who are personally familiar with "back-slums" of our large towns, have any idea of the extent to which the practice of keeping pigs prevails amongst the poor, or of the degree in which the other evils of their condition are aggravated by it, without any corresponding benefit,—it being an established fact, that the inhabitants of towns, who have no refuse garden-stuff, &c. on which to feed their pigs, lose rather than gain by their maintenance. The Sanitary Committee at Sunderland complained that, during the prevalence of the cholera, the places most abounding in offensive pig-sties were the spots most visited by that scourge.

Those who are aware of the "bad eminence" which the Bethnal-Green district holds, amongst the metropolitan districts, in the scale of unhealthiness, will be at no loss to understand why Dr. Gavin chose it as the scene of his sanitary ramblings; nor will they be surprised to learn that he found in it one of the most horrible combinations that can be conceived, of all the disgusting abominations which can taint the air, outrage decency, and depress the physical and degrade the moral state of a human population. Our own knowledge of some parts of this district is enough to convince us that Dr. Gavin's picture is by no means overcharged; and we recommend it to all who desire to know the real state of one of our worst collections of human pigsties (for such they are rendered by the filth, in the midst of which they are situated), and who wish for some strong facts wherewith to make an impression on the opponents of sanitary reform. Perhaps the aldermanic swine, before whom our excellent friend, 'Punch,' represents Lord Morpeth as casting his pearls, may find a weighty argument in the following remarkable statement:

"Immediately adjoining the Patent Manure Manufactory,* is the establishment of a bottle-merchant. He complained to me in the strongest terms of the expenses and annoyances he had been put to, through the emanations which floated in the atmosphere having caused his bottles to spoil the wine which was bottled in bottles that had not been *very* recently washed. He was compelled frequently to change his straw, and frequently to wash his bottles; and considered that, unless the nuisance could be suppressed, he would be compelled to leave his present premises." (p. 27.)

This fact is particularly striking; since it shows that emanations from these masses of putrescence may act as ferments on dead as well as living organic compounds, and that they may even adhere to such a solid as glass, and may thus be conveyed to any distance, there to propagate their

* This manufactory, situated in Charles street, bordering on the parish of Whitechapel, has lately been suppressed as a public nuisance.

destructive influence.—The following passage is too good not to quote, as a curious example of the facility with which habit renders us blind to our own frailties, whilst it leaves us fully open to the perception of the not dissimilar faults of others. We happen to know two ladies, each of whom had a most abominable strabismus (we use the past tense, because each has been most effectually and completely cured by operation), their two eyes, as a facetious friend remarked, "having different spheres of usefulness;" and it was highly amusing to hear each of them commiserating the other's misfortune, as thus;—"Poor Miss A., what a pity it is that she squints so dreadfully;"—or, "Poor Mrs. B., she really has the worst squint I ever saw;"—each forgetting, or happily ignorant of, her own want of optical symmetry. Dr. Gavin's manure manufacturer, and a nightman who had a yard close by, present a striking parallel to our own fair friends.

"The nightman's servant accused the premises of the manure manufacturer as the source of perpetual foul smells, but thought *his* yard free from any particular cause of complaint; while the servant of the patent manure manufacturer diligently and earnestly asserted the perfect freedom of his master's yard from foul exhalations; but considered that the raking up of the drying night soil, on the other side of the wall, was 'quite awful, and enough to kill anybody.'" (p. 27.)

Dr. Gavin's inquiries have most forcibly brought out, as regards the district in question, the entire emptiness of any considerable improvement emanating from the local authorities, even when endowed with ample powers. The Commissioners and Guardians are themselves the chief proprietors of the dwellings of the poor; and, as they generally pay the rates themselves, and have already exacted for their tenements the highest attainable rents, any increase of rates would only be an increase of their own expenditure. We quite agree, therefore, with Dr. Gavin,—

"That the knowledge of the most economical and effectual means of carrying out the necessary works must be provided for the local authorities, and that the manner of executing them must be supervised by a central power; so as to prevent a wasteful expenditure of the money of the parishioners in works, irregular, imperfect, and inefficient, without any comprehensive plan or unity of design." (p. 115.)

We strongly recommend this work of Dr. Gavin's, as containing a large amount of original information, peculiarly adapted to promote improvement by showing the demand for it in the strongest possible colours that *truth* can supply.

The Bolton Report is peculiarly interesting as the production of a *working man*, whose intelligence and acquirements have distinguished him amongst his fellows, and raised him into an honorable and useful position. It was prepared at the request and at the expense of the Mayor of the Borough (in his private capacity), and exhibits a picture of the evils of local irresponsible self-government, which it would be difficult to find surpassed even in London or Liverpool. The following table displays the relative mortality in the different parts of the Bolton Union, calculated from an average of five years; showing also the annual excess of deaths above the standard rate of two per cent. or 1 in 50, to which, as we have

formerly shown, it is next to certain that the mortality even of large towns might be reduced by proper sanitary regulations.

TOWNSHIPS.	Population in 1845.	Deaths per ann.	Mortality per cent.	Or one in	Excess over 2 per cent.
Great Bolton	35,914	1313	3·65	27·39	595
Little Bolton......	17,251	485	2·81	35·38	140
Out Townships ...	51,043	1119	2·19	45·66	86

Thus the mortality of Great Bolton is greater than that of Sheffield, which hitherto enjoyed a bad eminence in this respect; and out of a population of about 36,000, nearly *six hundred*, or 1 in 60, die unnecessarily every year. What should we think of an annual sacrifice of one out of every sixty of our population, to satisfy the cravings of some insatiable monster like the Minotaur of old? Should we not put forth every effort, and be ready to sacrifice all our worldly possessions, to avert it? And yet this sacrifice is in effect offered up every year in Great Bolton to the Ogre *filth*. The fact is too plain to be gainsaid. There is scarcely any difference between Great and Little Bolton, as regards the employments of the inhabitants and their command of the necessaries of life; but there is a very important difference as regards the sewerage of the two townships. Though the drainage of Little Bolton is very far from what it ought to be, still it is by no means ineffectual; the natural facilities arising from the declivity of its site having prevented any serious blunder in fixing the levels. But in Great Bolton, with its population of thirty-six thousand, drainage can scarcely be said to exist. Many streets are entirely destitute of sewers, at least there are none known to exist in them,—a superannuated paviour being the only authority upon the subject: and where street-sewers do exist, they sometimes run up-hill, and are in other cases so small as to be completely flooded by a storm of rain, so as to pour in upon the inhabitants of cellars and drive them from their miserable abodes. House drainage is almost unknown.—We think that the extraordinary mortality of Great Bolton, which is *more than double* that of the most favorable country districts, is thus sufficiently accounted for. There is ample room for improvement in Little Bolton; the annual number of unnecessary deaths being about one in every 123 of its population. And the Out Townships are far from presenting the condition of rural districts; being chiefly, in fact, the suburbs of the borough, and participating in its abominations.

We trust that the 'Sanitary Act' will work a speedy reformation in these and similar localities; and that its beneficent operations will not be resisted by those, who have shown themselves so grievously incompetent to take care of their own and their neighbours' lives.

PART SECOND.

Bibliographical Notices.

ART. I.—*General Index to the Twenty-Four Volumes of the British and Foreign Medical Review, or Quarterly Journal of Medicine and Surgery.* Edited by JOHN FORBES, M.D., F.R.S., F.G.S., &c.—*London,* 1848. 8vo, pp. 304.

IT was scarcely to be expected that Dr. Forbes, after the heavy pecuniary loss he had sustained by his Journal, should have ventured upon the large outlay and probable additional loss, entailed by the construction and publication of a General Index to the whole series. Yet he has done so, apparently under the impulse of a desire to render a work, which has unquestionably contributed largely to the improvement of the profession, as perfect as possible; and with that chivalrous disinterestedness which his whole career in connexion with the Journal has manifested.

We need scarcely point out the value of an index of this kind, to every one who possesses the series to which it refers, and who desires to make use of its multifarious contents. For ourselves we can say that since we have become possessed of it, the index-volume has never been far from our desk; and that we have continual reason to be grateful for the assistance which it has afforded us. On no occasion, when we have desired to refer to any author or subject which we knew to have been noticed in the Review, have we been disappointed by meeting with a deficient or inaccurate direction; and our appreciation of the rich treasure of information contained in the work has been greatly raised by the new discoveries to which the Index has led us, as to valuable matters forgotten or overlooked at the time. We have every reason, then, to regard the Index as accurately and judiciously compiled; and that no trouble has been spared in its construction will appear from the fact, that the subordinate references as well as the principal headings are placed in strict alphabetical order. Each reference has its own separate line, with the volume and page conspicuously displayed in one invariable order of sequence and position; and the various subjects are referred to under so many different heads, that it is almost impossible for any important matter to escape even a superficial search.

We cannot dismiss this volume without the remark that the twelve years of the existence of the 'British and Foreign Medical Review' have formed a period in the history of the profession, which never has been, and perhaps never will be, surpassed in interest,—either as regards the amount of positive knowledge gained, or as respects the rise of medicine in the scale of sciences. For during that period we have witnessed the development of the reflex doctrine of nervous agency, in its multitudinous applications to physiology and pathology, by Dr. Marshall Hall and his

followers; a great addition to our knowledge of the special functions of particular nerves, by the labours of various neurologists; the rapid advance of embryology, chiefly through the labours of German observers; the discovery of the agency of cell-growth, in the first production, continual renovation, and functional activity, of the tissues and organs of animals, first made in Germany, but extended and completed in this country, thereby placing the science of physiology upon a more solid foundation than it had ever before possessed, besides affording us an exact knowledge of many details previously considered inscrutable; the rapid advance of organic chemistry, mainly through the researches and speculations of Liebig and Mulder; the revival of the humoral pathology, in a new, definite, and less exclusive form, resulting in great degree from the progress of organic chemistry; the introduction of a far more scientific and successful method of treating various diseases, whose true pathology has now become known; and the discovery of the wonderful efficacy of ether and chloroform as anæsthetic agents. On all these subjects, and on many others scarcely less important, the pages of the 'British and Foreign Medical Review' contain rich stores of information, most of it contributed by men who have attained distinction in the profession, and altogether constituting a body of literary and scientific medical history, which, we venture to say, cannot be paralleled elsewhere. The series, now immeasurably increased in intrinsic value by the addition of the Index, well deserves a place, therefore, in every professional library, public or private; and we are confident that, from the peculiar circumstances to which we have just adverted, it can never be out of date, but will be referred to with interest a century hence.

Art. II.—*Memoranda for Young Practitioners in Midwifery.* By Edward Rigby, m.d., &c. Second Edition, considerably enlarged.—*London,* 1848. 18mo, pp. 64.

Dr. Rigby has found some encouragement in enlarging the present edition, as 2000 copies of the first edition have been sold. It is altogether a trifling production, and in our minds of very questionable utility. Any attentive student at an hospital would have taken, in his attendance on lectures, as good and as full a set of memoranda for his practical guidance in after life, as these of Dr. Rigby; and if any of the pupils at St. Bartholomew's hospital are induced to abandon the valuable discipline of taking and collating their own notes of lectures, because they have been supplied by the lecturer, we feel assured that this little book will have done more harm than good. One of the evils of such a book is the dogmatism which almost of necessity pervades it; a style of writing which is very taking to a badly-informed practitioner, but which tends to make him a man of routine, with very little principle and less thought. There is no need for criticising the memoranda; as they are all contained and fully worked out in Dr. Rigby's Midwifery in the Library of Medicine, a work which has made the author's reputation, and deserves the attentive study of those junior practitioners, to whom these emasculated memoranda would prove a miserable substitute and we would fain hope a superfluous book.

Art. III.—*The Treasury of Natural History; or a Popular Dictionary of Animated Nature, illustrated with upwards of Eight Hundred Figures on Wood.* By Samuel Maunder.—*London*, 1848. Fcap. 8vo, pp. 812.

This volume is the fifth of a well-known series of "Treasuries," prepared by a very painstaking compiler, with more of discrimination and accuracy than is usually to be discerned in such works. It will prove a most convenient work of reference to all those who wish to know something of the animals whose names they meet with in the course of their reading, and will be especially useful to the student of comparative anatomy. The information in many departments is brought down to a very recent period; thus under the head of "Dodo," we find a notice of the novel views put forth by Mr. Strickland at Oxford last year; and the account of the Lepidosiren includes a brief statement of the question at present under discussion as to its reptilian or piscine character. Several of the most important novelties in palæontology also,—such as the Dinornis, Glyptodon, and Mylodon,—receive a fitting notice. The few defects which we could point out do not seriously impair the value of the work; and we shall not be so ungracious, therefore, as to dwell upon them. The illustrations are on the whole very well executed; though their small size is frequently a serious disadvantage. We may remark that an unduly large proportion of them consists of birds and insects, and that many of the former are so like one another, that they scarcely supply any distinctive characters; whilst on the other hand, many of the least-known and most remarkable animals are unfigured. Some change in this respect would be advisable in a future edition. Subjoined to the Dictionary is a syllabus of practical Taxidermy, which is much more comprehensive than its title implies; for in addition to the instructions given for skinning and stuffing, we find some very good practical directions for collecting animals of almost every description. And the volume concludes with a very copious glossarial appendix, in which we meet with a concise explanation of the descriptive terms employed in zoological works.

We think it right to state, lest our readers should form a wrong impression from the title of the volume, that it treats of the Animal kingdom alone; the author being apparently unaware, or not having sufficiently reflected, that the terms "Natural History" and "Animated Nature" refer to the whole organized world.

Art. IV.—*Insanity tested by Science, and shown to be a Disease rarely connected with permanent Organic Lesion of the Brain, and on that account far more susceptible of Cure than has hitherto been supposed.* By C. M. Burnett, m.d.—*London*, 1848. 8vo, pp. 108.

The author's object is to establish the *humoral* origin of most forms of insanity; but though we are inclined to accord with his conclusion, it is not on account of the arguments he has advanced, which fall very far short of the demonstration at which he has aimed. Thus he has scarcely adverted to the well-known fact of the production of a state of temporary insanity by various intoxicating agents, such as nitrous oxide, hachisch,

and opium; and even the intoxicating powers of alcohol, though referred to, are not made to support his argument as they might have been. Dr. Burnett seems not to have been aware that the doctrine in question was put forwards a few years since by Mr. Sheppard, of Stonehouse; the class of facts to which we have referred being made its basis, as they legitimately deserve to be. Some considerations on the subject will be found also at p. 220 of Vol. XXIII of the 'British and Foreign Medical Review;' so that he cannot claim any novelty as regards the idea in question. The form in which he has clothed it will not, we think, impress our readers with any very exalted idea of his clearness of thought or expression:

> "These are a few of the arguments which would strengthen the idea that the vital and the mental principles, if separate, are nevertheless equally resident in the whole mass of the circulating fluid; that their phenomena must be dependent upon each other; and that the organs which act in obedience to them are mutually connected, and exert an influence as inseparable in their action, as complicated in their effects upon each other." (p. 11.)

We have failed to discern, in the treatment advocated by Dr. Burnett, any indication of superiority to that which is usually practised by judicious physicians, experienced in the management of insanity.

ART. V.—1. *Register of Cases professionally attended.—London*, 1848. 8vo.
2. *Register of Midwifery Cases professionally attended.—Lond.*, 1848. 4to.

It must be quite unnecessary for us to dwell on the importance of the preservation of records of medical, surgical, or obstetric practice: since this is admitted by every thoughtful man, however little he may himself act upon it. But we may remark that, for the comparison and generalization of results, it is highly desirable that the observations should be made on a uniform system; and we cannot but rejoice, therefore, at the appearance of the above publications, as alike calculated to facilitate, and therefore to promote, the registration of medical facts, and also to bring about the desiderated uniformity of method. They are prepared, we believe, at the suggestion and under the direction of Mr. Farr, our invaluable Deputy-Registrar; and they are issued by the Government stationer at a price scarcely beyond that of the same quantity of blank paper. The Register of Midwifery cases is furnished with Dr. Tyler Smith's periodoscope, a simple instrument of marvellous convenience, which informs the accoucheur, without the trouble of calculation, of the time when labour may be expected, the times of special danger of abortion, and other periodical phenomena incident to the female sex.

We perceive that a register of cholera cases is announced; and we trust that every practitioner will feel it a duty to avail himself of it, when the opportunity is afforded him; that he may contribute, by faithfully recording his observations, whether few or many, to the elucidation of the pathology of this fearful disease, and to the determination of the most successful method of treatment.

ART. VI.—*Third Report of the Commissioners appointed to Inquire whether any and what Special Means may be requisite for the Improvement of the Health of the Metropolis.* Dated Gwydyr House, Whitehall, 13th July, 1848.

THE Commissioners having been informed that cases of fever of a peculiar nature had recently occurred among the scholars of Westminster School, as well as among the inhabitants of several houses in the cloisters and precincts of the Abbey, directed their attention to the locality, especially as rumour ascribed the origin of the fever to the emptying and cleansing of a number of cess-pools in the neighbourhood.

Of 36, the total number attacked with fever, 32 were attacked within a period of eleven days, from the 14th to the 25th of April, and of the 36, 3 died of fever only. The cess-pools were cleansed in the latter end of January and beginning of February. The Commissioners very satisfactorily show that this process was not the cause; for although they allow that it cannot be doubted that danger might arise from the emptying of so large a number of cess-pools, if the work were done in such a way as to disturb and diffuse the noxious gases contained therein, they show that the plan adopted involved no such injurious results. The soil was removed by pumps, like fire-engines, with a closed flexible tube both for suction and discharge, and consequently with little or no exposure to the external atmosphere. The fecal matter was also considerably diluted with water, and was mixed at the same time with a deodorizing fluid, the effect of which, the Commissioners observe, in reducing smell, and fixing some of the diffusible and volatile gases, is universally admitted. Independently of these circumstances, the period between the alleged evolution of miasma and the outbreak of fever was too great to allow the latter to be fairly ascribed to the former.

After examining other alleged causes of the fever, and showing that they had no share in its production, the Commissioners demonstrate that the track of the fever was coincident, or very nearly so, with the track of a large private sewer; this sewer being so constructed that its contents could not flow into the public sewer, except by running up hill.

We shall in a future article revert to more of the facts brought forward in this Report; we may here state, however, that it is a valuable addition to the literature of fever, and satisfactorily exhibits some part, at least, of its etiology.

ART. VII.—*The Hand Phrenologically considered: being a Glimpse at the Relation of the Mind with the Organisation of the Body.—London,* 1848. 12mo, pp. 78.

WE almost surmised, when we first glanced at this production, that it was intended as a satire upon craniology; but we soon found that the writer is in earnest. There is not, however, any pretence at originality in the book; the materials for which are derived from the 'Chirognomonie' of D'Arpentigny, and from a more recent work on the same subject by Professor Carus. The author states that whilst availing himself freely of the materials supplied by these authors, he has "modified, corrected, or omitted their theories and statements, when not in accordance with his

own experience." As the author withholds his name, we have no power of estimating his experience or his capacity for reasoning upon it; and it would consequently have been much more satisfactory to the reader to have had the unaltered views of the original authors.

By far the greater part of the treatise is occupied with general considerations on the connexion of bodily and mental structure; which are apparently designed to establish an *a priori* argument on behalf of the hand as specially manifesting the psychical character. The *proofs* of the position, as deduced from observation, are of the most meagre nature possible. Four types of manual conformation are described;—the *elementary* hand, in which the palm predominates, characteristic of a rude state of society, and of the predominance of habit and instinct over reason;—the *sensitive* hand, such as is to be commonly met with amongst females of the higher classes;—the *motive* hand, whose characters depend on the strong and massive development of the osseous and muscular systems;—and the *psychical* hand, distinguished by its small size and delicacy, with long tapering fingers, and indicative of the predominance of the higher mental faculties.

Between these, many intermediate gradations exist; such as the *artistic* hand, which seems compounded of the sensitive and the psychical; the *spatulate*, in which the sensitive and the motive are combined, in a manner that adapts it to mechanical and other pursuits in which manual skill is required; and the *philosophical*, which forms the transition from the motive to the psychical.

The subject is by no means destitute of interest. There can be no doubt of the general fact that particular types of mental character very commonly coexist with particular types of bodily conformation; so that we almost instinctively form a judgment of character from personal appearance; and the hand is perhaps as good an exponent of the typical character, as any single organ could be. But at the same time, a far greater amount of observation is required to substantiate the claims made for it in this work; and after all, we do not see that any practical benefit will be derived from the establishment of the proposed classification.

ART. VIII.—*Sulla inefficacia dello Zaffo, e sui vantaggi dell' Incisioni Laterale della Bocca dell' Utero nelle Isterorragie per Distacco di Placenta, gli ultimi Mesi di Gravidanza.* Dal Dottore G. B. BELLINI. —*Firenze*, 1845.

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We must confess ourselves strongly opposed to the practice of the author. If the os uteri be at all dilatable, the hand alone properly used as a wedge is alone quite sufficient to induce sufficient dilatation to allow the child to be turned; and it is very seldom that there is much rigidity, when any considerable hemorrhage is going on. Neither do we agree with all the objections urged against the plug. Its employment is certainly dangerous when the membranes are ruptured, the uterus inert, and the placenta only partially attached over the mouth and neck, as fatal hemorrhage might go on into the cavity of the uterus, vaginal escape being prevented. But when the placenta is completely attached all round the os uteri, and this orifice is small, hard, and unyielding, the plug is most invaluable. The placenta itself is a plug preventing hemorrhage into the cavity of the uterus, and if the vagina be well filled with lint or cotton, the uterine contraction and dilatation of the mouth go on in perfect safety, until interference is required.

We have certainly never met with a case in which incision of the uterus was at all called for, or justifiable on any grounds; still it is well to know that it has been safely and successfully done.

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THE College of Physicians is the stronghold of the *literæ humaniores* of our profession; and we should be very sorry to see it forced to surrender its charge to the clamour of practical utility; for we are satisfied that, for the higher class of practitioners which it includes, it is quite feasible to frame a plan of education which shall combine both. Still we think that the honours of the College should be bestowed for professional rather than for literary merit; for as the cultivation of literature alone does not make a good physician, and as it is quite possible for a man to be a scientific pathologist and a successful practitioner who never knew a particle of Latin or Greek, we cannot see that classical attainments can be legitimately regarded in any other light than as means to an end. We discern in Dr. Hawkins's Oration what we cannot but regard as an undue disposition to exalt the learned, at the expense of the scientific, members of his College. For the reasons stated in the early part of this Number, we are satisfied that by thus throwing itself back upon the past, instead of moving with the present, the College will continue to lose ground with the profession and the public, rather than regain that which it has already lost. It has lately made some good steps in the right direction, more especially in the appointment of its lecturers, who have been selected amongst the younger physicians of the metropolis, most distinguished by their scientific attainments, instead of from the grave and reverend seniors, whose chief merit lies in their capacity to write an elegant Latin oration.

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Dr. Hawkins's Oration is a very clever production, and, with the exception we have alluded to, is liberal and gentlemanly in its tone. It contains some amusing hits at some of the current quackeries of the day, done into very ingenious Latin; and will altogether well repay perusal.

Art. X.—*Gray's Supplement to the Pharmacopœia; being a concise but comprehensive Dispensatory and Manual of Facts and Formulæ for the Chemist and Druggist and Medical Practitioner.* By Theophilus Redwood, &c. Second Edition.—*London,* 1848. pp. 1070.

The progress of science having removed from our pharmacopœias a crowd of substances which still continued to be employed in medicine, and our modern dispensatories being confined chiefly to a detailed description of the contents of the pharmacopœias, the publication of some work became necessary, in which all the substances should be noticed that are used in medicine, or even in the arts. This want was supplied by Gray's 'Supplement to the Pharmacopœia,' which, first published in 1818, attained considerable circulation, and passed through six editions. Of this very useful work, Mr. Redwood, Professor of Chemistry and Pharmacy to the Pharmaceutical Society, has published a new edition, respecting which he says: "Using as much of the matter of previous editions as I considered useful, yet omitting much, and adding still more, I have entirely rewritten and rearranged the work." So well has the author executed his task, and so much was the work required, that a second edition has been called for in less than a year from the issue of the first.

As the nature of the work renders it unsuitable for review, we cannot do better than give an idea of its contents, chiefly from the author's own statement.

"The long preface appended to former editions, is in this replaced by a brief chronological history of pharmacopœias and dispensatories."

This part of the work has been made more complete in Mr. Redwood's second edition, especially with regard to the foreign pharmacopœias.

The articles on weights and measures, specific gravities, thermometrical scales, &c., have been much amplified, and several tables for facilitating calculations, some of which have been prepared especially for the work, have been added."

To this portion also corrections founded on recent investigations, especially in relation to specific gravities and atomic weights, have been added in the second edition.

"In the part which treats of 'Animals yielding Products employed in Medicine, Domestic Economy, and the Arts,' about three hundred animals are described (*noticed*), which are arranged according to Cuvier's classification, some of the characters, the habitations, food, and useful products of these are briefly noticed.

"Among the 'Vegetables yielding Products employed in Medicine, Domestic Economy, and the Arts,' are included nearly three thousand plants. These are arranged after De Candolle's classification, and reference is given for all the genera to the Prodromus, &c. The notices of the applications and uses of the plants or their products, are necessarily brief in accordance with the scope and purpose of the work; they are given on the authority of the writers to whom reference is made."

These references being chiefly to works which are themselves compilations, the original authors are necessarily in many cases unnoticed. But to the second edition many additions are made and a great number of

synonymes have been appended, and the authorities for the different botanical names have in many cases been supplied.

'The last part of the work comprehends the 'Formulæ for the Preparation of Compounds employed in Medicine, Domestic Economy, and the Arts,' together with mineral substances, and some animal and vegetable products. Besides all the formulæ of the three British Pharmacopœias, a selection is here given from the foreign pharmacopœias of various parts of the world."

To these formulæ, the authorization of which is indicated by explicit reference to the sources from whence they are derived, are added a great number of others for the preparation of substances either sold or used by the class of persons for whom the book is intended. Many are now published for the first time, and relate to compounds for which there have hitherto been no good published formulæ.

To this part of the work considerable additions and corrections have been made in the second edition, which, though containing more matter than the first, is much more compact by adopting a more condensed typography. The utility of the work is greatly increased to every one by the ready references afforded by a copious and well-made index, which includes from pages 959 to 1070, and will give the best idea of the varied contents of this useful and well-executed work.

ART. XI.—1. *Answer to the Religious Objections advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery.* By J. Y. SIMPSON, M.D., F.R.S.E., &c. &c. &c.—*Edinburgh*, 1847. 8vo, pp. 24.

2. *Scriptural Authority for the Mitigation of the Pains of Labour, by Chloroform and other Anæsthetic Agents.* By PROTHEROE SMITH, M.D., &c. &c. &c.—*London*, 1848. 8vo, pp. 52.

HOWEVER little we may feel the want of these two pamphlets for our own personal comfort, we cannot say that we are altogether surprised at the existence of a necessity for their appearance. Probably the announcement of the successful employment of anæsthetic agents in surgical operations was welcomed by almost everybody with feelings of thankful joy and wonder; not unmingled perhaps with some few doubts and misgivings, as if the intelligence were indeed too good to be true; but still without any scruples as to the lawfulness of making free use of so great a blessing. But when in the course of a short time these marvellous substances were found to be equally efficient in midwifery, by doing away with the pangs of childbirth without interfering with the muscular contractions of the uterus, then many serious-minded persons had to pause for a moment before they could heartily approve of so strange and bold a proceeding as this appeared to be. It certainly did seem at first sight, as if the might of human science were coming into a positive and direct collision with Divine Power and Wisdom; as if GOD had said to woman, "In pain and agony ye shall bring forth your offspring," and daring man (like another tempter,) had said in reply, "No, ye shall suffer pain no longer." This was the light in which the whole matter presented itself at first to the mind of many a thoughtful and earnest man, and perhaps still more to scrupulous and conscientious women; but, when they began

to look deeper into the subject, they found that this was in fact only another instance of that most important truth, that human science is *not* opposed to the Divine decrees, and that, whenever the contrary seems to be the case, it is only because our ignorance or imperfect intellect makes us unable to harmonise the apparent contradictions. Of course as a matter of *feeling*, many persons are still opposed to the use of anæsthetic agents in midwifery; but with these it is vain to attempt to argue, and time alone and the example of their neighbours will be able to induce them to give up their resolution; but we really think that as a matter of *reasoning*, the two pamphlets before us fairly and completely settle the question, and that it can never be made a subject of discussion (at least on religious grounds,) for the future.

It is urged by some persons against the abolition of the pains of labour, "that what GOD ordained at the fall as woman's curse, is an established rule, with the operation of which it is not lawful for man to meddle." (Smith, p. 7.) The argument, when stated fully, is as follows:—It is not lawful for man to oppose any of the Divine decrees; the abolition of the pains of labour is an opposition to one of these; and therefore the use of anæsthetic agents in midwifery is unlawful. The syllogism is correct in form, and the truth of the major premiss would (we believe) be admitted at once by almost every member of our profession; but the minor we deny *in toto*, and we contend with Drs. Simpson and Protheroe Smith, that to annihilate the pangs of childbirth is *not* an opposition to one of GOD's established rules. And here comes in an exceedingly valuable piece of philological criticism in Dr. Simpson's pamphlet (p. 8, &c.), in which he draws attention to the different meaning of the Hebrew words עֶצֶב *'Etzeb* on the one hand, and חֶבֶל *Chebel* or חיל *Chil* on the other; of which the last two designate the pangs of parturition, and the first is used to signify generally, "maceratio animi et corporis, molestia, labor, solicitudo," and the like, and is accordingly rendered in the Septuagint version by the words λύπη, ὀδύνη, πόνος, μέριμνα, &c. Now in Gen. iii, 16, "In sorrow thou shalt bring forth children," (which is the text to which the objectors principally appeal); the Hebrew word, which is rendered "sorrow" in our version, is not *Chebel* or *Chil*, but *'Etzeb;* or in other words, the woman is not told that childbearing shall be accompanied by the pangs of parturition, but by "sorrow," which may of course partly relate to the bodily pain, but which has also a much more extended meaning; for (as Dr. Simpson himself remarks) "there are abundance of 'maternal sorrows' connected with children and childbearing in the civilized woman, quite independently of the actual agonies of parturition." (p. 10.)

This distinction between the Hebrew words is undeniable, and we believe unanswerable; and we consider that Dr. Simpson has done good service in bringing it so prominently forward. However, he goes on to express his opinion (p. 13) that "we are justly entitled to infer that the Hebrew term, which, in our English translation of the primæval curse, is rendered 'sorrow' (Gen. iii, 16), principally signifies the severe muscular *efforts* and *struggles* of which parturition—and more particularly human parturition—essentially consists;" and again, after stating in his letter to Dr. Protheroe Smith (p. 45) that "it is only of late that these two elements or constituents of labour-pains (viz. first, the contraction of the uterus,

and secondly, the sensation of pain,) have been recognised and studied by the profession as two separate objects," he proceeds:

"It is surely worthy of remark and wonder, that the language of the Bible is, on this as on other points, strictly and scientifically correct, and long ago made, with perfect precision, the very distinction which we are now-a-days only recognising. For the Hebrew noun, *'etzebh*, distinctly signifies the muscular contraction or effort; and the nouns, *hhil* and *hhebel*, as distinctly signify the sensations of pain accompanying those efforts.........Now the efforts or muscular contractions (the *'etzebh* of the curse) are left in their full and complete integrity under the state of anæsthesia; while the pangs or sufferings (or *hhil*), against which the language of the curse does *not* bear, are alone annulled and abrogated."

Here we are compelled to differ from Dr. Simpson; not indeed because we think the language is incorrect, but because we doubt whether Dr. Simpson rightly understands it; because we believe that the word *'etzeb* has nothing to do with "muscular contractions or efforts;" and in fact because, so far from wishing to alter the word "sorrow" in our translation, we think that (provided it be not misunderstood) no other word could so well and so adequately express the meaning of the Hebrew term, comprehending, as it does, all the anxiety, weakness, weariness, fatigue, &c., attending pregnancy, parturition, and the rearing of the infant. If Dr. Simpson will produce an example in any medical work (as, for instance, the Hebrew translation of Avicenna), where the word plainly signifies "muscular contraction or effort," we shall not then deny that it may *possibly* bear that meaning in this passage of Genesis, though even then we shall still prefer the sense given by our version, which agrees with that of the Septuagint (*ἐν λύπαις*), and the Vulgate (*in dolore*); but till he does this, we shall take the liberty of retaining our present opinion, which is, that there is nothing either in the derivation or the use of the word that will warrant Dr. Simpson's interpretation. Our readers must not think we have forgotten ourselves, and that we fancy we are writing for a *philological* instead of a *medical* journal; but it is clearly important that, when a sound and useful argument is brought forward, it should not run the risk of losing part of its value in consequence of its being pushed to an unwarrantable extent.

Having routed the enemy by his philological artillery, Dr Simpson proceeds to follow up his victory by a discharge of rhetorical small arms, in which he uses the *argumentum ad hominem*, and the *reductio ad absurdum* with great effect; showing that those who refuse to produce sleep and anæsthesia by means of ether or chloroform, do not hesitate to use opium and other narcotics with the same object (pp. 19, 20;* and Letter to Dr. Smith, p. 45); and that, if we wish to be consistent, we ought not only to abstain from doing anything at all to mitigate the pains of childbirth, but also to let our ground lie fallow for "thorns and thistles" to run riot in, or at any rate eschew the use of machinery because it was said to man, "In the sweat of thy face shalt thou eat bread." (p. 6; and

* As even "Homer sometimes nods," we cannot be surprised that Dr. Simpson, who has had so much to do with "hypnotics" lately, should occasionally find himself under their influence at times when he would wish to be wide awake. A trifling slip of the pen at p. 20 has made the writer say exactly the contrary to what he intended. He says, "there is no greater impropriety or sin in producing sleep and freedom from pain by exhibiting a medicine by the mouth, than by exhibiting it by the lungs." Of course, for the purpose of his argument, the words *mouth* and *lungs* require to be transposed.

Smith, p. 46.) He also reminds us, "that, if God had really willed the pains of labour to be irremoveable, no possible device of man could ever have removed them," (Smith, p. 47); and briefly notices the objection that the employment of anæsthetic means and the abrogation of pain in labour must be irreligious because it is "unnatural." (p. 49.) Surely persons ought to abstain from talking about what is "unnatural," till the "laws of nature" are really and thoroughly understood, which is far enough from being the case at present. In the mean time, we think Dr. Simpson gave a very sufficient answer to one of those who used the argument. "How unnatural," exclaimed an Irish lady to him lately, "how unnatural it is for you doctors in Edinburgh to take away the pains of your patients when in labour." "How unnatural," replied he, "is it for you to have swum over from Ireland to Scotland against wind and tide in a steam-boat." He might have gone farther, and have quoted Horace's lines, (Od. i, 3.)

> "Nequicquam Deus abscidit
> Prudens oceano dissociabili
> Terras, si tamen *impiæ*
> *Non tangenda* rates transiliunt vada;"

and have said, "How unnatural was it for you to cross the water at all!"

Our remarks have been chiefly directed to Dr. Simpson's pamphlet, because it is to him *principally* that we owe the application of anæsthetic agents to midwifery; but they are equally applicable to Dr. Protheroe Smith's little work, for much the same arguments are to be found in both. The latter, however, contains also several pages of purely theological matter, which are indeed written in a serious and excellent tone and spirit, but which we must leave to theologians to discuss, and which some persons will perhaps consider to be somewhat *ἀπροσδιόνυσα*, or in plain English, rather out of place.

We have only to add that we can heartily recommend both pamphlets, not only to the members of the profession, but also to such non-medical persons as take an interest in the subject.

Art. XII.—*A Treatise on the Production and Management of Fish in Fresh Waters, by Artificial Spawning, Breeding, and Rearing; showing also the Cause of the Depletion of all Rivers and Streams.* By Gottlieb Boccius.—*London,* 1848. 8vo, pp. 38.

Times are changed since it used to be an article in the indentures of apprenticeship in more than one part of this country, that the apprentice should not be required to eat salmon for dinner more than three times a week. The author of this little treatise, lamenting the depopulation which has been gradually in progress amongst the finny inhabitants of our rivers and streams, has carried on a course of experiments, during several years past; from the results of which he is satisfied that our fresh waters might easily be again made, as of old, to yield a very large supply of wholesome food to our hungry people, to say nothing of the increased amusement which he promises to the angler. The gradual diminution, and almost extirpation, of the fish in many streams, he chiefly traces to the contamination of their tributaries by the sewerage of towns and villages, which

has a most pernicious influence on the eggs and young fry. He does not regard this contamination as injurious (in its present amount at least) to the fish when more advanced; on the contrary, it increases their supply of food by engendering large numbers of worms and insects, and thus favours their growth and vigour. His plan for restoring the finny population, therefore, consists essentially in the system of artificial breeding, by which the young fry may be reared in pure water, until fit to be turned into the streams. The treatise is well worthy of attention, both as regards its economical value, and containing many facts of interest to the naturalist and physiologist.

ART. XIII.—*A Familiar Introduction to the Study of Polarized Light; with a Description of, and Instructions for using, the Table and Hydro-Oxygen Polariscope and Microscope.* By CHARLES WOODWARD, F.R.S., President of the Islington Literary and Scientific Society. Illustrated by numerous Wood-Engravings.—8vo, pp. 40.

WE cordially recommend this little treatise to such among our readers as are in the habit of using the Polarizing apparatus in connexion with their microscopes, and desire to understand something of the rationale of the beautiful effects which they produce by its means; and to such also as are occasionally perplexed by the question now so frequently asked of men of any scientific pretensions,—What is polarized light?—for which it is just as well to be prepared with an answer, instead of being obliged to confess ignorance. The public are under great obligations to Mr. Woodward for the pains he has taken to improve the hydro-oxygen microscope, and to develope the resources of the polariscope; and as he possesses not merely an extensive acquaintance with the phenomena and laws of optics, but also great skill in giving simple and lucid explanations of complex and abstruse matters, his "familiar introduction" has the rare merit of being well adapted for the learner, and at the same time really scientific in its character. The numerous diagrams supply all the illustrations needed to render the descriptions readily intelligible.

ART. XIV.—*Obstetric Plates, with Explanations: selected from the Anatomical Tables of William Smellie, M.D.—London,* 1848. 8vo, pp. 28. Twelve Plates.

THE demand for these interesting Plates, which represent Smellie's views of the mechanism of labour, and the manner of adapting and using the forceps in the artificial extraction of the child, appears to be so great as to have called for a new edition, which the publisher has prepared with praiseworthy care. Comparing this with the former edition, we find the descriptive part almost entirely rewritten, and made to correspond with our present knowledge of midwifery. We cannot afford our readers a better proof of this, or give them a fairer sample of the character of information which the book is calculated to impart, than by transcribing the description of Plate I.

"Plate I shows a side view of the *pelvis, spinal column,* and *uterus,* during the first stage of a natural labour.

"The child is seen in its most usual attitude and position within the womb. It

forms an *ovoid*, which corresponds to the shape of the uterus at the latter months of gestation. The head, which is the smaller end of the *ovoid*, is presenting with the *occiput* to the left side of the *pelvis;* and the forehead, or larger *fontanelle*, to the right side. The chin is slightly inclined towards the chest; the arms touch the sides of the chest, and the fore-arms and hands cross over the chest. The legs are in a general state of flexion, and the feet cross each other.

"In the first stage of labour, the lower part of the womb, or *os uteri*, has to be dilated, so as to allow a free communication between the cavity of the womb and the *vagina*. This is effected mechanically by the womb contracting and forcing down a column of the *liquor amnii* towards the *os uteri*, which presses out the membranes at this part, forming an even hemispherical water wedge, by which the circle of the *os uteri* is equally enlarged. When the womb contracts, or, in other words, during a labour-pain, the head of the child is raised, as is seen in the plate, the waters gathering below it; and after the pain, when the womb is relaxed, it again subsides on the *os uteri*, and can there be felt through the thin membranes." (pp. 3, 4.)

We cordially recommend this little book to the notice of students and practitioners of midwifery.

ART. XIV.—1. *Continental Travel; with an Appendix on the Influence of Climate, the remedial Advantages of Travelling, &c.* By EDWIN LEE, Member of the principal European Medical Societies.—*London*, 1848. 8vo, pp. 353.

2. *The Baths and Watering-Places of England, considered with reference to their Curative Efficacy; with Observations on Mineral Waters, Bathing, &c.* By EDWIN LEE. Second Edition, much enlarged.—*London*, 1848. 12mo, pp. 212.

MR. EDWIN LEE, Member of the principal European Medical Societies, being apparently troubled with the *cacoethes scribendi*, it is well for himself and the public that his malady takes a form so harmless as these treatises would indicate. The first of them may be regarded as an improved edition of his 'Memoranda on France, Italy, and Germany,' which appeared some years since; and the author's object is to impart such condensed information respecting those parts of the Continent most frequently visited, as may be useful to the traveller, and especially the valetudinarian; without entering into the ordinary details of guide-books. As he speaks from repeated observation and experience, and is on the whole well qualified to give an impartial opinion, we are disposed to think that this work will be found a trustworthy indication to the continental visitor; though we cannot see its peculiar adaptation "for home reading as a book of travels." The illustrations, characterised in one of the advertisements as "splendid," whatever may be their fidelity, are anything but agreeable to the eye. The appended observations on the influence of climate and the remedial advantages of travelling, are of a sensible character, but without any claim to higher eulogy.

The little volume describing the baths and watering-places of England, contains a large amount of information in a concise form. The author has combined his own observations with the data obtained from local authorities; and as his accounts of the localities, with which we are ourselves well acquainted, are very accurate and characteristic, we are inclined to take his word as to the remainder.

PART THIRD.

Periscope.

ANATOMY AND PHYSIOLOGY.

On the Minute Structure of the Supra-renal Capsules in Man, and the four Classes of Vertebrated Animals. By Professor ALEX. ECKER, of Bâsle.

OF this valuable monograph, which is worthy to take rank with Mr. Simon's Essay on the Thymus Gland, we shall endeavour to present, within a narrow compass, the most important features. After some general observations on the nature of the so-called "vascular glands," Professor Ecker proceeds to inquire into their true relations with the acknowledged glands; of the essential elements of which he gives the following enumeration.

1. The glandular membrane, thin, and structureless, fashioned into variously-formed cavities, vesicles, or canals. These cavities either open externally, as is the case with glands having a *permanent excretory canal;* or they are closed and only periodically open, as in the case of the ovaries and the vesicles of the intestinal mucous membrane, which are examples of glands with a *temporary excretory canal.*

2. A capillary network distributed over the surface of the glandular membrane.

3. Cells and cell-nuclei, which either cover the internal surface of the membrane in question, or entirely fill its cavity.

These structures must be discovered in the "vascular glands," if they are still to be ranged in the class of glandular organs. In the *thyroid* gland, the true normal vesicles were first made known by Bardeleben (1841); he did not, however, determine the nature of their contents. These were stated by Mr. Simon (Phil. Trans. 1844), to consist of cells and cell-nuclei; and Professor Ecker has arrived at the same conclusion. The truly glandular structure of the *thymus* has been completely demonstrated by Mr. Simon, whose researches are confirmed by those of Dr. Ecker. In the *spleen*, the glandular vesicles have been known as Malpighian bodies, since the time of the anatomist from whom they received their name; but doubts have been entertained regarding the existence of a glandular membrane around them, especially in the mammalia. Professor Ecker, however, has been able to ascertain the existence of this membrane in mammalia as well as in birds. The accounts hitherto given of the *supra-renal* capsules are very imperfect; the distribution of the blood-vessels has been well described, and the occurrence of cells and cell-nuclei in their tissue has been determined; but the glandular membrane has not yet been demonstrated. We can only give Professor Ecker's *résumé* of his researches.

1. The intimate structure of the supra-renal bodies is the same in the four classes of vertebrated animals.

2. Everywhere we meet with closed glandular vesicles, formed of a structureless membrane, and containing a granular substance.

3. This substance is composed of (*a*) a plasma rich in albumen, mingled with very minute and very numerous granules of coagulated albumen; (*b*) cells and nuclei, the latter being either compact and dotted over with granules, or being vesicles with one or two nucleoli (the latter form occurring in fishes, and in man and

mammalia during the early period of life); these nuclei either lie freely in the midst of the granular mass, or they have an envelope, and the surface of this is often so condensed as to form a cellular membrane: all the cells of the supra-renal bodies are formed in this manner around a mass of granules including a nucleus; (*c*) numerous fatty particles: in many animals, such as the carnivorous mammalia, birds, and batrachia, a layer of fatty particles closely envelopes the cells.

4. The glandular vesicles are themselves the result of the development of simple cells. The nuclei multiply themselves by endogenous generation, probably by successive divisions of the primitive nucleus; and the enveloping cell-membrane, being distended, becomes the glandular membrane. We are thus acquainted with three modes of formation of glandular cavities; (*a*) by fusion of cells; (*b*) by intercellular spaces; (*c*) by the increase and distension of simple cells.

5. New vesicles are continually being developed from cells, and the old ones successively disappear.

6. A vascular network surrounds the glandular vesicles.

7. In all vertebrata, excepting the mammalia and man, the glandular vesicles form the entire substance of the organ. But in the mammalia and in man, the supra-renal body is composed of two substances, of which the cortical alone includes vesicles; in the horse, however, they are found in the medullary substance likewise. In the cortical substance the vesicles are generally elongated, and disposed end to end; and from this arrangement they frequently resemble tubes. The medullary substance is a network of conjunctive fibres, of vessels, and of very numerous nerves. The interstices of this network are occupied by a material similar to that which is contained in the vesicles of the cortical substance.

8. In serpents, the supra-renal bodies have afferent as well as efferent veins; being supplied with blood from the same source as the portal system of the liver.

9. In man alone are the supra-renal bodies more developed during the early period of life.

10. The elements of the gland furnish a fluid rich in protein and in fat, destined to be discharged into the vascular system, either by exosmose, or by the dehisence of the glandular vesicles. Its use has reference to the general function of nutrition.—*Annales des Sciences Naturelles*, August, 1847.

The author does not add anything to what was previously known regarding the development of these bodies; and he seems unacquainted with the observations of Professor Goodsir (Phil. Trans. 1846), who has ascertained that they are to be regarded as portions of the blastoderma or germinal membrane, continuous at an early period with the thymus and thyroid bodies, and retaining their original simplicity of structure whilst other parts undergo a histological as well as a morphological change. This interesting discovery is in complete harmony with the views which have been of late generally entertained regarding the function of the vascular glands: namely, that they are elaborating organs, destined to withdraw certain portions of the raw material from the current of the circulation, and to restore them again in a state better prepared for their purpose in the nutritive operation. "I have therefore," says Professor Goodsir, "been led to consider the supra-renal capsules, the thymus, and thyroid as organs essentially similar in structure; as developments of the remains of the blastoderma, being formed of a continuous portion of that part situated along each side of the spine, from the Wolffian bodies to the base of the cranium, the supra-renal capsules being developed in connexion with the omphalo-mesenteric vessels, the thymus with the jugular and cardinal veins and ductus Cuvieri, and the thyroid with the anastomosing branches of the first and second aortic arches—as organs performing functions, whatever these may be, analogous to those of the blastoderma, differing from them only in this, that the blastoderma not only elaborates nourishment for the embryo, but also absorbs it from without, that is, from the yelk; whereas the three organs in question only elaborate the matter which has been already absorbed by the other parts and is now circulating in the vessels of the more perfect individual."—(Op. cit., p. 640.)

On the Capillary Circulating System. By M. BOURGERY.

A SERIES of Memoirs on this subject have been presented to the Academy of Sciences of Paris, by M. Bourgery; of which the following are the general conclusions: He maintains that besides the *vasa intermedia*, which form the direct communication between the arteries and the veins, and which are in fact either the ultimate ramifications of the arteries, or the incipient rootlets of veins, there is in every organ a plexus of capillaries, which penetrates it more minutely, and is subservient to its functional activity. These "functional capillaries," according to M. Bourgery, are usually not above half, one third, or even one fourth of the diameter of the blood-corpuscles, and consequently can only convey the liquor sanguinis; they constitute a sort of diverticulum from the general circulation; and the movement of fluid in them may be almost stagnated, without any interruption to the passage of blood from the arteries into the veins. It is also affirmed by M. Bourgery that the lymphatic system has multitudinous free communications with the sanguiferous capillaries.—*Gazette Médicale*, 1848, No. 37.

As we have not yet before us the evidence upon which M. Bourgery rests his affirmations, we cannot judge of their value. We have never ourselves seen any evidence of the existence of the class of vessels he describes; and their absence in certain tissues which they were formerly supposed to penetrate,—such as cartilage, —is now a matter of positive certainty.

On the Multiplication of Vegetable Cells by Division. By Prof. MITSCHERLICH.

SOME interesting observations on this subject occur in a memoir read by Professor Mitscherlich before the Royal Academy of Berlin, on the Development and Composition of the Confervæ. They accord with the account of the process given by Mohl, and confirmed by Henfrey, as to the mode in which the multiplication of vegetable cells takes place by simple division. The process commences by a doubling-inwards of the "primordial utricle" or lining membrane of the cell, which detaches itself from the proper cell-wall, and exhibits an hour-glass constriction round its middle. This constriction continues to increase, until the original cell-cavity is divided into two parts, the communication between which is entirely closed up. Between the two layers of the primordial utricle thus folded in, a new layer of cell-membrane is subsequently formed; and thus the two new cells are at last completely divided from one another. The opinions of observers are becoming more and more in favour of the view, that multiplication by cell-division is the regular mode of increase in vegetating or growing parts. On the other hand, it is also generally agreed that spores, pollen, and embryos are produced by free cell-formation from nuclei.—*Annals of Natural History*, June, 1848.

On the Spleen. By Drs. VERGA and TIGRI.

DR. VERGA detailed to the *Scienziati* at Venice the results of his multiplied experiments upon the removal of the spleen of cats, dogs, &c. He arrived at the following conclusions:—1. Nature does not constantly provide animals who have been deprived of their spleens with a new one, nor with a greater development of the liver, the thyroid body, the omentum, or the mesenteric glands. 2. Obesity, salacity, or sterility, are not constant or frequent effects of its removal. 3. Among the least unfrequent phenomena are to be noted during life, a greater vivacity, conjoined with a tendency to tabes, notwithstanding a keen appetite; and after death various alterations in the liver.

Dr. Verga was opposed to considering the spleen as performing a mere mechanical office, as a diverticulum of the blood in the case of impeded circulation; but still he was desirous of giving some account to the congress of Dr. Tigri's researches, who had discovered in a special condition of the vessels of the human spleen—a mechanism which he termed a "compensator for the circulation." He found as far as the eye and the scalpel could pursue them, that the splenic arte-

ries and veins always ran within a common, inextensible cellular sheath, the veins being four or five times larger than the arteries, and in good part surrounding the calibre of these; the parietes of both vessels being so thin as to allow of the action of the fluid they contained being reciprocally felt. When a too large influx of blood upon the spleen takes place, therefore, the veins compress the arteries and impede a farther flow. Dr. Tigri was surprised to find that in the *horse* the veins and arteries ran at some distance from each other; but this fact, which seemed at first to oppose his theory, was found to support it, when he discovered that Nature, besides having given the vena portæ in this animal a valve, as first shown by Ernest Weber, has likewise furnished the veins leaving the spleen with valves, so that a regurgitation of blood into the viscus is prevented.

Dr. Verga mentioned, that in removing the spleen in cats and dogs, we may divide the duplicature of the peritoneum, connecting it to the stomach, without tying any of the small vessels into which the arteries and veins are there subdivided, these not giving rise to any important hemorrhage.—*Gaz. Medica di Milano*, 1847, No. 47.

ORGANIC CHEMISTRY.

On the Acidity and Alkalinity of certain of the Human Fluids in the state of Health and Disease. By M. ANDRAL.

IN their physiological conditions, each of the humours of the body presents a certain degree of acidity or alkalinity; and the spontaneous transformation of a naturally acid fluid into an alkaline one, or *vice versâ*, never takes place in the healthy organism. The utmost that can occur in this respect, is the rendering the fluid temporarily neutral by great dilution, as in the case of excessive perspiration—the water then being abstracted from the blood in larger proportion than the other principles. However this may be in health, the opinion is very generally entertained that in disease such chemical change in the humours does often take place; and the object of this paper is to investigate its accuracy.

Of all the fluids of the economy, the *serum of the blood* is the most decidedly alkaline; and whatever the nature of the disease or its duration, in which M. Andral has examined this fluid, he has never found the intensity of this reaction sensibly vary. Vogel quotes a case of metro-peritonitis from Scherer, in which the serum of the blood was said to be perfectly neutral, but adds, that he himself had never met with anything similar. If blood is examined after death, any acidity then found is the result of decomposition, and not the effect of disease. In examining the condition of fluids formed from the blood, it should be borne in mind that upon the same surfaces liquids possessed of different reactions may be found; so that the accidental predominance of one of these fluids may easily be mistaken for a change in the reaction of another. Thus the *sweat is acid*, but the *sebaceous matter is alkaline*. In the very various conditions in health and disease under which M. Andral has examined the sweat, he has found it generally acid, sometimes from dilution neutral, never alkaline; but at the same time, at some parts of the skin, where sebaceous follicles abound, as the axilla and other hairy parts, an alkaline reaction may exist. It is evident, then, that the sweat is not a simple escape of the serum of the blood, charged with certain of its principles, for then it would be alkaline; and if the skin be irritated by blisters and the like, the fluid consequently effused will be found decidedly alkaline. So is the fluid found in herpes, eczema, pemphigus, &c., vesicular diseases preceded by more or less congestion of the skin; and it is remarkable that the contents of *sudamina*, which unlike these are preceded by no congestion, are acid, being also destitute of albumen, which is found in the others. Although sudamina are usually accompanied by excessive sweating, cases of typhoid fever are sometimes met with where this is not the case.

Still more remarkable is the difference of the reactions of the various fluids found on *mucous membranes*, giving rise to considerable chance of error. Throughout their whole extent, they furnish, like the skin, an acid principle, which exists in the transparent fluid, destitute of globules, which they normally separate from the blood; but when this fluid is replaced by one of an opaque appearance and containing globules, secreted under the influence of acute or chronic inflammation, the reaction becomes decidedly alkaline. Few animal fluids are so strongly alkaline as that furnished in coryza; and in bronchitis the acid and alkaline are not unfrequently found together, and yet remaining quite distinct, in their transparent and opaque forms. The mucous membrane of the *mouth and tongue*, too, offers varieties of condition. Examined in the morning, before food is taken, in the vast majority of cases, the fluid covering it is acid, but examined later in the day this is found to be alkaline. In the first case, it is due to the presence of the mucus; in the latter to that of the saliva. The acidity of the mouth is then no indication of a morbid condition of the stomach, occurring as it does in the healthiest persons, and in every variety of disease, and being distinct in proportion to the length of time food has been abstained from, and the secretion of the salivary glands has remained unexcited. The mucous membrane of the *stomach*, examined after death, generally furnishes an acid, sometimes a neutral, but never an alkaline reaction; and this whether it yet contains the remains of food, or whether digestion has been long suspended. How are we to reconcile this with the results of experiments which declare the fluids of the organ to be alkaline, save when stimulated by the presence of food or foreign bodies? This is not the case in *man;* for in the most opposite forms of disease the author has found acidity; and the great majority of matters rejected during life manifest the acid reaction. It is not rare to find this also in the duodenum and upper portions of small intestine; although these are often rendered alkaline by the arrival of the fluids from the liver and pancreas. Throughout the large intestine there is always marked alkalinity. The *tears* and *saliva* have always been found by M. Andral alkaline; and he believes that when this latter fluid has been said to be otherwise, that of the mucous membrane has been mistaken for it. Thus in the very cases furnishing an acid reaction, if we, by means of a sapid body, excite the flow of the saliva, we immediately find this alkaline. "And thus falls to the ground one of the principal arguments which has been adduced in support of the theory which regards glucosuria as resulting from the acidification of the blood or other humours of the economy."

In a state of health, *urine* which has not remained too long in the bladder, and is examined soon after voiding, is always acid, although such acidity may become much enfeebled, or even neutralized, if very abundant drinks be taken without corresponding diaphoresis. Circumstances may render the urine temporarily alkaline, as the taking alkalies, or the prolonged use of exclusively herbivorous aliment. The privation of food, however long, does not remove the acidity of the urine; but it is a curious fact, that in some convalescents we find the urine become temporarily alkaline when they commence a better diet. Nor does disease render the urine alkaline. Multiplied as have been the author's observations upon this point, he has never met with a case in which the urine, from the influence of the disease itself, left the kidneys in an alkaline state; and he feels convinced that the statements which have been to the contrary made are founded in error. It has been said that diseases of the spinal marrow have this effect; but, in fact, the urine never becomes alkaline in these, until the mucous membrane of the bladder is diseased. It is not then an alteration of secretion, but a purely chemical one; the urine becoming decomposed and ammoniacal, from coming in contact with pus and other morbid products. *Pus*, whatever its source, is always alkaline, consisting as it does of the serum of the blood, amidst which special globules are developed; and this, as well as other morbid secretions, never becomes acid, except after long exposure to the air.

The immutability of the secretion of the acid and alkaline principles of the animal fluids is then a law of both their physiological and pathological conditions;

and it must be a very important one, seeing that it persists without any exception, save one of a very temporary character, in respect to the influence of alimentary substances in the urine.—*Gazette Médicale*, 1848, No. 28.

On a New Substance occurring in the Urine of a Patient with Mollities Ossium. By DR. BENCE JONES.

THE urine in this case spontaneously solidified on cooling, but was reliquefied by heat. The substance to which this property was due constituted as much as 67 parts per 1000 of urine. It was quite insoluble in alcohol, and was completely precipitated from the urine by the addition of that reagent. It slowly but entirely dissolved when thrown into cold water; but was much more readily dissolved by boiling water. After boiling for some little time, a gelatinous coagulation took place. The aqueous solution gave an immediate precipitate with nitric acid, which entirely and readily dissolved when heated. Boiling caused no precipitation; but on cooling, the precipitate was again formed. Strong hydrochloric acid dissolved the substance, giving a splendid purple blue solution. It was soluble in caustic potass at 140°, or, after long standing, at the ordinary temperature. From this solution it might be precipitated by excess of acetic acid; but the precipitate was re-dissolved, as before, by heat. When the alkaline solution was boiled, a deep inky blackness was produced by dropping acetate of lead into the solution. If the watery solution was acidulated with acetic acid, an immediate white precipitate fell on the addition of ferro-prussiate of potass.

These reactions announce the substance in question as completely *sui generis*. From ultimate analysis its composition per cent. is found to be C 52·10, H 6·70, N 15·17, O 26, with 1·03 per cent. of sulphur, and ·19 of phosphorus. The presence of the two latter ingredients proves that it is not an oxide of protein, notwithstanding its resemblance in behaviour with several reagents to Mulder's hydrated tritoxide of protein. Its reaction with nitric acid hinders all possibility of confounding it with albumen, which may be separated from it by adding nitric acid, boiling, and filtering whilst hot. It is regarded by Dr. Bence Jones as a hydrated deutoxide of albumen.

In the case in question, there was as much of this peculiar albuminous substance in the urine, as there is of ordinary albumen in healthy blood; so that, as far as the albuminous constituent alone is concerned, the secretion of each ounce of urine was equivalent to the loss of an ounce of blood. The patient, as might be expected, rapidly sank; and on post-mortem examination it was found that the bony structure of the ribs might be cut with the greatest ease. The condition of the bones has been minutely described by Mr. Dalrymple (Dublin Journal, August, 1846).

Dr. Bence Jones remarks that this substance should again be looked for in acute cases of mollities ossium. The reddening of the urine on the addition of nitric acid might perhaps lead to the re-discovery of it. When found, the presence of chlorine in the urine, of which there was a suspicion in the above case, should be a special subject of investigation, as it may lead not only to the explanation of the formation of this substance, but to the comprehension of the nature of the disease which affects the bones.—*Philos. Transact.*, Part i, 1848.

On the Action of the Pancreatic Fluid. By M. CH. BERNARD.

THE author of this paper concludes from his experiments that the pancreatic secretion is essential to the reception of fatty matters into the system. He found that it immediately produces an emulsion, when mixed with oily substances; a property which is not possessed by any other animal fluid. The first action seems purely mechanical; but after a time a further change takes place, the fats being decomposed into their fatty acids and glycerine. In this state the bile, which does

not act on the neutral fats, will readily take them up; and thus a mixture of bile and pancreatic juice, such as is found in the duodenum, has the double power of dissolving the neutral fats and the fatty acids. The author has found that if the pancreatic ducts be tied, no fatty matters find their way into the chyle.—*L'Institut*, Mai 3, 1848.

On the Composition of the Blood in the General Paralysis of the Insane. By M. MICHEA.

1. *Chemical Facts.*—The quantitative analysis offers very valuable results. In the majority of cases the *globules* are increased, in a minority they are in the normal proportions, and in a smaller minority they are deficient. The *fibrine* in the majority of cases maintains its physiological limits, sinks below these in a minority, and rises above them in a still smaller minority. The *solid matters of the serum*, organic and inorganic, are in normal amount in the majority of cases, and notably surpass this in a slight minority. The *organic matters of the serum* notably diminish in a little less than a third of the cases. The *water* is in excess in a slight majority, defective in a large minority.

2. *Pathogenetic Inductions.*—The increase of globules and the absolute diminution of fibrine,—sometimes one only of these changes, especially the first, sometimes both of them,—are the causes of the cerebral congestion, which plays so important an etiological part in this disease. It is a capital condition of the disease, but not sufficient to initiate it, being only the proximate or direct cause of the secondary phenomena. The increase of globules, far from being inherent to the essence of the disease, depends upon many purely contingent or accidental causes, e. g. the male sex, sanguine temperament, strength of constitution, mean age of life, amount of appetite, and activity of the digestive organs. The diminution of the amount of globules sometimes induces attacks of convulsion or catalepsy. The increase of fibrine often coincides with epileptic attacks, and with several other symptoms of acute inflammation of the brain or membranes. The spontaneous diminution or insufficient formation of the albumen of the blood has probably somewhat to do with the serous effusions which so often compress the brain during the later periods of paralysis.

Therapeutical Indications.—Bloodletting and moderate vegetable diet are the most natural means for the prevention of the cerebral congestion in these cases, and for its removal when present. In the cases in which a compression of the brain by accumulated serosity is suspected, and where analysis of the blood reveals a tendency to a diminution of globules, purgatives, not bloodletting, are indicated.—*Comptes Rendus*, tom. xxv, p. 810.

PATHOLOGY.

On Influenza and Cholera. By Dr. MARC D'ESPINE.

DR. MARC D'ESPINE, after describing the epidemic of influenza which prevailed at Geneva during the present year, institutes a parallel between the progress of that disease and the cholera.

1. Influenza is a "*peregrinating*" disease, which has never appeared spontaneously in Geneva, as shown by the history of seven epidemics in 60 years. 2. If seasons and meteorological conditions are not without their influence on the physiognomy of the disease, its general diffusion, and the nature of its complications, they seem to be powerless as regards the epochs of its appearance and duration. 3. It is propagated successively from one country to another, but with varying rapidity in different directions. Thus, latterly, it has more rapidly extended itself from Paris to Marseilles, than from Paris to Geneva. 4. Thus far influenza and cholera agree; but a first difference between them is, that while cholera seems to radiate from

towns to the adjacent country, as if, to develope its influence, it required agglomerations of people, influenza seems to act during its route just as easily upon the scattered inhabitants of rural districts as upon those of crowded towns. 5. Both diseases, contrary to most epidemic affections, may attack the same individuals several times. 6. While cholera attacks rather more males than females, influenza attacks a decidedly larger proportion of the latter. 7. Children are generally spared by both diseases. While cholera commits great havoc among the aged, influenza especially attacks those between the ages of 20 and 40; but this difference becomes effaced when, instead of the number attacked, we count the mortality; for as influenza is seldom fatal before 50 or 60, like cholera, it carries off a large proportion of the aged. 8. The influenza, like the cholera, is a general disease, affecting the entire organism, and its physiognomy is characteristic enough to enable us to distinguish it from other acute diseases. Yet it approaches nearest to catarrhal affections, just as cholera does to acute diseases of the digestive organs. 9. Influenza is scarcely ever mortal in its simple state, becoming so from complication with thoracic inflammations. 10. An epidemic of influenza is not accompanied with any diminution in the number or mortality of the ordinary diseases of the season and place. During the prevalence of cholera at Paris in 1832, the number of deaths unconnected with it was just the same as if it had not been present. 11. Although influenza and cholera are diseases of very different severity, their mortuary effect does not vary so much as might be supposed. The epidemics of influenza in 1837 and 1848 nearly doubled the mortality of the populations on which they fell, which is much about what the cholera did in Paris in 1832. It is true the cholera lasted, not two months like the influenza, but six; and though causing one death in every two, attacked only one in twenty, while the influenza attacks one half of the population. 12. While the influenza may appear several times without necessarily being followed by the cholera, this last would seem to be generally preceded by it.—*Gazette Médicale*, Nos. 20 and 21.

On Diseases of the Heart in Birds. By M. RAYER.

M. RAYER, in a note recently read at the Académie des Sciences, states that he has long made the diseases of birds a subject of careful investigation; and in reference to what he has observed as regards disease of the heart, he is induced to ask this question: "Is there among birds, mammalia, and especially man, any relation between the activity of the generative function and the production of disease of the heart?" It is founded upon the facts—1. That all the birds in which he has observed disease of the heart are of the male sex, although of every species he has examined a far greater number of females than males.—2. All these birds manifest uncommon generative ardour; such are the common cock, the cock pheasant, the domestic pigeon, and the musk duck.—*Gazette Médicale*, No. 25.

On Diabetes Mellitus. By M. MIALHE.

CONTRARY to the opinion of those who believe that diabetes is due to the influence of a particular agent conferring on the patient the power of converting certain elements into sugar, which, arriving thus ready formed into the blood, is excreted by the urine; the author maintains—1. That the conversion of amyloid substances into sugar is not peculiar to diabetic patients, but is a necessary condition for their assimilation. 2. It is effected by a special ferment existing in the saliva, which, from the similarity of its properties to those of *diastase*, he terms *animal diastase*. 3. In all animals, for amyloid substances to become assimilated, they must undergo saccharifaction by means of this principle. Sugar thus introduced into the economy, undergoes decomposition in its fluids; and when it appears in the urine, some cause has prevented such decomposition, and therefore its assimilation. This cause the author believes is a *deficient alkalinity of the fluids;* as prior researches have convinced him—1. That it is by means of the

alkalies contained normally in the blood and the animal liquids, that digestion of the amyloid and saccharine substances is accomplished. 2. As amylaceous substances require to be converted into glucose or saccharine matter by means of animal diastase, before they can be absorbed, so must this be transformed by the alkalis of the blood into other new products,—kali-saccharic acid, formic acid, ulmin, &c. —bodies endowed with very energetic deoxygenating powers, and in all probability intended as a counterpoise to respiratory oxygenation.

In health the normal alkalinity of the blood suffices for this transformation; but when this is not the case, the sugar, undecomposed and unabsorbed, becomes a foreign body, and is rejected as such, not only by the kidneys, but by the whole secretory apparatus. Man has naturally his blood in an alkaline state, but the constant introduction of acid by means of aliment would lead to the predominance of this, were it not counterbalanced by special secretions of an acid nature, as the sweat and urine. Other secretions, are, however, alkaline, as saliva, tears, fæces, &c., and it is by the due balance of these that health is maintained; and any defect of such equilibrium, produced by food, medicines, or temperature, may induce disease. Normally alkaline blood may become vitiated by (1) the ingestion of acids, (2) the too exclusive use of azotized articles of diet; and (3) by defective cutaneous transpiration. In diabetes thus originating, the author proposes the administration of alkalies, and the favouring of an increase of the sweat by means of diaphoretics. He furnishes the particulars of a case produced by the ingestion of acid subtances, and rapidly recovering under this plan of treatment.

[M. Mialhe's explanations of the manner in which the various physiological and morbid changes are produced in the economy, are frequently highly ingenious, though somewhat too purely chemical. We are not aware that he has, in the present instance, based his theory upon any analytical proof of the altered condition of the blood—an alteration which M. Andral, in the paper quoted under the head of Chemistry, entirely denies.]—*Bulletin de l'Académie*, t. xiii, 1224-30.

On the Signs of Death. By M. BOUCHUT.

THE Académie des Sciences has recently decreed to M. Bouchut the prize founded by M. Manni in 1837 for the best essay on the distinctive signs between real and apparent death. His observations and experiments lead to this conclusion: that all varieties of *apparent death*, and especially such as are due to syncope and asphyxia, however much their symptoms may differ, present the common character of the *persistence of the heart's pulsation, audible to auscultation.* A great variety of additional observations made by the commission appointed to consider the essay, and reported by M. Rayer, fully confirm this conclusion. According to M. Bouchut, then, the *certain signs of death* are *immediate* or *remote*. The first consist in, 1, the prolonged absence of the sounds of the heart; 2, the simultaneous relaxation of the sphincters; and 3, the sinking of the globe of the eye, with loss of the transparency of the cornea. The first of these is alone regarded by the commission as conclusive. The *remote* signs are: 1, cadaveric rigidity; 2, the absence of muscular contractility under the influence of galvanism; and 3, putrefaction. These are admitted by all medical legists.—*Gazette Médicale*, 1848, No. 23.

On Cysts of the Epididymis, Testis, and Appendix of the Testis. By M. GOSSELIN.

ALTHOUGH cysts in connexion with the spermatic cord have been frequently described under the term *encysted hydrocele of the cord*, those developed in the testis and epididymis have been noticed only by very few authors. M. Gosselin believes that they may be most conveniently considered separately, according as they are *large* or *small*; the former giving rise to certain difficulties in respect to diagnosis and treatment, the latter being chiefly interesting to the anatomist and physiologist; the one, too, having connexions with the spermatic passages, which the other has not.

1. *Small Cysts.*—These are of very common occurrence on the surface of the testis and epididymis, varying in size from a pin's-head or millet-seed to a pea, their favorite locality being the convex surface and free extremity of the epididymis. They are sometimes sessile, and sometimes connected by a small pedicle. On the testis they are placed between the serous membrane and the tunica albuginea, the closeness of texture of the epididymis preventing this being easily demonstrated there. Even injection of the testis with mercury shows no connexion with the seminal passages; and although their contents, sometimes serous, are at others opaline, yet do they never consist of semen, as proved by the microscope. Sometimes they are so distended with fluid, and their envelope is so dense, that they then acquire the hardness of a solid body. At others they burst; the little sacs becoming afterwards hypertrophied, so as to resemble small pedunculated polypi. Their increase in size is very slow, and never extends beyond that of a pea. These cysts are never found before the period of puberty; are even rare and very small until after 40; while after this age they are found in two thirds of the testes examined, being also then multiple, thicker, and larger, and in old men often provided with fibrous pedicles. They are found both in the healthy and diseased states of the testis, without these last seeming to influence their development.

By Morgagni, Hunter, and their successors, these cysts have been regarded as *hydatids;* but this has only arisen from the loose manner in which this term has been employed, the microscope proving them not to be so. The supposition of Sir A. Cooper, that they may be dilatations of seminiferous vessels obstructed in some part of their course, is also void of foundation—both the contents of the cysts and their frequent anatomical position opposing it. M. Gosselin regards them as new formations closely connected with the diminished activity of the testis. Nature, he says, has endowed this organ with a secretory *molimen,* which, when not employed in the production of semen, gives rise to accidental productions; and, perhaps, this may explain the fact of the seat of election of these cysts being the head of the epididymis, where so frequently a portion of the spermatic vessels are in the progress of age obliterated. Moreover, do we not observe something analogous to this in other parts of the economy? The *ovary,* which in so many particulars resembles the testis, has also its temporary functions, and as life advances, its vesicles become obliterated and itself fibrous; and it is even yet more liable than is the testis to the formation of cysts, several of which, especially the smaller ones, are not mere developments of its vesicles. On the surface of the ovaries of old women, these are often found; and we may, perhaps, establish for them the same distinction, in respect to the ovary as the testis, into *large* and *small* cysts, the former being developed at the expense of the primary elements of the organ, the latter being accidental productions, independent of its proper structure.

2. *Cysts of the Appendix of the Testicle.*—As the *appendix* is itself very imperfectly known, and is even indicated at all but by few anatomists, the author goes into some details respecting it. It is a small fibro-cellular mass (sometimes containing a little fat) met with, at all ages, on the tunica albuginea, just below the head of the epididymis. Its volume hardly ever exceeds that of a pea, and may be less. It adheres to the tunica by one extremity, which sometimes takes on the form of a thin pedicle. Sometimes it is placed nearer the epididymis, but it is always rather an appendix of the serous membrane of the testis than of the epididymis. Its condensed cellular substance is surrounded on all sides by serous membrane, whose secretory surface it seems to be intended to increase, so that it might be regarded as a development of subserous cellular membrane analogous to the appendices of the peritoneum. The appendix may become the seat of various alterations, and, among others, of cysts, for which it has indeed been mistaken, especially when its tissue has been infiltrated in coexisting hydrocele. Morgagni mistook it for hydatids; and although Huschke has demonstrated the existence of the appendix, he, too, confounded it with hydatids. It is true it may become the seat of these; but very rarely.

3. *Large Cysts.* These have been described by the English surgeons, especially

Brodie and Curling, but so much neglected in France, that the author enters into details respecting them, into which we need not follow him. He gives some interesting cases which have fallen under his notice, either in the hospital or dissecting-room. Those large cysts that he has examined have been found all to contain spermatozoa; and he expresses his suspicion that the reputed cases of *hydrocele*, the fluid of which has been found to contain *spermatozoa*, have been nothing else than those large cysts, mistaken for the proper serous cavity of the testis. In one very large cyst which fell under his own notice, he found that when the fluid was drawn off, the proper cavity of the tunica vaginalis remained unopened. Spermatozoa might also be found in the fluid of hydrocele, he adds, if the cyst ruptured and discharged them into the tunica, or if, coexisting with a hydrocele, it was pierced by the trocar during the operation.

Still, however, the difficulty presents itself as to how these spermatozoa obtained admission into the cysts themselves. Examination and injection have detected no communication. The author cannot accept the hypothesis of their transudation; nor that somewhat hesitatingly advanced by Mr. Paget (Med. Chir. Trans. vol. 27) of their formation within the cysts themselves—seeing that so elaborate a secretion as the semen could never be formed without its special apparatus. More than this, at the very time these cysts contain spermatozoa, these may be absent in the spermatic passages themselves. He suggests, then, that there may have been originally a rupture of some of the seminiferous vessels, perhaps consecutive to their obliteration in the head of the epididymis. The semen is effused, and around it gradually forms an accidental membrane, which encysts it, and of itself forms fluid in addition to that already present. At the same time the ruptured canals, compressed by the development of the tumour, became cicatrized, and so obliterated that not a trace of the original alteration remains.—*Archives Générales*, tom. xvi, pp. 24-42 and 163-181.

On the Anatomy of the Enlarged Thyroid Gland in Bronchocele. By Professor ECKER, of Basle.

OF this very important communication, we can only give the principal heads.—The author distinguishes two principal varieties of bronchocele, which may occur separately, but are frequently combined. These are (A) *struma vasculosa*, in which the vessels are chiefly concerned; and (B) *struma glandulosa*, which consists essentially in alterations of the closed glandular vesicles.

A. The first of these seems to originate in simple congestion, which may be only temporary; but if it becomes permanent, the capillaries and smaller arteries become aneurismatic and varicose, undergoing dilatation to three or four times their usual diameter. Sometimes the dilated portions seem to become detached from the rest of the vessel, and to form cysts containing blood; a mass of altered nearly-colourless blood-corpuscles, adherent to each other, being often found in their interior. When the disorder has advanced to this stage, the gland-vesicles have usually disappeared. Of these changes, hemorrhage and exudation are frequent results. When blood is extravasated, it frequently becomes surrounded by a sort of cyst, as in apoplectic extravasations in the brain, and undergoes changes similar to those occurring in an apoplectic clot; this condition forms one variety of what has been termed *Struma cystica*, which depends (as we shall see) on various pathological changes. Another variety is the result of exudations, which are sometimes diffused, and sometimes collected in masses which become encysted. In the midst of these exudations, the components of which are generally primitive fibres and elementary granules, with blood-corpuscles, we are assured by Professor Ecker, that new vesicles or cysts frequently spring up, similar in all respects to those of the normal gland, but inferior both in size, and in the degree of development of their contents. These *exogenous* vesicles, according to him, constitute the only true hypertrophy of the gland. Another frequent alteration in the vessels is the obstruction and

obliteration of the smaller arteries and capillaries, by the deposition of calcareous matter in their coats.

B. The second primitive form primarily consists in the dilatation of the gland-vesicles, apparently from the retention of the secretion, which, through inactivity of the absorbents, is not taken back into the current of the circulation. The vesicles at first are simply enlarged; and are filled with complete cells distended with colloid matter, apparently of an albuminous nature. There are also to be seen more irregular masses of colloid matter, quite transparent, and containing numerous cells and nuclei, sometimes with crystals of cholesterine. As the disease advances, the cell-structure disappears, the walls of the vesicles and the intervening fibrous stroma become absorbed, so that their cavities coalesce, the vessels at the same time becoming obliterated, and thus is produced a third form of *struma cystica*. This change may continue until the whole gland becomes transformed into transparent colloid masses, in which few septa and no blood-vessels are to be seen.

These two primitive forms of bronchocele are frequently combined, so as to produce all sorts of intermediate and complex varieties. Thus, the congestion and dilatation of the vessels may supervene on the glandulose form, and may occupy the intervening parts of the gland between the vesicles; and dilatation of the vesicles may supervene on changes more immediately connected with the vascular system. In either case the result is similar; and it is, therefore, impossible to find a sharp and definite boundary-line between the two primitive forms.—*Zeitschrift für Rationelle Medizin*, 1847, Heft ii; and *Edinb. Monthly Journal*, Aug., 1848.

PRACTICAL MEDICINE.

On Hysteria. By PROFESSOR FORGET.

Professor Forget terminates a series of papers upon hysteria, illustrated by numerous cases, with the following conclusions:—1. Hysteria frequently attacks the poor and miserable classes. 2. It often affects persons of robust and sanguine constitution. 3. It is very rare in the male. 4. It is frequently produced in the absence of any lesion, whether material or functional, of the organs of generation. 5. Spinal irritation, considered as a distinct and necessary cause of hysteria, as described by authors, is an error of fact. 6. It is probable, but not proved, that hysteria is not unfrequently connected with lesions of the uterus, and especially the ovary. 7. It may be sometimes produced secondarily to a lesion of the blood or certain viscera; but it is frequently primary, and the product of a nervous diathesis or special neuropathy. 8. Such diathesis is indeed necessary for the manifest action of hysteria, even when it is secondary. 9. The determining cause of hysteria is usually a moral one, or a physical one of very variable seat. 10. The characteristics of hysteria are very diversified, and derived rather from the coexistence of morbid phenomena, than from the presence of any particular symptom. 11. The hysterical attack is generally accompanied by sensitive and convulsive phenomena, to which is frequently added intellectual disturbance. 12. It frequently puts on the characters of diseases different from itself. 13. It is one of the most obstinate of diseases, and very liable to relapse: so that mere suspension of the symptoms must not be mistaken for a cure. 14. It is a real and special disease, being essentially a neuropathy, whose nature is unknown; and its present name should be changed, as erroneous. 15. Its treatment consists in the removal of complications, and in the administration of means for acting on the nervous condition itself. 16. No absolute antihysterics exist; sedatives, tonics, stimulants, or antiphlogistics being required in different cases. 17. Its radical cure should be sought in improved hygienic conditions, a perversion of which is almost always a cause of it.—*Gazette Médicale*, No. 50.

On Œdema in Phthisis and other Emaciating Diseases. By M. Piedagnel.

Dr. Lewis, in a communication from Paris addressed to Professor Mütter, furnishes the results of several observations upon this subject, made in the wards of M. Piedagnel. Serous infiltrations of the lower extremities, which ordinarily result from some mechanical obstruction to the circulation, occur in other cases without the existence of any appreciable lesion to account for them. This is especially the case in those circumscribed infiltrations which occur towards the termination of pulmonary affections, and of adynamic fevers producing great emaciation. In many of these cases a *single limb*, and that almost exclusively the *left* one, is the seat of infiltration. M. Piedagnel has shown in numerous instances, some of which are recorded in the present paper, that this arises from a mechanical obstruction of the iliac vein, usually the left, by its corresponding artery or by both arteries. When very great emaciation occurs, the fat surrounding the aorta and its primary divisions is absorbed, and the left iliac vein, which had been previously separated from the vertebral column by a layer of adipose tissue, is brought in direct contact with the bone, just opposite the fourth lumbar vertebra, sometimes the third, against which it is forcibly compressed by the common iliac artery. For while the right iliac vein pursues a straight course nearly parallel to the artery, and thus escapes compression, the left passing between the left iliac artery and the spine, is compressed against the latter, leading to the formation of an obstructing coagulum, and infiltration of the corresponding extremity. Other cases are alluded to, in which the infiltration seemed to be due to the compression of the femoral vein by enlarged inguinal glands.—*Philadelphia Medical Examiner*, No. 41.

Clinical Observations upon Anæsthesia. By Dr. Beau.

The object of this paper is to exhibit the fact that in certain forms of disease a condition of anæsthesia is present, which has hitherto for the most part escaped attention.

1. *Saturnine Intoxication.* Writers treating upon the deleterious influence exerted by lead upon the system, have noted anæsthesia as an occasional and rare occurrence in individuals who have been long exposed to its operation; and M. Tanquerel states that of 2160 persons suffering under various effects of lead disease, only eleven offered symptoms of deficiency of general sensibility. Nevertheless, anæsthesia is an habitual, if not an essential symptom of impregnation of the system by this metal. M. Beau's attention was at first accidentally directed to the subject, on observing an insensibility to feeling and to pain in the thigh of a painter, and an insensibility to pain in other parts of his body. Trying the experiments upon other individuals similarly circumstanced, he found the same result,—pricking, pinching, &c. the surface exciting no sense of pain; and he has now investigated the fact in thirty cases, some being very slight examples of colic, &c. and others of very short duration.

In these cases there are, however, *two varieties* of anæsthesia, viz. insensibility to touch, and insensibility to pain, better termed *analgesia.* The first of these is by far the least common, having been met with in only four of the cases. It is, too, usually partial, extending only over a small portion of the surface; but it announces a far more serious lesion than analgesia. This last is *constantly* found in persons suffering from lead poisoning in any of its degrees. We must, however, not content ourselves with asking the patient whether he feels, but confine our question to the sensation of *pain.* Parts which are thus insensible to pain are so also to *tickling.* This form of anæsthesia may affect the entire surface, being, however, most remarkable in the extremities, and especially the upper ones. It may extend even to the mucous membranes, and especially those which are normally endowed with great sensibility—as the uvula, isthmus faucium, nares, or conjunctiva—any of which parts may be tickled without the usual consequences, the patient being still quite conscious of the mere contact. The anæsthesia disappears

as the cachectic colour is removed, and the appetite restored; with a slowness proportionate to the age of the patient, or to the severity of his disease. It has disappeared as soon as the sixth day after treatment, and at other times not until the twelfth or fifteenth. With this anæsthesia to excited pain, violent pain of a spontaneous character, as colics, &c. may exist.

2. *Hysteria.* M. Gendrin, in 1846, first stated anæsthesia to be a constant symptom in this disease; but he did not distinguish between insensibility to pain and insensibility to touch, the latter being very rare, and only observed in very bad cases.

3. *Hypochondriasis.* Anæsthesia is to be observed also in this disease when well marked and of long standing. Besides the diseases mentioned, it will probably be found in *scorbutus* and *pellagra* (both of which not infrequently terminate in paralysis), and in various other forms of colic besides that from lead. The *nervous delirium* of Dupuytren following traumatic affections is attended with this anæsthesia, and the insensibility to surgical operations manifested by some of the insane may be similarly explained; and these views throw some light upon the ease with which religious fanatics have at different periods apparently borne the most horrible sufferings.

The above pathological considerations lead to the physiological separation of the sense of touch and of pain; and this is susceptible of proof by experiment, for after a blow, cut, &c. the pain is distinctly posterior to the perception of the injuring body. "The sense of pain may, so to speak, be considered as annexed to that of touch, and thus is the first to disappear when the innervation does not possess its normal intensity. If the innervation is subjected to still farther diminution, then the sense of touch disappears in its turn, and the insensibility is complete. The object of the sensation of touch is to inform us of the presence of bodies in general, while that of pain has the no less important office of advertising us of the contact of disorganizing bodies." We may then say that there is a paralysis of the sense of pain, just as of the sense of touch, of the special senses, or of the motor powers.—*Archives Générales*, tom. xvi, pp. 5-24.

Since publishing the above communication, M. Beau has found that *typhoid fever* may be added to the diseases accompanied with anæsthesia, which persists even during convalescence.

He has also remarked that *analgesia* in the various cases is more observable in the erect than in the recumbent posture, after exertion, e. g. mounting stairs, and indeed after any action, whether physical or moral, which produces a temporary diminution of strength.—*Gaz. des Hôp.*, 1848, No. 26.

On the Use of Sulphur Baths in Asthma, with some Considerations on the Nature and Symptoms of this Disease. By E. CONTIN.

SULPHUR, which has been administered from time immemorial in catarrhal affections both internally and externally, is an exciting agent, the effect of which is manifested by increased heat of the skin and acceleration of the pulse. This property necessarily exists in sulphur baths; besides which the temperature of the water, when raised above that of the body, adding its action to that of the sulphur, may be such as to produce a febrile reaction which greatly favours the revulsion we would establish. To this double effect on the skin, is added that of the fumigation and medicamented inhalation to which the patient is exposed, when immersed in these sulphurous vapours.

These baths produce a restorative and tonic action on the healthy subject. The use of simple baths is followed by diminished sensibility to cold. For asthmatic patients, besides these prophylactic effects, which are of themselves most valuable, sulphur baths occasion immediate relief by inducing abundant expectoration of dense mucus, either in the bath itself, or immediately afterwards, and making it more easy in the succeeding day and night. According to M. Trousseau, another remedial agent possessed by these baths is sulphuretted hydrogen gas, which exerts

a stupefying action by diminishing the pulmonary excitement, which gives rise to the bronchial discharge. But as this calming property is greatly exceeded by the exciting effects induced by these baths, their employment is not advisable for persons suffering from fever, or those affected by hemorrhage or mucous discharges accompanied by a febrile condition. Are we to ascribe the happy effect of sulphur baths to the cutaneous revulsion, or to the fumigation and inhalation? All probably contribute to this result, which must be rather referred to a combination of many circumstances, than to any specific action of the mineral principle.

From 125 to 250 grammes of sulphuret of potassium should be used for one bath, whose temperature may vary from 25° to 30° C. The immediate effect of the first baths is to produce a greater dyspnœa, which is to be attributed, 1, To the warm and humid air surrounding the patient; 2, To a greater peripheral pressure. Occasionally these baths are insupportable beyond a few minutes; although by a slight effort of resolution, and through habit, the inconvenience attending them will soon be disregarded, and deemed of no consequence in comparison with the relief afforded by the increased facility of expectoration.

Asthma is almost always complicated, as for instance, in 18 cases out of 23 observed by M. Contin. In 13 cases there was gastric complication, most generally bilious or pseudo-bilious.

The success of the treatment depends on the removal of these complications, since the baths will remain almost wholly inefficacious as long as the anorexia and the other derangements of the digestion remain in force. Considered in this point of view, emetics have at all times been esteemed powerful adjuncts to the treatment, since they are administered with equal success against a saburral condition, and in the many varied, and still unknown neuroses connected with the process of the digestion, which combine to form the derangements classed by the ancients under the name of dyspepsia, and by the moderns under that of gastralgia.

The following is the formula employed by M. Beau for an adult:

Tartar emetic, 10 centig.,
Ipecacuanha, 1 gram.,

in a simple mixture; a spoonful to be taken mixed with warm water, every ten minutes. This generally acts freely as an emetic and purgative. Other affections of the bronchial mucous membrane are rapidly cured by the above mixture, as, for instance, common catarrh (tracheal, laryngeal catarrh, &c.), which, although very common in private practice, is rarely observed in hospitals, where patients do not generally come until they can no longer continue their usual avocations.

M. Beau has, however, found very great benefit during the last few years, from the use of sulphur baths in this affection. Some very serious cases have been rapidly improved without overcharging the stomach of the patient with emollient drinks, and without having recourse to opiates, which, even when administered in small doses, usually, and almost inevitably, destroy the appetite.

Sulphur baths have also been frequently employed with the most unhoped for success by M. Beau in cases of incipient pulmonary phthisis.—*Gazette Médicale*, 1848.

On Delirium in Pneumonia. By M. GRISOLLE.

THIS phenomenon is of importance, not only because it may arise from different causes, offering opposite indications, but also because it may, in some cases, give rise to the belief in the existence of a cerebral disease, when in fact the affection is seated in the lungs. And the necessity of a complete examination of the organs is shown by the fact, that where this has been neglected, it has not uncommonly happened that persons have been carried to lunatic establishments on account of a temporary mania, developed during the acute stage or the resolution of pneumonia.

A third part of such cases manifest themselves in persons addicted to drinking; and it has been said that pneumonia of the apex, especially, gives rise to this symptom; but of 27 patients exhibiting delirium, M. Grisolle has found the apex

affected in 9, the base in 14, and the middle lobe in 4; and MM. Andral and Briquet have made similar observations. It may, however, be stated, that double pneumonia is more likely to give rise to delirium than single. It manifests itself more frequently in men than women, in the proportion of 21 to 6, and of these 27 patients, 3 only were less than 40 years of age—the usual age being between 50 and 60. MM. Hourmann and Dechambre state that among the old women at Salpêtrière, delirium is an habitual accompaniment of pneumonia. The symptom generally occurs from the 4th to the 6th day, especially in drunkards, although it is by no means rare to find it at the commencement of the pneumonia, or even preceding it by from one to four days. Lastly, when the progress of the pneumonia has been rapid, it may not exhibit itself until the decline of the disease, or during convalescence.

It may assume various forms, from slight incoherence to furious delirium; and in drunkards there are the hallucinations, excitement, sleeplessness, and tremor, observed in *delirium tremens*. This last form, if not relieved, terminates in coma and death in 4 or 5 days. The autopsy generally exhibits only some vascularity of the pia mater and injection of the cerebral substance, signs of recent meningitis being observable in about a fifth of the cases. The presence of this delirium gives rise to an unfavorable prognosis, not only on its own account, but because of its generally being conjoined with extensive pneumonia; but the prognosis is less serious in the case of drunkards than in others. Of 27 cases, M. Grisolle lost 8.

Although in the *treatment* of the case which has given rise to these remarks, bleeding was not resorted to, in consequence of the age of the patient, the long duration of the disease, and the smallness of the pulse,—antimony and a blister being successfully substituted,—yet it is, in general, far from being contraindicated. When the delirium seems to predominate over all the other symptoms, M. Récamier has given musk, in the form of pills, in doses of from 8 to 10 décigrammes, with success; but M. Grisolle has frequently failed with it. In the cases of drunkards, full doses of opium, continued until sleep is procured, are indicated; with which full doses of tartar emetic may be advantageously combined. Alcoholic drinks are also highly useful, not only for the cure of the disease, but also, as M. Chomel long since observed, for its prevention. We should, therefore, inquire into the patient's habits, and remembering that in drunkards delirium comes on from the 5th to the 7th day, we should, if any trembling of the lower lip and sleeplessness indicate the approach of the complication, at once administer the alcohol, and often the best effects will follow.—*L'Union Médicale*, 1848, No. 9.

On Cerebral Congestion in relation to Hemorrhage and Ramollissement of the Brain. By M. DURAND FARDEL.

THE object of this paper is to direct attention to the important part which congestion plays in inducing these conditions. Its influence may be sometimes judged of by premonitory symptoms, such as giddiness, singing in the ears, numbness of the limbs; still, in 59 cases, 32 of acute ramollissement, and 27 of hemorrhage, the author only finds these recorded in 21. M. Rostan, to whom is due the first accounts of *ramollissement*, regarded the disease sometimes as an encephalitis, and sometimes as a peculiar degeneration analogous to senile gangrene, two very opposite conditions; but the author, who has long had opportunities of observing it among the aged, maintains that in them also it is an encephalitis. In the dissections of 29 such cases which he has published, all the usual signs of congestion of the brain were observable. In the production of *cerebral hemorrhage*, Abercrombie and Bouillaud lay much stress upon the ossified condition of the vessels, and Rochoux regards it as proceeding from a special alteration, a hæmorrhagiparous ramollissement of the cerebral pulp. It is certain that a rarefaction rather than a ramollissement of this tissue does take place, and in almost all aged persons the meningeal vessels have become more friable; but mere diminution of cohesion of the cerebral fibre would never give rise to hemorrhage, and ramollissement may go

through all its stages without doing so. So, too, when hemorrhage takes place earlier in life, changes in the vessels are not observed; and even later, such altered vessels are not to be followed into the substance of the brain, where the hemorrhage really takes place. A fluxion of blood to the part explains all the phenomena; and the author believes that the rigid bony canals in which the veins are placed, cause this organ to be freed of any surplus blood with difficulty. If congestion of the brain or encephalitis occur at an earlier period of life, there is usually some appreciable cause discoverable; but this is not so in old age, when they would seem rather to become developed under the influence of physiological modifications, independently of external circumstances. Life, as age approaches, seems to retreat from the periphery to the centre. First, the functions of the skin and of the senses diminish in activity, and the systems of voluntary motion and of organic life, the secretions and the digestive functions, die, or at least are gradually enfeebled. The lungs, the heart, and the brain, alone preserve their physiological activity, and they alone become liable to disease; and thus it is that the aged die of affections of the lungs, or the brain.

M. Rochoux, in the discussion which followed the reading this paper, observed that he still adhered to the opinion he expressed in 1814, that in the great majority of cases of *apoplexy*, there are none of the so-called premonitory symptoms present; so, too, the great bulk of persons suffering from these, never become the subjects of apoplexy. A vast number of cases of apoplexy are on record, which occurred while the patient presented no circumstance indicative of congestion; and, on the other hand, various employments, the games of children, attacks of epilepsy, &c., give rise to great congestion without inducing it. M. Rochoux believes there is an imperceptible, organic, molecular change effected in more or less of the cerebral tissue, diminishing its power of cohesion and resistance, until, at last, unable to withstand the force of the circulating blood, it becomes torn, and the hemorrhage we term apoplexy is produced. The softening is just the same whether death takes place in an hour, or in several days, and must be regarded as precedent, not consecutive, to the effusion. This *hemorrhagiparous ramollissement* exerts nearly all the action attributed to all the other producing causes of apoplexy, including local or general congestion.

M. Baillarger observed that another consequence may be said to result more directly from congestion of the brain than these mentioned, viz. *general paralysis*. The patient on coming to himself, is found to have some embarrassment of speech, and gradually his intellect fails him. The anatomical changes in general paralysis have especial reference to the periphery of the brain, and we can easily conceive here the direct operation of repeated congestion. In hemorrhage and ramollissement, besides congestion, there must be some local lesion of the organ, which explains why one portion rather than another is attacked.—*Bulletin de l'Académie*, tom. xiii, 944-68.

SURGERY.

On Treatment of Porrigo Decalvans. By M. DEVERGIE.

A GENERAL precept should be laid down, which applies to all diseases of the hairy scalp, however these may differ, viz. *never to cover the head in such a manner as to prevent the access of air*, the contact of which is, in fact, one of our most powerful aids. Hence, covering the head with wigs, false hair, &c. is very mischievous. The skin secretes substances which in the normal state are acid, but which, by being retained in contact with it, become alkaline and irritating, as may be seen on examining the secretions in eczema and other diseases. The scalp should be covered then only by some pervious material, which will not confine the secretion.

A second general precept is, the maintenance of *entire cleanliness*, which is best

accomplished by the employment of different washes, according to circumstances. M. Devergie prefers those formed by the addition of two or three spoonfuls of *chloride of soda* to a quart of water, or of 10 parts of *subcarbonate of soda* to 500 of water, employing them every other day.

When in *porrigo decalvans* the patches are of a reddish colour, or puffed up, emollients and cataplasms for two or three weeks are indicated; and afterwards resolvent ointments, such as, *tannin* 2 to 4 parts, *camphor* ¼ to ½, *lard* 30. Then follows the tar *ointment*, to conclude with the *carbonate of copper* ¼ or ½, to 50 of lard; varying the strength for different patients, and for different spots of the same patient. When the hairs begin to reappear, tonic and stimulating ointments, such as that of Dupuytren or of cantharides, are required. Lastly, as the disease declines, *corrosive sublimate* lotions (1 part to 999), should be used four or five times a day, and every fifth day a gentle application of *nitrate of silver* (1 part to 10) should be made to the parts that have been affected.—*Gaz. des Hôp.* No. 27.

Blistering the Eyelids in Affections of the Cornea. By M. VELPEAU.

IN affections of the cornea, attended either by interstitial deposition of lymph or the formation of pus threatening penetration, M. Velpeau has found blistering the eyelids extremely useful during the last eighteen years. In conjunctivitis or iritis the practice is of no great utility; but in keratitis and inflammation of the anterior chamber, no means is so efficacious. It dissipates the sanguineous engorgement, prevents or arrests the plastic effusion, and favours its absorption if already deposited. It cleans the ulcers, and is very efficacious in preventing suppuration and softening of the organ. It is an error to suppose it is mischievous when employed in the acute stage; but its use requires some precautions. The skin of the eyelids is to be first rubbed with vinegar, and they are to be closed without being contracted. The blister is so applied as to secure contact in every part, a pledget of lint laid over it so as to fill up the orbital cavity, and the whole to be secured by a bandage. When the blister is removed next day, the eyelids are found more or less swollen, and it is not until this has subsided that we can judge of the effect. In this way it may be renewed three or four times.—*Gazette des Hôpitaux*, No. 80.

On Cauterization as a Preventive of Purulent Infection. By M. PHILIPPEAUX.

M. PHILIPPEAUX adduces a new case in proof of the doctrine held by M. Bonnet, of Lyons, that wounds which have been cauterized, or which succeed to the fall of an eschar, will not give rise to phlebitis or purulent infection. The latter affection is of more frequent occurrence than heretofore, probably from the modern surgeon employing cutting instruments, to the exclusion of caustic. When a wound becomes grayish and œdematous at its circumference, furnishes a sanious or putrid pus, is accompanied by fever and intense thirst, with fetid stools and great exhaustion, M. Bonnet deeply cauterizes its surface; and the results he has hitherto obtained have convinced him that this is a powerful means of arresting the development of the general symptoms, and localising the malady. But can the disease be thus arrested, when fever, with shivering, has become fully developed? In this stage the plan has been found more frequently unsuccessful than not; but M. Philippeaux now details the particulars of another case, in which cauterization of the wound with equal parts of chloride of zinc and flour, was attended with success after the full development of the disease.—*Gazette des Hôpitaux*, No. 57.

Rigidity of the Hand after Fracture of the Forearm. By M. HERVEZ DE CHEGOIN.

THE author observes, that owing to the compression used in the treatment of fractures of the forearm, and the prolonged extension of the fingers, the patient often remains with an impaired mobility of the hand and fingers, that places him in

a worse state than if no treatment whatever had been employed. Indeed, on this account has M. Velpeau abandoned the use of apparatus in these fractures.

The manner in which the author treats these cases, so as to prevent rigidity, is as follows:—In those in which there is no displacement he merely lays the forearm on a somewhat solid surface; but where there is a constant tendency to the reproduction of the displacement, he places it on a very thick and firm cushion, terminating opposite the bend which separates the hand from the arm—bringing it more or less near this bend, accordingly as the lower fragment projects forwards or backwards; so that in the first case it is this fragment which is brought against the cushion, and in the other, the lower extremity of the upper fragment. The hand is allowed to hang down in front of, and below, the cushion. On the twelfth day a splint and compress four inches long are substituted, and kept on by two broad tapes tied over a single pad on the back of the arm, so as to avoid all constriction. If there is displacement towards the interosseous space, a compress may be there interposed. Entire consolidation takes place towards the thirty-fifth day, and, in consequence of the absence of injurious compression, neither gangrene, muscular atrophy, nor adhesions occur, and tedious convalescence and imperfect recovery are avoided.—*L'Union Médicale*, No. 46.

On Tertiary Syphilis. By Dr. Gamberini.

Some of the doctrines so positively laid down by M. Ricord, and at first so readily received, are found ill to bear closer examination. Dr. Gamberini maintains, as the result of prolonged investigation in a large hospital, that the arbitrary division of the constitutional symptoms into secondary and tertiary is a mere play upon the words, and a confusion of what was simple; and that the attempt to treat the one class exclusively by mercury, and the other by iodine, is mischievous. In fact, in both the secondary and tertiary symptoms, mercury sometimes fails; but in the great bulk of even the tertiary symptoms, it is the preferable and even indispensable medicine. It is the same constitutional lues exhibiting itself on different points of the economy, in the secondary and tertiary forms; these may coexist, may mutually displace each other, and may both be propagated hereditarily. He admits, however, that the French surgeon has done good service, by showing the great utility of iodine in cases wherein mercury is inadmissible, or has proved unavailing.

He describes the various so-called tertiary symptoms at some length, as well as the treatment he has found efficacious; but we have only room for one or two of his remarks. Speaking of that distressing one, *severe pains in the bones*, he says that, instead of its being attributed, as it often is, to the employment of mercury, it should frequently be said to result from this drug having been so inefficiently employed as only to modify, not to remove, the syphilitic poison; and the very patients who have been sent into the hospital, supposed to be suffering rather from the drug than the disease, have recovered speedily under the use of additional quantities. The author, referring to his experience in 200 of these cases, states that although in those cases in which mercury failed iodine succeeded, these medicines thus supplying each others' places, yet that the undoubted superiority was due to the mercury. In respect to *periostitis*, he says that mercury cures the disease much more rapidly than iodine, about three fourths of the cases being amenable to it, leaving a fourth for the iodine, among which are the cases that have resisted mercury.

In so strongly recommending mercury, Dr. Gamberini always supposes the *vapour bath* to be the same time employed, as, without this, the medicine generally produces some ill effect, and especially salivation. The forms he prefers before all others are friction with the *ung. hydr.*, and the *deutochloride* in diaphoretic drinks. Out of some thousands treated by the one or the other of these, few have been the cases in which they have not succeeded. *Iodide of potassium* he regards as a succedaneum of mercury; and it is especially useful in cases for which mercury

has been given in vain. It may itself cure the syphilis, or so modify it, as to render it amenable to mercury resorted to again. Numerically, mercury, when no other treatment has been tried, will cure about 80 per cent., iodine about 20 per cent., of the cases admitted.—*Bulletino delle Scienze Medicale*, vol. xii, pp. 96-148.

On Cataplasms for Tumours of the Breast. By M. TANCHOU.

M. TANCHOU states, that according to his experience cataplasms are contraindicated in all tumours of the breast which do not originate in blows, falls, &c., and which are not of a simply inflammatory character. In certain cases they would seem to act as a touchstone, when we have reason to doubt the nature of a tumour, and to fear its cancerous degeneration. They at first calm the pains, and the patient seems satisfied; but very soon they induce an almost passive engorgement, the part also becoming mottled and sometimes livid. Pains of a new character now develope themselves, shooting towards the shoulder and arm, and the patient by a sort of instinct discards the cataplasm; or perhaps the tumour breaks, and reveals by its appearance its malignant character.—*Rev. Méd.-Chir.*, tom. iii, p. 343.

A New Mode of performing Lithotomy by the Rectum. By M. MAISONNEUVE.

AN interesting case has been recently published, in which the operation performed by Sanson and Vacca was advantageously modified. After placing the patient (æt. 28) in the ordinary lithotomy position, and giving the catheter (with a very large groove) in charge of an assistant, M. Maisonneuve, standing between the thighs, lodged the nail of his left index-finger, passed into the rectum, in the groove of the catheter, just anterior to the prostate. Along this finger he next slid a pointed bistoury, guarded by lint to within a centimetre of its end, and made a small incision through the rectum and membranous portion of the urethra. Still retaining his nail in the groove, he next passed in a double lithotome, with its concavity upwards, and having assured himself of its secure implantation in the groove, withdrew his index-finger, took hold of and slightly raised the catheter with his left hand, while with his right he opened the bladder with the lithotome. The catheter was now withdrawn, and the right hand so turned as to bring the concavity of the lithotome backwards. Next he introduced the index and middle fingers of the left hand above the lithotome, and separated the one from the other, so as to dilate the rectum and protect the sphincter while he withdrew the lithotome, the blades of which, separated fourteen lines from each other, made a bilateral incision in the prostate and rectum. The forceps were then passed along the fore-finger, and the stone removed.

The patient recovered so rapidly, as to be sitting in the yard on the fourth day, and he was exhibited at the academy after a long walk, on the ninth. An urinary fistula still remained when he returned to the country on the seventeenth day, but this subsequently healed. This operation differs from that of Sanson and Vacca by leaving the lower end of the rectum, the sphincter, and the perineum untouched; and this prevention of the exposure of the wound to external influences, places it very much in the same category with the subcutaneous incisions.—*L'Union Médicale*, No. 63.

On the Performance of Operations at Intervals. By M. VIDAL.

M. VIDAL recently read a paper at the *Académie* in which he endeavoured to show that advantage may sometimes result from performing the different stages of operations at intervals of some days. In this way may the economy be better able to allow of the removal of a long-standing disease, and the proceedings of nature are imitated. Thus, in the removal of foreign bodies from the vicinity of important

parts, she protects these by adhesions and thickenings before she forms her external aperture. M. Goyrand, of Aix, has already acted upon the principle in *removing a loose cartilage from the knee-joint;* for, having opened the capsule by an oblique subcutaneous incision, he forced the foreign body into the external wound, and detained it there until the aperture in the capsule behind it was healed up. So also before *opening deep abscesses and cysts,* MM. Begin and Recamier have made preliminary incisions nearly down to the cavity, or have applied caustic, and left the discharge to nature, or have only effected this after protective adhesions have had time to form. Various practitioners have performed *lithotomy* at distinct intervals, first making all the incisions even into the bladder, and extracting the stone on another occasion; but the author is of opinion that the wound should extend down to the bladder but *without opening it,* and be filled up with cerated lint while the condensation of the neighbouring cellular tissue is effecting, forming a kind of organic cement around the passage through which the urine will have to pass. This done, the bladder may be opened, and the stone removed without infiltration occurring. The author adduces several examples in which these principles have been advantageously acted upon in *autoplastic operations;* and refers to their especial importance in cases of recto-vaginal and recto-vesical fistula. The guiding principle is, that it is better to perform several, short, non-dangerous operations, than one long, complicated, and dangerous one.—*L'Union Médicale,* Nos. 14 and 15.

On Cicatrization after the Operation for Staphyloma. By M. SICHEL.

WHEN the crystalline no longer exists, or has been removed, the vitreous body and the hyaloid present a nearly circular black surface, or rather a transparent one, through which the dark colour of the bottom of the eye is seen. In a few days the margin of this surface becomes vascularized by a prolongation of vessels from the circumference of the cornea, which, at very variable periods, encase the wound in a vascular corona, that becomes a true organ of nutrition for the exposed hyaloid, upon the anterior surface of which the vessels deposit a grayish-white fibro-albuminous fluid. This opacity is, however, sometimes only developed later, or it may be absent. Little elevations are also observed along the circumference of the space circumscribed by the vascular corona or in the interstices of the vessels traversing the vitreous body, some being of a rose colour, others gray-red. They are true granulations, which, gradually approximating, become covered by a grayish membrane, whence results the cicatrix. Sometimes this new membrane for some time covers only the circumference of the wound, and the granulations may remain isolated in the middle, communicating only by very delicate vessels with the circumference. During cicatrization, when this is slow, a round blackish interval is left, through which rays of light, if the retina continues sound, allow the degree of vision acquired after the operation to be still retained; but the patient should be forewarned that this will be lost as cicatrization proceeds.

The false membrane, formed of inodular tissue, contracts more and more, so that it may become almost linear; but generally there remains between the lips of the wound a cicatrix formed by a foreign substance, which, from being circular, becomes gradually smaller and more or less transversely oval. The cases in which the lips of the wound, after the operation, approach and come into contact, in consequence of an evacuation of a considerable portion of the vitreous humour, are very rare, and even then a linear cicatrix is not obtained; for the lips of the wound become again distended by the pressure of the aqueous humour behind them. When only the central portion of a staphyloma is removed, a better cicatrix is not obtained; and if the patient does not wear an artificial eye, a too visible deformity is left, and a relapse is to be feared; while, when he resorts to one, the volume of the globe left is too great to allow him to employ it with ease and comfort.—*Annales d'Oculistique,* tom. xix, pp. 22-24.

Treatment of Hemorrhage after Excision of the Tonsil.

M. Hatin having met with a case of most alarming hemorrhage after the excision of a hypertrophied tonsil, which resisted all the usual styptics, an ingenious and successful mode of employing compression occurred to him. He took a long straight pair of forceps, used for the removal of polypi from the posterior nares, and having covered one of the blades with amadou and lint, applied it to the bleeding part, applying the other blade externally to the angle of the jaw, any amount of compression being producible by approximating the handles. M. Malgaigne, to whom the account of the case was sent, refers to a discussion which took place upon the same subject at the Surgical Society. In the case which gave rise to it, M. Chassaignac succeeded, after everything else had failed, in arresting the bleeding by holding a piece of ice by means of a forceps in constant proximity to the bleeding point. M. Guersant had met with three cases, one in the adult, stopped by the actual cautery; one of the two children had a hemorrhagic tendency, and in the other an alarming hemorrhage occurred on the third day. Muriatic acid and honey succeeded best, ferruginous preparations being given internally. In the only case met with by M. Huguier, he found *ice-drinks* suffice. M. Robert doubted the propriety of using ice in these cases, as it has sometimes produced a gangrenous eschar. He recommends that the thumb dipped in alum should be applied to the bleeding point, making corresponding pressure with the fingers externally. M. Monod was acquainted with cases in which the mere keeping the mouth open sufficed to restrain the hemorrhage. M. Malgaigne observes that he has never known hemorrhage follow either excision of the tonsil or of the uvula. Lisfranc, however, met with a bad case of the latter, and arrested the bleeding by compressing the part with a forceps, and applying nitrate of silver to the wound. M. Malgaigne has met with two cases of traumatic bleeding from the fauces. In the one, it succeeded the removal of a tumour, and was arrested by the application of the finger for a few minutes. In the other case it followed the division of an anormal adhesion of the *velum palati*, which completely obstructed the nasal fossæ; and the bleeding was only controlled as long as pressure was kept up. Believing that the obstruction of respiration contributed much to the continuance of the hemorrhage, M. Malgaigne rapidly completed the operation, and the passage of air being now free through the nares, the bleeding stopped as if by enchantment.—*Rev. Méd.-Chir.*, vol. ii, pp 353-41.

Dr. Hamilton, of Buffalo, states that usually a gargle of cold water will arrest the hemorrhage; but that where it fails to do so, no means is comparable with that of exposing the patient's neck, and applying a neckcloth filled with snow exactly opposite the bleeding part. If snow cannot be got, pounded ice or wet cloths may be used.—*Philadel. Med. Exam.*, No. 37.

On Gun-shot Wounds.

The insurrection of last June produced the bloodiest combat that the streets of Paris ever witnessed, the proportion of wounded among the combatants, and of dead among the wounded, having been truly frightful. The mortality has indeed even more than usual resulted from the mere severity of the wounds, numbers dying before they could be brought to the hospitals, or very soon after arriving there; for it is remarkable, that notwithstanding the overcrowding of these establishments, and of the jails with prisoners, no epidemic or contagious disease whatever was generated; and the absence of such fatal complications as erysipelas, hospital gangrene, and tetanus, most advantageously contrasts the medical constitution of the present year with that of 1830. The condition of the *morale* exerted a powerful influence over the mortality of those whose wounds did not forbid recovery, which was considerably greater on the part of the insurgents. Thus, M. Malgaigne states that at St. Louis, where so many of the wounded insurgents were taken, they furnished during the first five days a far larger proportion of deaths than the military (viz. 1 in 6 as compared with 1 in 15), in consequence of their wounds being so much

more severe—the barricades protecting the lower parts of the body, and the chest and head being the parts which chiefly suffered. At a later stage, the mortality, after having subsided in both categories, was as 1 in 11 among the insurgents, as compared to 1 in 42 among the military. This arose from the injudicious conduct of the authorities, who, in spite of the protest of the surgeons, instituted at too early a period the judicial interrogatories, and carried consternation among the wounded insurgents, who up to that time had been treated exactly as the soldiers, and with the same success. The real period of danger, both in private and hospital practice, was found to be as late as the eighth, tenth, or even the fifteenth day, when purulent collections formed, denuded splinters or other foreign bodies irritated the wounds, gangrene opened vessels, or purulent infection occurred. This last was so common a cause of mortality, that it was attributed very generally to the use of poisoned balls by the insurgents—a conjecture, receiving, however, no support from any of the surgeons.

There are so many points, concerning which the best informed surgeons are at issue, that at M. Roux's suggestion a formal debate upon the treatment of gun-shot wounds has been opened at the Academy; and we may briefly notice some of the opinions there advanced or published in the journals since our former article on the subject.

The question of dilating wounds with the knife (*débridement*) seems to be disputed at present in France with as much keenness, as it was in the days of J. Bell and Hunter in our own country; and the impropriety of making precautionary incisions, or indeed incisions of any kind, save for the removal of foreign bodies, or the securing of vessels, is anything but generally admitted, although strongly enforced by M. Baudens. So, too, the old question of *immediate amputation* has its warm partisans and opponents; M. Roux heading the former, M. Malgaigne the latter. The last-named able surgeon should rather be said to be inimical to amputation at any period, save in the case of a greatly-shattered limb; and he adduces much statistical evidence in favour of his opinion. This, however, is rather slippery ground, for his figures show the most extraordinary difference in the results of operations furnished by different surgeons. Thus, while Boucher declared that two thirds of the cases die, and Faure states that after Fontenoy only 30 or 40 cases were saved of 300; Fercoq says he lost but 2 in 60; Percy but 6 in 92; Guthrie but 7 in 45 in New Orleans, and 9 in 47 at Toulouse. The English army in Spain lost but 24 in 291; and at Aboukir and Camperdown no case was lost in 30 amputations. Larrey, looking back at his long career, estimates that he had saved three-fourths of his cases. M. Malgaigne had already doubted the accuracy of some of these brilliant results, when, during the campaign in Poland in 1831, where 80,000 men were in arms, neither he nor his colleagues ever succeeded in saving a case after amputation of the lower extremities. Ribes was struck with the fact, that among 4000 invalids, after the imperial wars, he could find no one who presented a consolidated femur which had been fractured by firearms; but then he adds, nor could he find one patient among them on whom amputation for this accident had been performed—a fact that has not made sufficient impression; for if it is true, that in trying to save a femur fractured by gun-shot we shall lose the greater part of our patients, we may ask, is the proportion cured increased after amputation. Examining the mortality of the Parisian hospitals in 1836-42, M. Malgaigne found that the mortality after amputation for *traumatic* causes amounted to nearly two thirds of the cases, amputation of the leg representing the mean of such mortality, that of the thigh its maximum, and that of the forearm its minimum. From these and other figures, which we have not space to notice, he comes to the general conclusion, that by *seeking to save limbs after gun-shot wounds, we do not run greater risk of death than in amputating them.* M. Roux, on the contrary, believes that military surgeons have been most unjustly blamed for resorting too frequently to immediate amputation, which, he thinks, should be performed when the soft parts are much injured, even supposing the bone or joints are not so. He places no faith in the accuracy of the statistics adduced by M. Malgaigne; and believes that the

great success of some surgeons, or the failures of others, depend upon some circumstances, independently of the mere operation, which, if statistics are to be of any use, must all be accurately stated. Strange series of cases may occur to the same surgeon. Thus, at an early period of his own career, he had 12 successive cases of amputation, all successful; while a little later, he lost 12 out of 36 cases.

Extraction of balls. M. Roux recommends that this should not be attempted, except when they are superficially situated and easily got at; and he has had many opportunities of verifying Hunter's observations, that parts are so much the less liable to suppurate from the presence of a foreign body, as they are more deeply situated. Thus, when bodies work their way from the abdomen to the surface, it is only just as they approach this that they excite suppuration. M. Baudens, in one of his lectures, makes some very good remarks upon the removal of balls. He observes, that when they seem deeply buried in the soft parts, before abandoning all attempts at their removal, we should pass our fingers very carefully over the region of the body, opposite to that at which they entered, and not unfrequently we shall feel them in a position they may be easily cut down upon. Certain precautions are, however, required for making counter-openings. The ball, during its passage, drives and compresses all the tissues that oppose this, but by the *cellulo-fibrous tissue* it becomes surrounded, in the form of an accidental cyst, which, if the ball is not removed, exerts an important office in isolating it, becoming organized around it, and living by means of its adhesions to the neighbouring tissues. This cyst opposes an obstacle to the removal of the ball, and must be completely divided, while the ball is thrust forward by the finger and thumb placed behind it. When there is no danger in doing so, M. Baudens never hesitates removing a ball; and when, shortly after the receipt of the wound, he cannot find it, he postpones all further examination for a month or so, when, the traumatic inflammation having subsided, and the limbs becoming somewhat wasted, the patient will often himself be able to point out the spot where the ball is situated. The finger is the best exploratory instrument, and should be passed into the wound in search of clothing and other foreign bodies, which might produce troublesome suppuration, and do far more harm than the ball. Injuries to the *joints* from balls generally call for immediate amputation; but in some rare instances balls have entered the cavity without injuring the bone, and have continued annoying the patient exactly like a loose cartilage. Balls striking the shaft of a bone become sometimes themselves broken by the collision into two or more portions, which may escape by separate apertures. The presence of a ball in the substance of bone usually leads to a tedious and interminable suppuration; and from this cause old soldiers sometimes find their wounds reopen fifteen or twenty years after healing: so that whenever it can be accomplished without danger, balls so situated should be removed. In some rare cases a kind of osseous cyst is formed around them, and so imprisoned they continue harmless. Owing to the propinquity of the combatants, the balls in the Paris insurrections usually passed through and through; so that the number remaining to be extracted was exceedingly small.

Size of the apertures caused by the ball. No question has given rise to greater divergence of opinion, than that of whether the aperture of entrance or exit is the larger; and when we find competent observers giving, with cases before their eyes, exactly contrary opinions, we are disposed to agree with M. Roux that there is no absolute rule, much depending upon the force, distance, and direction of the impelling power. That the aperture of entrance is less than that of exit, was generally laid down, until M. Blandin maintained the contrary. He believes that the error has been retained so long, from the experiments of Dupuytren and others having been made on inextensible materials instead of upon so elastic a body as the skin. M. Blandin has never met with a case in 1830 or 1848, in which the aperture made by the entrance of the ball was not larger than that of its exit, and M. Malgaigne's observation is confirmatory of the remark.—*Bulletin de l'Académie*, tom. xiii, 1263, 1275; *Gazette Médicale*, Nos. 30, 32, 34, 35; *Revue Médico-Chirurg.*, t. 4, p. 51; *Gazette des Hôpitaux*, No. 56, 59.

MIDWIFERY, &c.

Case of Rupture of an Unimpregnated Uterus, from a Collection of Pus in its Cavity. By Dr. GUZZO, of Naples.

A woman, æt. 34, liable from puberty to uterine pains and irregularities, married, but childless, came under Dr. Guzzo's care in June, 1837, when he found the uterus as enlarged as at the fifth month of pregnancy, and, in a twelvemonth after it nearly had reached the umbilicus, occasional colourless discharges being observed. She continued to live until 1841, the tumefaction still increasing, when, after the use of a purgative, peritonitis was induced, and in a few hours she died. A large quantity of pus was found in the abdomen, and the uterus adhered to its parietes from the pubes to the umbilicus, filling up the iliac and hypochondriac regions, and was covered by the omentum. The cavity of the womb contained an enormous quantity of inodorous white pus, various irregular hypertrophic formations being developed on its inner surface. Its walls were thickened, and contained in their substance tubercular masses, varying in size from an olive to a walnut, some being crude, and others suppurating. Some of these tubercular abscesses were just on the point of opening into the cavity of the uterus, and a rupture had taken place at the posterior surface of the organ.—*Archives Générales*, xvii, 104.

Case of Prolapsus Uteri during Labour. By Dr. WATSON.

THE author was called to a woman (æt. 22), in labour with her first child, who, up to his arrival, had been attended by a midwife. He found the *whole of the uterus prolapsed*, having its surface dry and ecchymosed, and the os being very slightly dilated, thick, firm, and unyielding. Warm fomentation-cloths were kept on the uterus; and the pains, which had already been in existence for some hours, now became so vehement, that the reporter believed that, in so rigid a state of the os, rupture must occur, especially as the pains seemed to produce no dilatation. He therefore made two incisions into it, after which the labour was soon completed, and the organ returned to its proper situation, where it was supported by a sponge and bandage—the woman recovering nearly as rapidly as after an ordinary labour. The pelvis was not remarkably large.—*Philadelphia Medical Examiner*, No. 40.

On the Occlusion and Rigidity of the Os Uteri and Vagina. By Dr. TRASK.

THIS paper may be regarded as an appendix to the elaborate one upon rupture of the uterus noticed in our last. It contains a condensed account of sixty-eight cases with reference to the originals, and will be found a useful contribution upon a subject somewhat hastily passed over in most text-books. We may state generally that the most usual cause of the occlusion is inflammatory action, whether brought on by prior difficult labours, mechanical violence, or other circumstances. The occlusion may be perfectly complete, so that no trace of an os uteri can be found, or it may be incomplete. The orifice may be closed by firm cicatrization or morbid structure, or by a more or less dense membrane. The diagnosis in cases of complete occlusion of the os is sometimes difficult, and examples of obliquity of the uterus have been undoubtedly mistaken for it. The cases assembled warrant the author in his recommendation that interference should not be too long delayed; for while leaving the case to nature is to leave it to rupture of the organ and death, even the employment of abundant venesection, tartar emetic, and other debilitating remedies, is in some cases, owing to the structural character of the obstruction, quite inadmissible, and in others, if too prolonged, will only lower the patients' powers, without obviating the eventual necessity of an operation. Whenever, then, the rigidity does not yield to moderate attempts of this kind, *incisions* should be

resorted to at once, since they are nearly painless, attended with little or no danger, and usually followed by prompt relief.—*Amer. Journ. Med. Sc.*, vol. xvi, p. 95.

Examination of the Throat in Infants.

A mode adopted by the celebrated Gölis seems too often neglected. While playing with the infant, he passed his little finger between its jaws as far as the base of the tongue; the child immediately made an effort as if to vomit, during which, having previously assumed a proper position, he took the opportunity of making the inspection.—*Rev. Méd.-Chir.*, tom. iii, p. 231.

On the Induction of Premature Labour in other cases than Contraction of the Pelvis. By M. Paul Dubois.

M. Dubois relates some of the particulars of the case of a woman advanced 7½ months in pregnancy, who became attacked with so severe a bronchitis, that he was on the point of inducing premature labour, when at last the inflammatory disease yielded. Labour nevertheless did come on, dilatation having, as ascertained by examination, already taken place unpreceded by pain—an argument in favour of interference in these cases.

The modifications induced in the organism by pregnancy are of two kinds, anatomical and functional. Some of these are constant and essential, as all those connected with the increase and development of the uterus; while others are variable, such as the different sympathetic disturbances. Although in the great bulk of cases these last may remain within certain limits, yet at other times they proceed to so dangerous an extent, as to call for the induction of labour. Then there are certain diseases, the severity of which is much added to by the mere fact of their occurring during pregnancy; their aggravation being sometimes quite mechanical, and at others physiological. The following propositions may be laid down. 1. Premature labour is induced with the greater probability of success, in proportion as the morbid states against which it is employed are the more intimately connected with pregnancy, and depend more directly upon it. 2. Success is the more probable, in proportion as the circumstances for which the operation is instituted more generally and more certainly disappear on the cessation of pregnancy, whether induced artificially or spontaneously. 3. The operative procedures must be simple and easy of execution, and must not of themselves add in any way to existing dangers. 4. From the above it may be concluded that, all things else being equal, premature labour is more favorably induced in the morbid conditions *of* pregnancy than in those occurring *during* pregnancy. In the present question, the *viability* of the child does not occupy our attention, as it does in the case of narrow pelvis; for some of the diseased conditions, as obstinate vomiting for example, may occur at a very early period of gestation.

Among the *anatomical modifications*, M. Dubois enumerates the following:—1. *Increased size of the uterus from excess of liquor amnii.* This sometimes gives rise to much general disturbance, severe pain, general anasarca, and intense dyspnœa. Interference is, however, rarely called for; premature labour generally occurring spontaneously. In a case occurring to M. Duclos, in a woman seven months advanced, he evacuated fourteen pints of fluid at several intervals. 2. The uterus may only undergo its normal development, and yet become a source of danger, as when any tumour in part occupies the abdomen, or there is great deformity of pelvis. In these cases, too, labour generally occurs spontaneously. 3. Retroversion of the uterus may call for the induction. It may in some cases be produced accidentally, but generally depends upon some prior existing disposition. It may give rise to severe pain and other urgent symptoms, even in the fourth or fifth month, and these are continually augmented during the development of the organ. Retention of urine is one of the first of these, and the compression of the rectum may induce strangulation. Reduction should of course be attempted, but in some

cases this will be found impossible, and the evacuation of the contents of the organ become necessary. In some cases, the introduction of an instrument into the cavity of the organ has been found impossible, and perforation of its substance has been resorted to, sometimes with success, sometimes with fatal results. 4. Certain copious hemorrhages, due to the implantation of the placenta at or near the os uteri, call for the operation.—*Gaz. des Hôp.*, Nos. 22 and 30.

On the Statistics of the Induction of Premature Labour. By Dr. HOFFMAN.

DR. HOFFMAN observes that, favorable as he is to this operation in appropriate cases, his present statistical investigations convince him that it is resorted to with unnecessary frequency. Thus, in the kingdom of Saxony, with only a million and a half inhabitants, it was resorted to 64 times in the year 1839 alone; and Dr. Ramsbotham has employed it 72 times, which he thinks far too often for any single accoucheur to require its aid. He should bear in mind, however, that Dr. Ramsbotham has one of the largest consultation practices in this immense metropolis. Dr. Hoffman has collected 524 cases, and, as the references to all these are supplied, his paper is one of considerable bibliographical utility. Of these cases, 271 were due to German, 192 to English, and 17 to French practitioners; but when we find he only assigns 3 cases to Americans, we see how defective his researches in that quarter must have been.

The *age* of the mother is recorded in but 146 cases, the youngest being 17, and the eldest 44; in more than one half of the entire number she had reached or passed her 30th year. Of 258 cases, in only 49 was the operation resorted to in a *first pregnancy.* Although the *repetition* of the operation in the same woman must have been no infrequent occurrence, the author finds records of this only in 34 cases, in some of which it was performed three, four, or more times. The *stature* of the women is recorded to have been oftener small than large, as would be expected, from the greater frequency of small and rickety pelves in conjunction with the former. In comparatively few cases has the author found the *indications* for the operation furnished, but justly concludes that, in the bulk of cases, it has been instituted on account of narrow pelvis. In only 68 cases does he find that *preparatory treatment*—such as baths, tepid injections of the vagina, friction of the abdomen, &c.—have been put into force; an omission, he considers, much to be regretted.

In nearly two thirds of the cases, the *mode of operation* is given. Of the more generally admitted of these, the use of *secale cornutum* is recorded in 45 cases, almost entirely by English practitioners. In these, 23 children were born alive, 15 dead; and, of the whole 38 noted, 12 others died within 36 hours after birth. The *Hamiltonian* plan of detaching the membranes, modified by several Germans, is exceedingly tedious. The introduction of *prepared sponge* is a favorite mode with the Germans, and was employed in 70 cases. In 56 cases in which the condition of the child was noted, 42 were born living. *Puncturing the membranes* is the oldest mode, and has been resorted to in 180 cases, and, indeed, doubtless in many of the others not specified. It is beyond all others the easiest, quickest, and most certain means of inducing premature labour, but has been received with much more favour in England than in Germany. By it, however, a far less proportion of children are saved than by the use of the sponge. The fates of 178 are specified, of which 103 were born alive, 12 still-born, and 63 born dead.

As to the *presentation of the child,* it is specified in only 120 cases; and of these 45 were cephalic, 75 non-cephalic presentations. This proportion is, however, delusive; as it is nearly certain that all the cases not specified were natural presentations. Even allowing this, we still find every seventh case a preternatural one. In the 75 cases, the great number of 19 cross-births are noted. In 84 cases the completion of the labour required assistance; in 36 by the forceps, 18 by turning, and 11 by perforation.

The fate of the *child* is recorded in 373 cases, in which 250 were born living, or

recovered from asphyxia, and 123 dead. But in 77 of these cases, the child died from circumstances which could have had no reference to the operation, as faulty position, perforation, &c. Of 192 of the children born living, further reports state that 127 continued to live, and 65 had died—28 in the course of six hours, 6 in twenty-four hours, and the rest at periods varying from a day to a year or more.—*Neue Zeitschrift für Geburtskunde*, vol. xxiii, pp. 161-222, and 371-436.

[We have thought it right to give a few of the results detailed in Dr. Hoffman's long paper, the general conclusion from which would seem to be, that if a discharge of the liquor amnii is the most certain mode of inducing premature labour, other modes save a larger proportion of children. Still, this is uncertain, owing to the paucity of facts adduced, compared to those which have really occurred; for, valuable as is the numerical system, when operating upon large bodies of facts, of the *whole* of which the *entire* particulars are *similarly* recorded, its partial application is of little use, and may even give rise to the most erroneous conclusions.]

Position of the Rectum in New-born Infants.

M. HUGUIER, in commenting upon the difficulty that sometimes exists in passing the canula in the operation for artificial anus, observed that, in the fœtus at full time, the rectum is not found on the left, but on the right side. He has especially observed this in respect to females, in whom the rectum corresponds with the right side of the womb, while at a later period it will be found on the left side.—*Gazette des Hôpitaux*, No. 71.

MATERIA MEDICA AND PHARMACY.

On Gargarisms and Dentifrices. By M. MIALHE.

Gargarisms. There is a chemico-physiological fact relating to the action of these pharmaceutical preparations, which has escaped the notice of practitioners. It is, that whatever may be the chemical composition of these medicaments, their therapeutical effect is always manifested by one of two modes of action, viz. *astringent* or *detersive*. The reason is, that all really active gargarisms have for their base either a chemical compound belonging to the class of bodies, which, uniting with the albuminous portions of the blood and organic tissues, give rise to an insoluble combination—and hence called by me *coagulating* or *plastifying* agents; or they have for their base a chemical compound belonging to the class of bodies, which, uniting with these same living solids and fluids, liquefy instead of coagulating them—and hence called *liquefying* or *deobstruent* agents. That this distinction has not been understood, may be seen from the fact that the active principles of almost all the gargarisms usually employed, such as sulphuric, muriatic, tannic acids, the salts of alum, zinc, mercury, &c. belong to the class of coagulants. With the exception of *borax*, no article properly called liquefying is employed as a gargarism, and even this is used by most physicians as an adstrictive agent. It is true that certain metallic salts, and especially alum, coagulants, or local astringents in weak doses, acquire liquefying and detersive properties in stronger ones; but they offer this capital difference compared with the true liquefying compounds, that, independently of their local action, they exert a marked general one consecutively to their absorption. There can be no doubt that agents possessed of the power of softening the living tissues, and liquefying the fluids impregnating them, may be of great service in the treatment of affections of the buccal and laryngo-pharyngean mucous membranes; and this truth seems to have been empirically felt by the ancients, as is shown by the frequent use they made in such cases of preparations so pre-eminently liquefying as the alkalies. Preferable to these are the neutral salts with alkaline bases, and of such none is so simply liquefying as the *tartrate of potassa and soda*. Employed as in the following formula, it constitutes an excellent detersive for dissi-

pating the turgescence of the mucous membranes to which it is applied:—*Dist. water,* 150 parts; *tartrate of pot. and sod.,* 50 parts; *syrup of gooseberries,* 50 parts.

Dentifrices. These, considered as mere means of cleansing the teeth, are usually classed among the cosmetics; but it is a great mistake, for they tend to maintain the regular condition of the entire dental apparatus, and concur in rendering as complete as possible the important act of mastication. In this point of view, they should be regarded as very useful medicaments, and their chemical composition and mode of employment should receive the attention of the practitioner. By a prudent use of them, we may remove two of the most common causes of premature loss of the teeth. These causes are, the deposit of tartar, the tumefaction of the gums, and the acidity of the saliva. The effect of the accumulation of the tartar is acknowledged by every one, and Dr. Toirac has distinctly shown that a tumefaction of the substance of the gums may expel the teeth from their sockets. The destructive action of the saliva is well shown, whenever it acquires *acid* properties, especially in diabetes. Experience has shown that we may almost always prevent the accumulation of tartar, by the daily use of a tooth-powder sufficiently resisting to exert a suitable degree of friction on the teeth, without being sufficiently hard to injure the enamel. Experience likewise shows, that we relieve the flaccid condition of the alveolar tissue by conjoining with this a tonic, or rather an astringent substance. A mixture of *charcoal* and *cinchona* fulfils these indications well enough, and it is that which practitioners generally recommend. But charcoal has its inconveniences. It is too hard, of a disagreeable colour, and being perfectly insoluble in the saliva, lodges in the interdental spaces, and constitutes so many nuclei of irritation, collecting decomposing animal matter around them. The bark is often stringy, and possesses a disagreeable, bitter taste, conferring no utility upon it. The following formula was devised by M. Mialhe some years since:—

Sugar of milk	1000 grammes.
Lake	10 „
Pure tannin	15 „
Oil of mint	20 drops.
Oil of anise	20 „
Oil of orange flower . . .	10 „

Rub up the lake with the tannin, and add, gradually, first the sugar of milk, previously powdered and passed through a silk sieve having wide meshes, and then the essential oils. A long experience has convinced him of the superior efficacy of this compound, the daily employment of which almost always suffices to maintain the gums and teeth in a healthy condition. In those persons, however, who suffer from excessive relaxation of the gums, it is powerless, and such almost always derive effectual relief from using, besides, the following very astringent preparation: *alcohol,* at 33°, 1000 parts; *true kino,* 100; *rhatany root,* 100; *tr. of tolu, tr. of benzoin,* of each, 2; *oil of mint* and of *canella,* of each, 2; and *oil of anise,* 1 part. Macerate the kino and rhatany in the alcohol for eight days; filter, and add the other articles. A teaspoonful diffused in three or four spoonfuls of tepid water should be used as a gargarism, immediately after employing the powder.

As to the acidity of the saliva, the alkalies prescribed with tooth-powders are only of temporary benefit in removing it, and are nearly useless. The only means of rectifying the condition which produces this description of saliva, is by the internal use of these medicinal substances.—*L'Union Médicale,* No. 140.

On Digitaline. By Dr. HERVIEUX.

MM. Homolle and Quevenne, the discoverers of the active principle of *digitalis purpurea,* have pursued a patient course of investigation as to its properties, and MM. Bouchardat and Sandras have since made numerous experiments upon these; but although they have fully exhibited the physiological and pharmaceutical properties of the substance, clinical observations are yet wanting, and it is to supply

some of these that the author in the present paper details the results of its employment in several instances by M. Rayer.

These observations show that in doses of from 1 to 2, or even 3 milligrammes, the medicine does not produce repugnance by its bitterness, or cause any ill effects, as vertigo, headache, &c. In every case it lessened the *rapidity of the pulse;* the mean difference oscillating between 22 and 36, the maximum being 48, the minimum 12. No immediate effect was, however, produced on the pulse, the greatest observed change occurring generally five or six hours after. After a while it recovered itself again, and it required a week or two's use of the drug, to make a permanent impression on it. It exerted a remarkable effect upon the *regularity* of the pulse; for though in some it caused irregularity, in others it corrected this when it existed, or converted an irregular intermittent type into a regular one. Its effect on the *urine* was constantly observed, this fluid in most cases being augmented one half, in a less number a third or a fifth; and yet in a less number still, quadrupled or quintupled.

The medicine is indicated in disease of the heart and in dropsy; it allays the dyspnœa of phthisis, calms the cough, and procures repose. It is useful in nervous palpitation, and in all accidents resulting from too violent an impulse given to the course of the blood. This preparation is preferable to all others, in the certainty of dose which can be attained. Comparative experiments made by MM. Homolle and Quevenne, show that 4 milligrammes of digitaline are equivalent to about 40 centigrammes of carefully prepared powder of digitalis.—*Archives Générales,* t. xvii, 164-84.

Arsenic in Furunculus and Acne. By Dr. SCHWEICH.

DR. SCHWEICH has prescribed arsenic with great success, in various cases of furunculus that have come under his care for some time past, and has found the cure very durable, and the use of the medicine, during which the ordinary diet may be continued, attended with no inconvenience. He begins with 4 drops of Fowler's solution forenoon and afternoon, until a drachm has been taken, and then gives 5 drops until the second, and 6 drops until the third is attained, and so on. *Acne simplex,* in which the knotty pustular appearance of the eruption gives it the character of a miniature furuncle, and which is often so obstinate, and, when attacking the face of young people, so annoying, yields as readily to the arsenic. The injurious effect of aperients in these affections leads to the supposition that the source of the dyscrasis is a specific irritation of the alimentary canal, which is only augmented by the stimulus of purgatives, especially the saline.—*Casper's Wochenschrift,* No. 6.

Quinine in Insanity. By M. PIORRY.

M. PIORRY has of late prescribed large doses of quinine in certain cases of insanity, with speedy and good effect. They have been especially cases in which the insanity has seemed to have been connected with certain changes of the functions of the organs of sense—especially of hearing; or, in the case of hypochondriasis, with certain abdominal sensations. Periodicity is a characteristic in many of these cases, just as it is in other affections of the nervous system; and hence the utility of the quinine in such.—*Gazette des Hôp.,* No. 86.

Quinine as a Prophylactic in Puerperal Fever. By M. LEUDET.

M. LEUDET cites several cases which occurred during the prevalence of three different epidemics at Rouen, in which quinine, given in doses of five grains three times a day, and commenced with a few hours after delivery, seemed to exert a protective agency. On the third day, this quantity was diminished, and the drug left off about the sixth day. In those epidemics in which the disease commences very speedily after delivery, the quinine should be administered as soon as labour begins.—*L'Union Méd.,* No. 43.

FORENSIC MEDICINE.

On the Artificial Inflation of the Lungs of Newborn Infants, and upon Atelectasis Pulmonum. By DR. EULENBERG.

THE author adverts to the great discrepancy of opinion which prevails among physicians and writers on forensic medicine, as to whether the lungs of a child born dead may be inflated so as to enable them to swim in water. The affirmative has been maintained by Bohn, Teichmeyer, Morgagni, Leiberkühn, and Hunter; while the negative has been upheld by a much larger number, especially in modern times, and is at present the prevailing opinion. Dr. Eulenberg has repeated the experiments upon which these opinions have been based, and, as the result, has found that the effect of inflation much depends upon the period at which it has been commenced. He thus sums up: 1. That the inflation is always of easy accomplishment if undertaken shortly after birth, before rigidity has taken place; the effect being complete in proportion to the duration, force, and early institution of the inflation. 2. Inflation is of difficult accomplishment after rigidity has occurred, and generally cannot then be performed at all by the mere mouth, and only incompletely by means of a tube.

Dr. Eulenberg then passes in review the points of difference which are usually said to distinguish inflated lungs from those filled by inspiration. (1.) *Inflation produces a complete distension of the organ.* This, in fact, depends entirely upon the degree and duration of the inflation, and from its not being delayed until collections of mucus or other causes offer mechanical obstacles to the admission of air. After a pretty strong inflation, the author has still found certain parts of a liver-brown colour, and undistended. (2.) *Absence of crepitating sound on section.* If the lung is distended, this sound is not absent. (3.) *Expulsion of air by pressure.* Air is expelled with as much difficulty in this way, as when the lungs have been filled by respiration. In neither case can their power of swimming be destroyed, but by an amount of squeezing which destroys the elasticity of their texture; a destruction, however, more easily effected in lungs which have been rendered more friable by distension. (4.) *The bloodless colour of the lungs.* The amount of blood contained in lungs which have respired, is too variable to admit of this being taken as a criterion. (5.) The *colour* of the distended lung is a more important sign, as in the author's experiments it was found of a grayish or whitish dirty red, very different from the beautiful red of the lung that has breathed. (6.) More important and characteristic, however, is the condition of the *substance of the lung.* Upon the surface of the inflated lung, just under the pleura, are to be seen small, flat, roundish vesicles, the size of millet-seed, disposed in groups of from four to six, which are again aggregated into clusters as large as peas. They result, in fact, from a *vesicular emphysema.* In the neighbourhood of these are commonly seen other somewhat larger, single, elevated, transparent bodies, and especially at the anterior surface near the edges of the lungs. The stronger the inflation has been, the more numerous are these isolated vesicles, which only occur from rupture of the texture of the lungs, constituting a kind of *traumatic emphysema.*

When inflation induces this emphysema over a great portion of the lung, it cannot fail to be recognised as the cause; but when only some small portion of the lung presents either of these kinds of emphysema, then has it the greatest resemblance to a pathological condition of the lung of newborn children, which has attracted too little attention from juridical practitioners. In children born with *atelectasis pulmonum,* and who have lived for some time, we not infrequently find under the liver-brownish fœtal lung certain emphysematous spots, much resembling those produced by inflation. The following are, however, the chief points of difference. In *atelectasis,* the emphysematous portions project more from the surface, the lung is of a brighter red, richer in blood, closer in texture, its dark colour, too, being little influenced by pressure; the mucous membrane of the trachea and

bronchi is red, and often swollen. Moreover, in emphysema from inflation, air is found in other parts, as in the cellular texture near the thymus, the œsophagus, large intestines, &c. In a child with atelectasis, too, that has lived for some hours, the lungs though in a fœtal condition, will swim, or show a very slight tendency to sink; while in the inflated lung, all parts of the structure which exhibit no distension sink at once to the bottom. In three cases of atelectasis examined by the author, a thick, reddish mucus, more or less frothy, obstructed the bronchial ramifications, and seemed the chief cause of the non-development of the texture of the lung, for a passage of air could only be obtained through this by painful efforts; and when it did penetrate into some portions, its return would be obstructed by the same cause, and by the defective expiratory power of the organ in the child, so that at last the distended air-cells give rise to an emphysema. Mendelssohn considers atelectasis as dependent upon a stasis of the blood in the minuter capillaries of the lungs; but the stasis itself may be regarded as a consequence of the prevention of the necessary changes being operated on the fluid, by reason of the obstructing mucus. Rokitansky regards it as a catarrhal affection of the bronchial membrane, giving rise to reddening, swelling, and to obstruction by mucus. But all these may as well be explained as consequences, seeing that they do not occur until the disease has existed for some time; and if it lasts yet longer, even hepatization and infiltration may result. Whether the accumulation of mucus and thickening of the parenchyma must necessarily take place before the disease is completely formed, the author is unable to say, but generally it is certainly the case; and such impediment of the functions of the lungs by mucus has been long ago observed, especially by Röderer and Ploucquet. In some few cases of atelectasis, merely the traumatic form without the vesicular emphysema is seen, which is never the case in the inflated lung.

After referring to several reputed cases of the production of emphysema by gaseous extrication during fœtal life, the author comes to the conclusion that no credible example of such exists, and that objections on this score against the hydrostatic test are not tenable; and observes—1. That we possess sufficiently characteristic signs, as above described, of a lung having been inflated. 2. That where decomposition of the child's body has gone so far as to affect the lungs, all reliance on tests is at an end; although here, if a portion of not entirely destroyed lung sinks in water, it is all the stronger confirmation of the child not having breathed. 3. An emphysema from disease has never yet been observed in the lungs of a child that has not breathed; and cases of *atelectasis pulmonum*, with the common complication of emphysema, are entirely adapted for the test; by which the fact of respiratory movements having taken place can be proved.

In conclusion, he remarks upon the injurious effect produced by inflation upon the lungs of a newborn infant, and protests against Mendelssohn's recommendation of it (Mechanism der Respir., 1845), as a remedy in atelectasis. He thinks that tartar-emetic is the means most likely to be useful. He considers that artificial respiration should not be resorted to for the purpose of resuscitating infants, as if air is blown with any force into the lungs, even by the mere mouth, an emphysema will always result.—*Medicinische Zeitung*, 1848, Nos. 6, 7, 8.

On the Prolongation of the Period of Gestation. By Dr. M'ILVAIN.

DR. M'ILVAIN relates this case as furnishing, on account of the undoubted veracity of the persons giving the particulars, conclusive testimony of the possibility of the prolongation of the period of gestation. A husband had connexion with his wife on the 1st, 2d, 3d, and 4th of July 1847, and then left her for nine months. She was delivered of a fine, full-grown infant, 23d of April, 1848; so that if impregnation followed the first coition, 296 days, and if the last, 293 days had elapsed, i. e. from sixteen to thirteen beyond the ordinary period. The child was a female, weighing nine pounds,—at least a pound and a half above the average weight of female children.—*Amer. Journ. Med. Sc.*, vol. xvi, p. 247.

On the Production of severe Internal Injuries without External Marks of Violence. By Dr. CASPER.

DR. CASPER has communicated in his 'Wochenschrift' the results of the judicial post-mortems he has had occasion to make, and draws attention more than once to the vast amount of injury that may be done to important internal organs, unaccompanied with any mark of the external violence that has produced it; and justly remarks that such cases should teach practitioners great caution in the delivery of opinions from mere external inspection. Henke only observes that rupture of the spleen *may* occur without any external sugillation; but Dr. Casper has met with ruptures and fractures of various other parts, as the kidneys, liver, heart, lungs, the ribs, vertebræ, and sternum. One truly extraordinary case of this kind he gives in some detail. A dealer in glass was descending a hill with his heavily-laden cart, on a cold winter's night, and in alighting the better to guide the horse, was thrown against a tree by the road side. On the external examination of the body nothing was seen but some slight abrasion of the skin over the zygomatic arch and the arm. Nothing particular was discovered within the cranium; but on opening the spinal canal, a large quantity of dark, fluid blood flowed out, and the spinous process of the first dorsal vertebra was found entirely broken off, lying among the soft parts. The spinal muscles in their whole course were much sugillated, but the medulla was entire. In the left side of the thorax there were about thirty ounces of a bloody fluid, and it was soon perceived that the *heart* was absent from its proper place, and it was found lying *loose* in the back part of the thoracic cavity, *entirely torn away from the large vessels*, the ends of these being plainly seen. The texture of the organ was firm, and its cavities contained dark coagula. The left lung at its middle fissure was almost entirely torn through. The right lobe of the liver was likewise ruptured.—*Casper's Wochenschrift*, 1848, Nos. 1 and 7.

Calcined Magnesia in Poisoning by Arsenic. By Dr. BISSEL.

DR. BISSEL was called to a strong labouring man æt. 27, who had swallowed about a scruple of arsenic between two and three hours before. He procured free vomiting without any relief, the patient manifesting all the signs of arsenical poisoning in an advanced degree. Believing that further attempts at evacuation were useless, he determined to try the effect of calcined magnesia, which has been recently given with success in these cases. He ordered a drachm every hour in milk and water, with the effect of speedily abating the violence of the sufferings of the patient, and of ultimately entirely relieving him. Moderate action of the bowels followed the use of the remedy.—*Amer. Journ. Med. Sc.*, v. xvi, p. 121.

Question of Professional Secrecy.

DR. VIRIELLE, of Rochelle, was consulted by M. B. for a venereal affection, which, without knowing it, he communicated to his wife. She separately consulted the same practitioner. Some time after, the lady being desirous of procuring a separation from her husband, summoned Dr. Virielle as a witness, to testify to the injury she had received at his hands. Dr. Virielle refused to take the oath to speak the truth, as he could not reveal that which his professional obligations entitled him to keep secret; and he did not believe that even the authorization which the plaintiff offered him, as regarded her own case alone, would allow him to do so. The court decided that while the Code forbad professional revelations on the part of a physician made with an intention of doing injury, it did not authorize a refusal when demanded by justice; for this would be to exempt him from obligations which are imposed on all other citizens. The refusal is especially unjustifiable when the consultant herself demanded the revelation, and did not seek for any in respect to her husband.

A fine of sixty francs was imposed; but the medical society of Rochelle have

carried the case, by appeal, to the court of Poitiers, where it seems that in 1828, in a precisely analogous case, judgment was given in favour of the medical attendant. —*L'Union Médicale*, No. 50.

[Notwithstanding the approval which the editors of the French Medical Periodicals bestow upon the conduct of the physician in this case, we believe it quite unjustifiable. Inviolable, indeed, should be professional secrets in all the relations of private life; and no punishment could be too severe for him who, for mischievous purposes, betrayed those of which necessity, misfortune, or accident put him in possession. But, when summoned to the tribunals of his country, the furtherance of the ends of justice becomes the physician's highest duty; and if the inviolability here claimed were accorded, and fully carried out, especially in criminal cases, it might lead, not unfrequently, to the utter frustration of these ends.]

STATISTICS.

Revaccination in the Prussian Army in 1847.

THERE were revaccinated during 1847, 43,596 individuals; of these 34,264 presented distinct scars of a prior vaccination, 6405 indistinct ones, and 2927 none at all. The vaccination ran a regular course in 25,544, an irregular one in 7425, and failed in 10,627. These last being again submitted to vaccination, it succeeded in 2718 of them. Thus, of the 43,596 revaccinations, 28,262, or more than 64 per cent., succeeded. During 1847 only 5 cases of smallpox appeared in the army, and no death from this disease occurred.—*Med. Zeitung*, No. 15.

On Comparative Military Hygiène and Medical Statistics. By M. BOUDIN.

M. BOUDIN, so well known for his elaborate productions in the department of military medical statistics, in the present essay institutes a comparison of the hygienic condition of various armies in ancient and modern times. Its great length, and the tabulated form of many of the facts, prevent our doing more than noticing a few of the results. As on several former occasions, he pays well-merited compliments to our Army and Navy Medical and Registrar-Generals' Reports, which furnish a mass of material upon the subjects they relate to, such as no other country possesses. While recording testimony of this kind, frequently offered by enlightened foreigners, we are only made the more sensibly aware of the disgraceful anomaly, that the registration system, which has been found to work so well in England, and to be productive of such valuable results, has not, after the lapse of so many years, become extended to the other portions of the kingdom.

Exemptions from Service.—Of 4,017,539 men between 20 and 25 years of age, drawn for the *Prussian* army in 1831-40, 626 in every 10,000 were exempted on the ground of physical or intellectual infirmity; 1,203,981 were temporarily exempted; and 2374 in every 10,000 were exempted on the ground of defective stature, i. e. less than 5 feet. Of 5,811,944 examined in *France* in 1816-35, nearly a fifth were exempted on the ground of infirmities or defective stature, although this last was fixed as low as 4 feet 10 inches. The *English* soldier, according to Mr. Marshall, averages from 5ft. 7in. to 5ft. 8in., and M. Boudin furnishes a table exhibiting the number per 1000 of any given height in the English and French armies. This gives an immense superiority to the former; for while, e. g. there are only 4 per 1000 of the British as low as 5ft. 5in., there are 152 of the French; and of the heights of 5ft. 7in., 5ft. 8in., 5ft. 9in., the former furnish 180, 252, and 184 per 1000, and the latter only 69, 49, and 22.

Mortality.—In an average effective force of 105,000 *Prussians* in 1821-30, there was, per 1000 men, a mean annual proportion for the entire army of 11·7 deaths,—a low figure compared with other armies. The Prussian army, is however, composed of young men, compelled only to three years' active service, and

liable to few changes of locality. The civil population of the same age (20 to 25) in Berlin gave 10 deaths to 1000 living. The *British* army in 1819-28 furnished 15 deaths per 1000 in the United Kingdom, 57 in troops serving beyond its limits, and 37 for the entire army. It is a great mistake, however, to estimate the loss from disease by the mere number of deaths, for the discharges from the British army in the above period amounted to 85 per 1000. In an effective force of 337,687 men, the *French* army lost 34·2 per 1000, or deducting the officers, 46·5 per 1000, the portion of the army in Algeria much raising the amount of mortality. During the years 1829-38, the mean annual effective force of 5515 in the *American* army furnished 44 deaths per 1000, the mortality oscillating between 29 and 68, the average being only 18 in the army of the north, and 49 in that of the south. According to Dr. Arendt, after the 20 years of legal service in the *Russian* army, 1 man per 1000 in the infantry, and 3 in the cavalry, would remain, giving an annual loss of 50 per 1000. M. Denis states that from 160 to 180 deaths per 1000 occurred in the Caucasus. It is a curious fact that the *Jews*, who enter the Russian navy in great numbers, furnish a much lower mortality than do the other sailors; and when Prussia entered upon an investigation concerning the endemic prevalence of *plica* in Posen, it was found that the disease attacked the Sclavonic race in the proportion of 29 per 1000, the Germans of 18, and the Jewish only of 11. In his former works M. Boudin has noticed like immunities on the part of the Jews.

The mortality of the British soldiers in the *Colonies* is very various; for while at the Cape it is as low as 14 per 1000, at the Bahamas it reaches 200, and at Sierra Leone 483; and Colonel Tulloch recently informed the author that the 78th regiment lost in Scinde in six months, by disease alone, 680 men out of 960, the 26th and 98th faring no better in China. Taking the mortality within the kingdom at 1, it is 1·3 beyond the tropics, and 4 in intertropical regions. In the French Antilles the mortality has been found least on the calcareous isles, and most on the volcanic, fever predominating in the former, dysentery in the latter. Martinique furnishes a mortality of 110 per 1000, Guadaloupe of 105. Of 4000 men sent to the Antilles, 1154 had died in four years, 417 had been invalided, of whom 334 would probably die, and 406 remained in the corps. As a means of diminishing the mortality, M. Boudin recommends the enlistment of negro troops, the mortality of these having been found to be so much less than that of Europeans in Senegal; the encampment of the white troops on elevations 6 to 800 metres above the sea; and the discontinuance of the practice of selecting the recruits from the south of France, under the erroneous idea that they can best resist tropical climates.

Examining the mortality, according to the *nature of the service*, it is found in Prussia that the infantry furnishes 12·9 deaths per 1000, the cavalry 9, the artillery 10, and the engineers 6·4. So in the British army, 1830-6, the deaths of the Dragoons were 14 per 1000, and of the Foot Guards 21·6; and in 1830-7, the loss in an effective force of 1000 men by death and discharges was 41·6 Dragoons, 32·5 Horse Guards, and 58·0 Foot Guards. This is no new observation; for Marozzo states that in 1775-92, the Piedmontese army lost 18 per 1000 cavalry, 34 foot. These facts demonstrate the error of sending the feeblest recruits to the infantry.

M. Boudin observes that the condition of the British *navy* demonstrates the irresistible power of hygiene. The mortality for the period 1830-6, was 13·8 per 1000, or from internal diseases alone only 11·8; while seventy years since it was 123, and less than forty years ago 30. The official documents attribute this improvement to better diet, free ventilation, less severity of punishment, diminution of the rations of spirit, and the leaving less money at the men's disposal.

Deaths from Phthisis.—The amount of deaths from this disease was 3 per 1000, a slight proportion compared with that of the British army; but the amount of deaths does not indicate the full extent of its prevalence in an army, many soldiers discharged afterwards dying of phthisis. The Dragoons, Horse Guards, and Foot Guards in Britain furnished respectively 7·7, 8·1, and 14·1 deaths per 1000; the same disease causing from 2·5 to 6·9 per 1000 at the different Mediterranean stations. However difficult of exact numeration, in all armies the deaths from this disease

are enormous, and are due to defective ventilation, defects in the quantity, quality, or variety of food, *tedium vitæ*, disturbed rest, and careless recruiting. What has been done for *horses*, by improving their food and ventilation, shows what may be effected by the removal of some of these causes. Thus during 1830-6, the annual mortality of horses in the French cavalry was 197 per 1000; but during 1841-6 the number progressively declined from 126 to 68.

Revaccination.—During 1845, there were 42,671 soldiers of the Prussian army vaccinated, in 33,813 of whom there were traces of prior vaccination, in 6041 imperfect traces, and in 2817 no traces whatever. The results were, regular vesicles in 22,214, irregular in 8764, and *nil* in 11,693. During 1845 only one case of small-pox manifested itself throughout the army.

Suicides.—During 1829-38, the proportion of these in the Prussian army was 1 in 1985, which, although much greater than that prevailing in the French army, is far less than that of the British, in which, of 686 deaths occurring among the dragoons in 7 years, 35 arose from suicide. In the Prussian army the proportion was 2 per 10,000 in the artillery and engineers, 4 in the infantry, and 7 in the cavalry. In the British *navy* suicides are very rare, 4 only having occurred among 157,770 men in 7 years, while during the same period there were 59 among 44,611 dragoons.

General Observations.—The proportion which the expense of the army bears to the revenue in the different states is 20 per 100 for England, 22 for France, 32 for Austria, 36 for Russia, 40 for Sweden, and 42 for Prussia. The maintenance of a foot-soldier, however, costs 538 francs in England, 340 in France, 240 in Prussia, 212 in Austria, and 120 in Russia. An effective force of 100,000 men would be found to be reduced by death in France and Algeria, independently of the colonies, to 97,125, in England and colonies to 96,300, in America to 95,600. The mean daily number of patients in hospital is in France 45 per 1000, Prussia 44, picked British troops 37·3, the army in Ireland 51. So that 100,000 soldiers would be reduced in France to 95,550 combatants, in Prussia to 95,600, in England to 96,270, and in Ireland to 94,900.

Conclusions.—The facts brought forward in this paper authorise us to conclude.—1. That there is an intimate connexion between the hygienic institutions of an army, and the amount of reduction in it by disease, discharge, and death. 2. The losses of all armies, even in the time of peace, exceed those of the civil population of the same age. 3. In Europe and North America, bounded on the south by the isothermal line of 59°, phthisis and typhoid fever are the chief causes of death; while beyond this curve, and at the level of the sea, the great losses of Europeans arise from paludian disease, dysenteries, and diseases of the liver. 4. Everything leads us to believe that good hygienic institutions would lower the mortality not only to 10 per 1000, that of the English civil population between 20 and 30, but even to 6 per 1000, that of the Prussian engineers. What these hygienic measures should be, will furnish the material for another essay to explain.—*Recueil de Mémoires de Médecine Militaire*, vol. 64, pp. 1-140.

On the Influence of Cholera on the Proportions of the Sexes at Birth.
By DR. EMERSON.

DR. EMERSON while engaged in compiling tables illustrative of the vital statistics of Philadelphia for 1830-40, found the excess of male births, which was usually some hundreds, and had been the year before 415, sink down to 38 in 1833. Examining the subject more closely, he found that the female births during April and May (590) exceeded the male (532) by 58, i.e., 10 per cent.; and as the ordinary male excess had been 7 per cent., this made a diminution of 17 per cent. Now these two months include the period nine months after the *cholera* had so severely prevailed, viz. August and September, 1832. The amount of conceptions six months subsequent to its appearance exhibited a preponderance of females, viz. 1851 to 1826 males.

A corroboration of this view is found, Dr. Emerson states, in examining the returns from *Paris*, curiously modified by the fact of illegitimacy. Taking the whole

year 1832 through, the *legitimate male* births exceeded the female by 6½ per cent., the *illegitimate* by only 3¾; but if we compute only the births for December, i. e., nine months after the most fatal ravages of the cholera, we find the excess of males replaced by that of females.

The births for 1833 were—

	Males.		Females.
Legitimate births	11,852	. .	11,434
Illegitimate births	5,039	. .	5,042
	16,891		16,476

The births for December were—

	Males.		Females.
Legitimate births .	645	. .	679
Illegitimate births .	200	. .	199
	845		878

It was among the lower classes, which furnish the great bulk of illegitimate births, that the disease especially prevailed. Examining the city by *arrondissements*, it is found that the male legitimate births in 1833, which took place in the parts of Paris nearly exempted from cholera, reached the high preponderance of 7½ to 7¾ per cent., and even the illegitimate ones 6 per cent. But in those parts where it prevailed most, the excess of male legitimate births sank to 3½ per cent.; while among the illegitimate it was converted into a female preponderance. It has been found in all countries that a smaller number of *males* are produced in *illegitimate* than at legitimate births. Observations made at Paris during a long series of years show that the proportion of legitimate males to females is 23 to 22, that of illegitimate 16 to 15.

Dr. Emerson only adduces this deduction in relation to the influence of the cholera, as illustrative of the general remark, that whatever exerts, directly or indirectly, morally or physically, a depressing effect upon a community, is followed by a conspicuous reduction in the proportion of male births. After the subsidence of an epidemic or pestilence that has carried off the feebler portion of the population, an *increase* in the amount of males will soon exhibit itself, the parents being endowed with more than the average amount of vital energies, as seen in their exemption or recovery from disease.

"We think there is strong reason to believe that the institution of polygamy grew out of a preponderance in the amount of female population, induced, perhaps, by a scanty supply of food, or from the use of a description of diet not calculated to maintain a high condition of physical energy. Polygamy once established as a common usage must tend to foster itself.

"In conclusion, it appears to us that the proportions of the sexes at birth are, to a considerable extent, subjected to circumstances more or less under human control; and that all measures tending to lessen disease, and to promote the welfare and comfort of a population, while they serve immediately to increase the capacities for profitable labour, tend also to promote the multiplication of the sex supplying the main physical power."—*American Journ. Med. Science,* vol. xvi, pp. 78-85.

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Report of the Medical Cases treated in the Liverpool Northern Hospital during 1846 and 1847. By James Turnbull, M.D. 8vo, pp. 63.

A Few Thoughts on Cholera, in reference to its Origin, the Nature of the Exciting Cause, and the Principle of Treatment. By Michael Thomas Sadler, M.R.C.S. London, 1848. 8vo, pp. 52.

Contributions to the Pathology of the Kidney. By William T. Gairdner, M.D. Edinburgh, 1848. 8vo, pp. 54.

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Report of the Medical Officers of the Lunatic Asylum for the County of Lancaster, for the Year 1847. Lancaster, 1848. 8vo, pp. 22.

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A Course of Lectures on Dental Physiology. By John Tomes, Surgeon-Dentist, &c. With numerous Illustrations. London, 1848. 8vo, pp. 398.

The Clinical Repertory, containing Tabular Forms for the Collection, Arrangement, Comparison, and Analysis of Pathological and Physiological Phenomena. London, 1848. Folio.

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1. The Principles and Practice of Midwifery. By David H. Tucker, M.D. Philadelphia, 1848. Small 8vo, pp. 406.

2. Elements of General Pathology: a Practical Treatise on the Causes, Forms, Symptoms, and Results of Disease. By Alfred Stillé, M.D. Philadelphia, 1848. Small 8vo, pp. 484.

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